reasonable to presume that these marks are records, not merely ornamental lines, and if records, of children born? Such a carved stone, once proudly worn by an Indian of high rank, if broken, as this has been, would naturally be preserved; and that it is but the half of such an one, as seen in Fig 1, is proved by the fact of a hole being drilled in the lower corners, as shown by the dotted lines; a hole that became of no use when the specimen was broken, or at least was less well placed than that subsequently drilled in order to suspend the relic as an ornament, as an ear-ring, or addition to a necklace, as previously suggested.

The traces, as they really are now, of the graves of our aborigines occasionally contain a single specimen of the above-figured relic. So far as I have been able to examine these graves, such relics are never associated with the stone axes and spear-heads characterising the graves of adult males, but simply with other forms of stone ornaments, and a single small mortar and pestle, or earthenware vase. In one instance the "brooding bird" was so placed with reference to the narrow strip of discoloured earth that marked where the body had been laid, as to show conclusively that the relic was attached to the hair, as shown in Fig. 1.

If we examine a series of these relics, it will be at once seen that every one has holes drilled at the lower corners. Such specimens could only be worn upon the top of the head, without being upside down, as would necessarily be the case had they been suspended. It must, too, be borne in mind that these relics are nowhere very abundant, but on the other hand, nowhere unknown north of Mexico. Had they been knife-handles, as suggested by Schoolcraft, or corn-huskers, as suggested by various writers, certainly they would be much more abundant than they really are. Indeed, in considering them as ornaments for married women, I am forced, in consideration of the scanty number that have been collected, to restrict them to women prominent in their tribes, the wives of kings, chiefs, and eminent warriors. If this be true, then the eight birth-records on Fig. 2 are those of "Indian princes," it may be. I must admit, however, that this broken specimen is the only one that I have seen having like marks cut upon it; but such record marks, as I believe them to be, are quite common upon other forms of stone ornaments, particularly those stone tablets and crescents that I have elsewhere (Smithson. Ann. Rep. for 1874) called "breast-plates."

These facts considered, I think that the suggestion of Mr. Gillman, based upon information received from an aged Indian, truly explains what this much-discussed relic truly is—an ornament for married women, an emblem of maternity.

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THE BRITISH ASSOCIATION

REPORTS.

Report of the Committee on Luminous Meteors, by Mr. James Glaisher.—The report related, as usual, to meteors doubly observed, and to aërolites, the portion having reference to the latter being the more interesting, as the falls of aërolites which have been placed on record since the last report were more than ordinarily numerous and interesting. A mass of meteoric iron fell on Aug. 24, 1873, at Maysville, California, and is one of the very few metallic irons the actual descent of which has been witnessed. In the following month a number of meteorites fell near Khairpur, in the Punjaub; and it is also related that in the month of December, when the British army halted on the banks of the Prah, an aërolite fell in the market-place of Coomassie, and was regarded by the native population as a portent of evil. On the 14th and 20th of May, 1874, aërolites fell at Castalia, in North Carolina. The last stone-fall of the past year took place near Iowa city on the 12th of February, 1875, and of this meteorite also special analyses were made in the United States, of which some unforeseen results were lately announced by their author, Mr. A. W. Wright. In England no detonating meteor has been observed this year; and the brightest meteor recorded since the last report occurred on the 1st of September last, taking its course over the north of England, or Scotland, where clouded skies must have prevailed, as its flash was like that of lightning. Other bright meteors occurred on the 2nd and 16th of September, 11th of October, 17th of December, 9th of March, 12th of April, and 2nd and 4th of May in this year. A meteor burst with a loud detonation over Paris and its neighbourhood on the 10th of February; it was of great size and brilliancy, and left a cloud-like streak of light on its track for more than half an hour. No duplicate observation of it was obtained in England. Another fireball fell at Orleans on the 9th of March, and of this two good observations appeared to have been obtained in England, which may assist to determine its real height. During the annual meteor showers of the past year very unfavourable weather generally prevailed for recording meteor tracks, and few meteors were seen on those nights when the usual expectations of their appearance were entertained. A thorough examination of all the observations collected by the committee since the publication of the Meteor Atlas in 1867, with the view of extending and correcting the list of general and occasional meteoric showers which it embraced, has been continued with satisfactory results under the direction of Mr. Greg. The report also contained a *résumé* of the contents of the recent publications on the subject of meteoric astronomy. Mr. Glaisher remarked that the report was the result of considerable labour performed by Prof. A. S. Herschel, but he pointed out that the work of properly treating meteor observations had now become so great as to be beyond the power of the Association to grapple with, and alluded with satisfaction to the arrangements being carried out by M. Leverrier. A discussion took place on the connection of comets and meteors, in the course of which Sir William Thomson said that there was nothing to justify the assertion that the mass of comets was so small as was sometimes supposed, and he considered there was good evidence for believing that the comet's tail was really a train of meteors.

The Report of the Committee on British Rainfall, by Mr. G. J. Symons, began by giving an epitome of the rainfall work done in connection with the British Association during the last fourteen years. It then referred to the steps taken after the meeting at Belfast to obtain additional stations in Ireland, which were so successful that the committee received 190 offers of assistance. The acceptance of all these offers would have involved an expenditure far beyond the funds at the disposal of the committee, and they were therefore reluctantly compelled to make a careful selection, resulting, however, in the establishment of sixty-six stations, many of them in localities of extreme importance. In the past fifteen years the number of stations had been raised from 241 to nearly 2,000. The influence of size and shape on the indications of rain gauges had been experimentally examined, and also the effect of height above ground. The laws which regulate the seasonal distribution of rainfall had been to a certain extent ascertained. The secular variation of annual fall had been approximately determined. A code of rules had been drawn up for observers. Nearly 250 stations have been started at the cost of the Association, and 629 stations have been visited, and the gauges examined by the secretary. They had obtained and supported observations on mountain tops, and places difficult of access where no observations had been made, in Cumberland, Westmoreland, Wales, and Scotland, and also an extensive series in Ireland. When the works actually in hand are completed, they will furnish an index to all the observations hitherto made.

The committee appointed to examine and report upon the reflective powers of silver, gold, platinum, and speculum metal did not present any report, but was reappointed at its own request, with the addition of Prof. Ball.

Owing to the absence of Col. Babbage in India, the committee for estimating the cost of Mr. Babbage's analytical engine had not met, but it requested to be reappointed. No report was received from the committee for the determination of the mechanical equivalent of heat, but it was stated that Prof. Joule's experiments were making good progress. The committee on teaching physics in schools was reappointed. Also the committee for considering the possibility of improving the methods of instruction in elementary geometry was reappointed, with the addition of Prof. Henrici and Mr. J. W. L. Glaisher, and requested to consider the syllabus of the Association for the improvement of geometrical teaching, and to report thereon.

Mr. W. C. Roberts read a note from the committee which had been appointed to investigate *the methods of making gold assays and stating the results*. It stated that the standard gold plate had now been finished, and that portions of it had been forwarded to different mints for the purpose of being assayed. The reports read were very satisfactory, as was shown by the fact of M. Stas, of Brussels obtaining 999·95 parts of pure gold out of 1,000 as the result of an analysis. The same plate had also been examined by Mr. Lockyer by means of the spectroscope, and the lines having been compared with the solar lines, it had been shown that silver, copper, and iron were absent, and that therefore the purification of the metal had been very great.

Mr. A. H. Allen read the *Report of the committee appointed for the purpose of examining and reporting upon the methods employed in the estimation of potash and phosphoric acid in commercial products, and on the mode of stating the results*, in which he stated the object of this committee was to examine all the known methods of analysis of manures and potassium salts. They had hoped to be able now to present to the Section some practical and easy process as a neutral standard of reference by which the present discrepancies might be avoided. The plan adopted by the committee was to draw up a printed list of queries which were sent round to all the members of the Chemical Society, with the request that they would send back answers; this plan had been found to work well with very few exceptions, who declined to give up the processes which they alone employed. The report ended by the committee desiring to be re-appointed, and expressing a confident expectation that by the end of another year some really good results would be obtained.—The President remarked, at the conclusion, that the estimation of potash seemed to present much less difficulty than that of phosphoric acid.

Second Report of a Committee, consisting of Prof. A. S. Herschel and G. A. Lebour, on Experiments to determine the Thermal Conductivities of certain Rocks, showing especially the Geological Aspects of the Investigation.—The experiments during the past year were directed chiefly to a re-examination, with improved apparatus (fully described in the report), of the rocks observed last year. With the exception of Kenton sandstone, which was misplaced in the last table, all the rocks have, under the new mode of treatment, kept the same *relative* positions, and the absolute conductivities given in the present report are believed to leave little or nothing to be desired on the score of accuracy. Quartz has been added to the list, and proves to have less resistance to the passage of heat than any of the other substances examined. Slate has been tried both in the line of cleavage and across it, showing *less* resistance in the latter position than in the former. Some rocks have been experimented on wet as well as dry, the addition of the water giving an increased conductivity of a tolerably constant value. It is intended to continue the experiments in the direction foreshadowed by these results. A full table of absolute conductivities and resistances, with the results of both series of experiments compared, forms part of the report. Coal still maintains its position with the greatest resistance yet found.

SECTIONAL PROCEEDINGS

SECTION A—Mathematics and Physics

Captain Abney read a paper *On the Increase of Actinism due to difference of Motive Power in the Electric Light*, in which he stated that having been called upon by the War Office to undertake the photometric measurements of certain magneto-electric lights, he had determined to carry out actinic measurements of their value at the same time, believing that the eye observations would be closely checked by such an independent method. In the first comparison of the results obtained by both kinds of measurement, a considerable discrepancy was found to occur in the values given to the different lights. The photographic records could not err except through gross carelessness in the chemical preparations, and against this every precaution had been taken. At first it seemed likely that the eye observations were in fault, but a more critical examination convinced Captain Abney that both were correct; and that though the curves obtained for the values of the lights did not coincide, yet that they did act as a check, the one on the other. In all there were six different machines to examine, each of which was driven by a ten-horse power engine. Several were driven at varying speeds that the difference in the light caused by the variation might be tested.

The eye observations were made by a little instrument called by Captain Abney the Diaphanometer, and described in the *Monthly Notices* of the Astronomical Society for last June. The method adopted for registering the actinic power of the light was by exposing uniformly sensitive chloride of silver paper to the action of its rays. Two registrations were carried out with each light: first, paper was exposed to the naked light at a fixed distance from the carbon points for three minutes; and secondly, a strip of the same paper was exposed beneath black wedges of slight taper for sixteen minutes. The eye observations were carried on simultaneously with the latter exposure of the sensitive paper, in both cases obtaining an integration, as it were, of the light during that period. Between ten and twenty observations were taken for each light at the beginning, middle, and end of each trial. Diagrams of the steam pressure were taken in the usual manner, and diagrams were also taken of the steam pressure when driving the machine without exciting a current, at the same speed as that at which the light was produced. They were also taken in many cases when the machines were what may be called short circuited. The data were thus obtained for calculating the power necessary to produce a light of a certain value.

Diagrams were exhibited showing the mean of the results of a series of experiments with one instrument; one curve, deduced from eighty readings, giving what may be called the optical value; another, deduced from 450 readings, giving the actinic value; whilst a third showed the ratio of the actinic to the optic value—the abscissæ being in all these cases measures of the horse power. The curves are interesting as showing the rapid decrease of the optical value, and still more of the actinic value, of the light when worked with a low motive power. They also show that each machine has a point beyond which the increase in motive power is not compensated for by increase in light, the curves apparently becoming asymptotic.

Captain Abney stated that he was not at all prepared for the great diminution of the value of actinic power in the lights, though he expected it in a smaller degree. The early experiments of Draper and others had shown that with increase of temperature the more refrangible portions of the spectrum appear after the least refrangible, but there seemed to be no measurements which would have been applicable to the present set of experiments. The curves must evidently be some function of the wave-lengths, and the author hoped to carry out other experiments in fixed portions of the spectrum in order to ascertain if the formula which he thought should hold good could be employed.

SECTION B.

Chemical Science.

Opening Address by A. G. Vernon Harcourt, M.A., F.R.S., F.C.S., President.

To the privilege of presiding over this Section custom has added the duty of offering some preliminary remarks upon the branch of science for whose advancement we are met.

In discharge of this duty some of my predecessors have reviewed the progress of chemistry during the previous year; and until a few years ago there was no more needful service that your President could render, though the task of selection and abstraction was one of ever-increasing difficulty. But a few years ago the wisdom and energy of Dr. Williamson transformed the Journal of the Chemical Society into a complete record of chemical research, and this Association materially promoted the advancement of science when it helped the Chemical Society in an undertaking which seemed at one time hopelessly beyond its means. The excellent abstracts contributed to the Journal err, if at all, on the side of brevity, and yet the yearly volume seems to defy the bookbinder's press. I shall not venture to attempt further abstraction, nor to put before you in any way so vast and miscellaneous an aggregate of facts as the yearly increment of chemistry has become. The advancement of our science—to borrow again the well-chosen language of the founders of this Association—is of two kinds. The first consists in the discovery and co-ordination of new facts; the second in the diffusion of existing knowledge and the creation of an interest in the objects and methods and results of scientific research. For the advance of science is not to be measured only by the annual growth of a scientific library, but by the living interest it excites and the number and ardour of its votaries. The remarks I have to offer you relate to the advancement of chemistry in both aspects.

One fact has been brought into unpleasant prominence by the Journal of the Chemical Society in its present form, namely, the small proportion of original work in chemistry which is done in

Great Britain. All who are ambitious that our country should bear a prominent part in contributing to the common stock of knowledge, and all who know the effect upon individual character and happiness of the habit and occupation of scientific inquiry, must regret our backwardness in this respect. The immediate cause is easily found. It is not that English workers are less inventive or industrious than their fellows across the Channel, but that their number is exceedingly small. How comes it that in a country which abounds in rich and leisurely men and women —for neither the reason of the case, nor the jealousy of the dominant sex, nor partial legislation excludes women from sharing this pursuit with men—there are so few who seek the excitement and delights of chemical inquiry? Moralists tell us that the reason why some men are content with the pleasures of eating and drinking and the like is, that they have never had experience of the greater pleasure which the exercise of the intelligence affords. I am not about to represent it as the moral duty of those who have means and leisure to cultivate chemistry or any branch of science; but no taste for a pursuit can be developed in the absence of any knowledge of its nature. A taste for chemistry is often spoken of as a peculiar bias with which certain men are born. No doubt there are differences in natural aptitudes and tastes, but the chief reason why it is so rare for men of leisure to addict themselves to scientific pursuits is, that so few boys and young men have had experience of the pleasure which they bring. Much has been done during the last twenty years, both at the Universities and at the Public Schools, to provide for the teaching of science. To speak of what I know best, the University of Oxford has made liberal provision for the teaching of science, and for its recognition among the studies requisite for a degree; nor have the several colleges been backward in allotting scholarships and fellowships as soon as and whenever they had reason to believe that those elected for proficiency in science would be men equal in intellectual calibre to those elected for proficiency in classics or mathematics. But the result is somewhat disappointing, and under a free-trade system science has failed to attract more than a small percentage of University students. Excellent lectures are delivered by the professors to scanty audiences, and the great bulk of those educated at the University receive no more tincture of science than their predecessors did twenty years ago.

The recognition of science among the subjects of University examinations is by no means an unmixed advantage to those concerned. Examinations have played and will continue to play a useful part in directing and stimulating study, and in securing the distribution of rewards according to merit; but they produce in the student, as has often been pointed out, a habit of looking to success in examination as the end of his studies. This habit of mind is peculiarly alien to the true spirit of scientific work. Only such knowledge is valued as is likely to be producible at the appointed time. Whether a theory is consistent or true is immaterial, provided it is *probable*, that is to say, advanced by some author whose authority an examiner would recognise. All incidental observations and experimental inquiry lying outside the regular laboratory course, which are the natural beginnings of original work, must be eschewed as trespassing on the time needed for preparation. The examination comes; the University career is at an end; and the student departs, perhaps with a considerable knowledge of scientific facts and theories, but without having experienced the pleasure, still so easily gained in our young science of chemistry, of adding one new fact to the pile of knowledge, and, it may be, with little more inclination to engage in such pursuit than have most of his contemporaries to continue the study of Aristotle or Livy.

However, examinations have their strong side, to which I have referred, as well as their weak side; and although it is the natural desire of a teacher to see his more promising pupils contributing to the science with whose principles and methods they have laboured to become acquainted, the younger, like the elder branches of knowledge, must be content to serve as instruments for developing men's minds. Chemistry can only claim a place in general education if its study serves, not to make men chemists, but to help in making them intelligent and well-informed. If it is found to serve this purpose well, the number of chemical students at the Universities ought to increase; and if the number increases, no rigour of the examination system will prevent one or two, perhaps, in every year adopting chemistry as the pursuit of their lives. But the Universities have little power to determine what number of students shall follow any particular line of study. With certain reserves in favour of classics and mathematics, their system is that of free-trade. Young men of eighteen or nineteen have tastes already formed, some for the studies which were put before them at school, in which, perhaps, they are already proficient and have been already successful, some for games and good fellowship. It is, from the nature of the case, with the masters of schools that the responsibility rests of fixing the position of science in education. During the last ten years provision has been made at most of the larger schools for the teaching of some branches of science; and those who recall the conservatism of schoolboys, and their consequent prejudice in favour of the older studies, will understand a part of the difficulties which have had to be encountered. The main and insurmountable difficulty is what I may call the impenetrability of studies. A new subject cannot be brought in without displacing in part those to which the school-hours have been allotted. It is the same difficulty which occurs again and again in human life. There are so many things which it would be well to know and well to follow; but life, like school-time, is too short for all. From the educational phase of this difficulty the natural difference of tastes and aptitudes provides in some degree a way of escape. I think that wherever a school can afford appliances for the teaching of chemistry, all the boys should pass through the hands of the teacher of this subject. Two or three hours a week during one school-year would be sufficient to enable the teacher to judge what pupils were most promising. There may be instances to the contrary, but I no not think it likely that any boy who attended chemical lectures for a year without becoming interested in the subject would ever pursue it afterwards with success. Suppose that out of one hundred boys who have gone through this course, five are selected as having shown more intelligence or interest than the rest; they should be permitted to give a considerable part of their time, while still at school, to studying science without suffering loss of position in the school, or forfeiting the chance of scholarships or prizes. If any such system is possible and were generally adopted, each school sending annually to the Universities, or other institutions for the education of young men, its small contribution of scientific students, the professor's lecture-rooms and laboratories would be filled with young men who had already learnt the rudiments of science. Laboratories of research as well as of elementary instruction would find a place at the English Universities, and the reproach of barrenness would be rolled away.

Some of the defects or difficulties to which I have adverted are perhaps peculiar to our older schools and universities. The introduction of the study of natural science has borne earlier fruit in schools whose celebrity is of more recent date, such as the excellent college in this neighbourhood. Oxford and Cambridge ought to possess, but are far from possessing, such laboratories as have lately been built at the Owens College, Manchester. It is proposed to constitute in this city a College of Science and Literature, similar to Owens College and in connection with two of the Oxford colleges. The scheme set forth by its promoters appears thoroughly wise and well-considered, and all who are interested in scientific education must wish it success.

I have placed first among the modes in which science, and in particular chemical science, may be advanced, the assignment to it of a more prominent and honoured place in education; but owing, as I do, my own scientific calling and opportunities of work to a bequest made to Christ Church by Dr. Matthew Lee more than a hundred years ago, I cannot forget or disbelieve in the influence of endowments.

I have spoken of the leisurely class in this country as that to which scientific chemistry must look for its votaries. In our social conditions and in the absence of endowments it is hard to see where else they can be found. Men who have their livelihood to make cannot afford to spend money, and still less to bestow their time and energy, on the luxury of scientific inquiry. Even if they have the opportunity of earning their livelihood by scientific teaching, and with it the command of laboratory and apparatus, no leisure may remain to them for original work, and the impulse to such work (often, it must be admitted, of a feeble constitution) is starved in the midst of plenty. The application of endowments to the promotion of original research is a difficult question. I am inclined to think that posts, constituted chiefly with this object, should be attached in every case to some educational body, and should have light educational duties assigned to them. The multiplication of such posts in connection with the many colleges and schools in this country, where there is some small demand for chemical teaching, with the provision in each case of a sufficient laboratory and means of work, would probably do more than any centralised scheme for the promotion of chemical research.

To the advancement of chemistry by the formation of public opinion on the questions of scientific education and the endowment of original research, the Chemical Section of the British Association may reasonably hope to contribute. But doubts have been expressed as to the serviceableness of this or any such organisation for the direct advancement of our science itself. No doubt we cannot accomplish much. Chemical inquirers at the present time may be compared to a party of children picking wild flowers in a large field: at first all were near together, but as they advanced they separated, till now they are widely scattered, singly, or in groups, each busy upon some little spot, while for every flower that is gathered ten thousand others remain untouched.

That the science of chemistry would advance more rapidly if it were possible to organise chemists into working parties, having each a definite region to explore, cannot, I think, be doubted. Is such organisation in any degree possible?

The experiments of which Bacon has left a record, though curious historically, have no scientific value. But in one respect his "Physiological Remains" furnish an example which we might follow with profit. "Furthermore," he writes, "we propose *wishes* of such things as are hitherto only desired and not had, together with those things which border on them, for the exciting the industry of man's mind." I will quote further, as an example, a part of one of his "wishes," which has very recently been fulfilled. "Upon glass four things would be put in proof. The first, means to make the glass more crystalline. The second, to make it more strong for falls and for fire, though it come not to the degree to be malleable."

I do not know that the industry of M. de la Bastie's mind was excited by Bacon's mention of glass more strong for falls and for fire among things hitherto only desired and not had; but the conception of such an enumeration seems to me worthy of its author. Much fruitless and discouraging labour might be saved, a stimulus might be given to experimental inquiry, and chemical research might become more systematic and thus more productive, if Bacon's example were followed by the leaders of chemistry at the present day.

The Council of the Pharmaceutical Conference, whose meeting has just preceded our own, has published a list of subjects for research which they commend to the attention of chemists. Where one of these subjects has been undertaken by any chemist his name is appended to it. Might not the representatives of scientific chemistry issue a similar list?

Perhaps two or three of the distinguished English chemists who are members of this Association might be willing to serve on a committee which should put itself into communication with the leaders of chemical inquiry abroad, and should make and obtain and publish suggestions of subjects for research. Such a list so got together would, I think, find a welcome place in all scientific journals, and would thus be widely known and easily accessible to every student.

That which chiefly makes the organisation of chemical inquiry desirable is the boundless extent of the field upon which we have entered. Not every fact, however laboriously attained and rigorously proved, is an important fact, in chemistry any more than in other branches of knowledge. Our aim is to discover the laws which govern the transformations of matter; and we are occupied in amassing a vast collection of receipts for the preparation of different substances, and facts as to their composition and properties, which may be of no more service to the generalisations of the science, whenever our Newton arises, than were, I conceive, the bulk of the stars to the conception of gravitation.

It may, however, be urged that the growth of chemical theory keeps pace with the accumulation of chemical facts. It is so, if the elaboration of constitutional formulæ is leading us up to such a theory. But at present, however useful and ingenious this mode of summarising chemical facts may be, it does not amount to a theory of chemistry.

Two objections to regarding such formulæ as anything more than a chemical short-hand, as it has been termed, seem worth recalling. The first is mentioned at the outset in most text-books in which these formulæ are employed, but sometimes, I venture to think, lost sight of afterwards. The arrangement of the atoms of a molecule in one plane is equally convenient in diagrams, and improbable as a natural fact. But is not this arrangement used as though it were a natural fact when the possible number of isomeric bodies is inferred from the number of different groupings of the atoms which can be effected on a plane surface? The conceptions of plane geometry are much simpler than those of solid geometry (which is another recommendation of the present system of formulæ); but so far as I am able to follow the similar theories which have recently been propounded independently by MM. Le Bel and van't Hoff, the consideration of the possible isomerisms of *solid* molecules leads to new conclusions.* Wislicenus has found that paralactic acid undergoes the same transformations as ordinary lactic acid when heated and when oxidised. The two acids differ in their action on polarised light. His conclusion is that paralactic acid does not differ in its atomic structure from the lactic acid of fermentation, and that the kind of isomerism which exists between the two acids is not connected with the difference in the reciprocal arrangement of the atoms, but rather with a difference in the geometric structure of the molecule. To this difference he gives the name of "geometric isomerism."† The authors named above agree in supposing that the action of substances in solution on polarised light results from an unsymmetrical arrangement of atoms and radicles in three dimensions around a nucleus-atom of carbon.

The second objection relates to the statical character of the account which "developed" formulæ give of the differences between different kinds of matter. The modern theory of heat supposes, not only that the molecules which constitute any portion of matter are in constant rapid motion, but that the atoms which constitute each molecule are similarly moving to and fro. Such movement might be an oscillation about the position assigned to the several atoms in the constitutional formula of the molecule. Since, however, the modes of formation and decomposition of substances are the principal facts upon which the formulæ are based, it is to be considered whether these facts may not depend altogether upon the nature or average nature of the motion impressed upon the atoms—that is, upon dynamical and not upon statical differences.

Many substances are known whose existence is contrary to the theory of valency and saturation, such as nitric oxide and carbonic oxide; others, which transgress the theory of isomerism, such as chloride of dichlordibromethane ($C^2 Cl^2 Br^2, Cl^2$) and bromide of tetrachlorethane ($C^2 Cl^4, Br^2$), which should be identical, but are isomeric:‡ yet these theories are simply an expression of the statement that certain substances can exist or can differ, while others cannot. It is true that in the vast majority of cases the theoretical limitation seems to hold good. But just as the absence of any fossil remains of the connecting links between species is only significant if the geologic search has been sufficiently thorough, so it is with chemical theories depending upon the non-existence of certain classes of bodies. Indeed, in our case, where investigation is guided by theory, and, as a rule, only those things which are looked for are found, the limitation may be partly of our own making. A chemist who should depart from the general course, and set himself to prepare substances whose existence is not indicated by theory, would perhaps obtain results of more than the usual interest.

Among chemical inquiries, if ever such a list as I have ventured to suggest should be drawn out, I hope that many would be included relating to the most familiar substances and the simplest cases of chemical change. The thorough study of a few reactions might perhaps bring in more knowledge of the laws of chemistry than the preparation of many new substances.

I believe that if any chemist not content with a process giving a good yield of some product examines minutely the nature of the reaction, observing its course as well as its final result, he will find much more for study than the chemical equation represents. He will probably also find that the reaction and its conditions are of a formidable complexity, and will be driven back towards the beginnings of chemistry for cases sufficiently simple for profitable study.

In concluding my remarks, I desire briefly to refer to another branch of chemical science, to the advancement of which this Association seeks to contribute, I mean applied or technical chemistry. One of the principal differences between the papers read before this Section, as a class, and those which the Chemical Society receives, is the larger proportion in our list of papers on technical subjects. Whatever chemists may hold, there can be no doubt that the estimation of our science by the outside world rests largely on the well-founded belief that chemistry is useful. Indeed, though scientific chemists are justly eager to vindicate the value of investigations remote from any application to the arts, they cannot feel a livelier sense of triumph when the suc-

* Bull. de la Soc. Chem. de Paris, t. xxii. p. 337, and t. xxiii. p. 295.
† Ann. Chim. et Phys., 5me série, t. l. p. 122.
‡ Bull. de la Soc. Chim. de Paris, t. xxiv. p. 197.

cessful synthesis of a vegetable principle yields at the same time a product of great technical value, as in the case of the production of artificial alizarin.

By visiting in turn the principal centres of British industry, this Association brings together men engaged on pure and on applied chemistry. We who come as visitors may hope that our papers and discussions here may bring fresh interest in the science, if not actual hints for practice, to those whose art or manufacture is based on chemistry. In return, the most interesting communications the Section has received have not unfrequently been the descriptions of local industries; and there is no part of our hospitable reception more welcome and more instructive to us than the opportunities which are provided of seeing chemical transformations on a large scale, effected by processes which observation and inventiveness have gradually brought to perfection and with the surprising familiarity and skill which are engendered by daily use.

SECTION D.—BIOLOGY.

Department of Zoology and Botany.

Dr. Hector, chief of the New Zealand Survey, gave a most interesting account of the modes of occurrence of the Moa bones in New Zealand. He used the term Moa in preference to that of Dinornis, because the bones of the New Zealand birds were now divided among so many genera. He demonstrated most conclusively that the knowledge of their former existence was not communicated to the Maoris by the Europeans, who deduced their structure from their remains, but, on the contrary, was imparted to the latter by the former. Up to recent times there had been a constant fulfilment of the statements made by the Maoris concerning the localities in which the bones would be found. He believed there was no hope of ever finding the birds alive, for he himself had been over the whole of the islands very thoroughly without seeing them. Dr. Hector exhibited a map of New Zealand on which were denoted all the areas in which Moa bones had been found, and all the localities in which considerable finds of bones had been made, with indications of their condition or surroundings. He found that the country occupied by primeval forests before the advent of Europeans was that in which Moa bones did *not* occur. His deduction was that they lived in the open and low scrub, in which they could walk. In all this region, within his own memory, the Moa bones were extremely abundant in the South Island, all over the ground; but these bones were very rarely found in collections, for they were usually decomposed and split and warped. In the enormous extent of Sub-Alpine country in the South Island, which was covered by only a light vegetation, large quantities of well-preserved Moa remains had been recently found, associated with remains or reliques of natives. It appeared to him that the natives had pressed up the country for the purpose of capturing, killing, and eating the Moas; and as the natives could not follow them through the sharp bayonet-grass and other underscrub, they seemed to have got at them by setting portions of it on fire, which collected the animals together, often killed them, and accounted for so many of their bones being accumulated in particular spots. And in some of these localities where the Moas were destroyed by fire, little heaps of chalcedonic quartz pebbles, which were their crop-stones, were found, each heap associated with the remains of one bird. And this fact, of their being the crop-stones, had been conclusively proved by the discovery of a carcase crushed and decayed so as to be unfit for anatomical purposes, but containing within the thorax just such a little heap of pebbles as had been described. The second chief mode of occurrence of Moa bones was in the turbary deposits and desiccated swamps, occurring in almost all the valleys leading to the east coast. One notable deposit was at Glenmark, where the remains of a terrace at a higher level had been cut through by the stream, leaving a large turbary deposit on the shoulders of the hill on both sides. Here were found a great number of Moa bones, without any associated Maori implements. Out of this place had been got bones sufficient to cover twice the area of the Section Room. They occurred mixed together, and above, below, and among great accumulations of drift-wood, which were ten or twelve feet deep over many acres. The bones got out of that deposit indicated at least 1,700 individuals, which had either been carried down and smothered in floods or which had died naturally and been carried down by the water. Similar deposits occurred in caves, and in turbary deposits on the coast, which were exposed below high-water mark, showing that there had been comparatively modern submersion; but there were no marine deposits above, and they rested on a denuded surface of the latest Tertiary beds. There seemed to have been an uninterrupted submergence of New Zealand since the time when the Moas were first developed in such large numbers; and there had been no considerable re-emergence of the land since then. Another mode of occurrence of Moa bones was wherever the country was favourable for Maori camps, on the sheltered grassy plots and links, or among the sand-hills near. They were associated with their cooking-hollows, and with stone implements, which, however Neolithic in aspect, were similar to those used now by Maoris. It had been said that the oldest Moa remains were those associated with the ancient moraines of the upper valleys, but these were the great natural roads up which it was very likely that some Moas would travel and leave their remains there. In caves the Moa bones were found resting on the stalactitic shelves, perhaps cemented by a little carbonate of lime. They were hardly ever found on the lower surfaces of the caves; and he believed they had mostly gained access to the caves by falling through the upper chasms. He had evidence that sheep in modern days fell through in the same way, and their bones were found similarly situated in the caves. The earliest traces of the Moas that had been found were footprints at Poverty Bay, occurring in a soft pumice sandstone, within six or eight inches of the upper surface. Many blocks had been procured with these undoubted footprints. The lower surface of each depression was formed of very fine micaceous sand, but it was filled up with much coarser green quartzose sand. After the birds had passed, the impression had been filled up by blown sand. Undoubtedly a true bird-bone had been found in Tertiary deposits in New Zealand, but he was inclined to think it belonged to a gigantic extinct Penguin.—The President testified to the value of Dr. Hector's address by saying that he had never till that time really understood the modes of occurrence of Moa bones.—Prof. W. C. Williamson said that scientific workers who had advice and sympathy readily accessible to them could know little of the energy and enthusiasm required to sustain the solitary individual who had to labour without meeting a scientific or even an educated man for weeks and months. Dr. Hector was a conspicuous example in this respect, and deserved all the honour his fellow-workers in England could give him.

Dr. Carpenter gave a summary of the results of his investigations into the nervous and generative systems of comatula. He described as a nervous cord the cord existing in the axial hole of the skeletal segments, which Müller had described as a vessel. No cavity was to be found in it, and in a favourable plane of section branches from it to the tentacular muscles were detected. Although this cord was destitute of the ordinary structure and insulating material of nerves, that was explicable by the fact that only one kind of muscle had to be affected, and that all the muscles acted simultaneously, in flexion of the arm. The cord to each arm came off from the curious five-lobed organ in the calyx below the perivisceral cavity. This was determined to be the central nervous mass by the following experiment. A living comatula was taken, and the visceral mass was turned out. A needle was thrust into the supposed nervous organ, and instantly all the arms were coiled up to their full extent, and were gradually relaxed. This was repeated several times. A curious generative axis had also been discovered in the shape of a cord passing through the middle of the nervous centre, and through the visceral mass to spread into a plexus around the mouth. Thence branches were given off to the arms and pinnules, and the ovaries and testes were directly connected with these cords as axes. Dr. Carpenter said that these facts were such as to necessitate the separation of the crinoids much further from the rest of the echinoderms than hitherto. In fact, he considered they had little in common beyond the calcareous network of the skeleton. In conclusion he said that he had learnt from a trustworthy observer that after a recent hurricane in the West Indies a vast number of Pentacrini had strewed the shore of Barbadoes, in all stages of growth, from one inch to eighteen inches in length; but unfortunately no naturalist was at hand to reap the rich harvest.

Dr. I. Bayley Balfour read a paper *On the Flora and Geological Structure of the Mascarene Islands.* He said that in Bourbon there was a great contrast between the flora of the older north-western portion and that of the south-eastern district within the area formed by the volcano now acting. Here the soil was very barren, with only a few composites and other plants that flourished in a dry soil. The flora was not most closely allied to that of Africa, but rather to that of India and the Indian Archipelago.

There was a great profusion of ferns, mosses, and lower crypto-gams; and evergreens were abundant. The species were few in proportion to the genera, and the genera in proportion to the orders. The proportion of indigenous plants and of species to any area was generally small; but in Bourbon there was the great number of 1,700 species. The most remarkable genus in the group, perhaps, was Pandanus, the screw-pine, which had species peculiar to each island, though the commonest, *P. utilis*, occurred on all three islands. Certain genera were found to be endemic to the group, especially in the Rubiaceæ and Compositæ. In addition, in each island there were certain genera endemic to that island alone. In North-western Bourbon, although, as in Mauritius, settlers had produced much alteration by cutting down trees, &c., there was still an abundance of plants which flourished in a moist climate. The flora of Mauritius exhibited affinities with that of N. W. Bourbon, although possessing endemic genera. Perhaps no place in the world had had its flora so much altered by settlers, especially by means of fires through carelessness. The original flora had been almost exterminated. The few plants now remaining included one new genus; and there were certain peculiar Pandani, but the general type was allied to that of Mauritius. In many of the small volcanic and coral islands which surround Mauritius and Rodriguez, very often little more than rocks, there were genera which were peculiar to those islands, or else species that were representatives of other species existing on the main islands. Round Island, a mere cone near Mauritius, had three genera of palms represented by different species, which were found nowhere else; and exhibited many other peculiarities in its flora. Dr. Balfour reserved his opinion on the vexed question of the origin of these islands by independent volcanic action or by the submergence of an ancient continent connected with Africa; but stated that soundings taken between Mauritius and Rodriguez, about fifty miles west from the latter, gave a depth of 2,000 fathoms; while 100 miles S.W. of Mauritius the depth was 2,700 fathoms.—Prof. Williamson remarked on the parallel between these facts and those first brought to light by Mr. Darwin relative to Galapagos. It appeared that these modifications of species and genera were such as must necessarily have resulted from modifications in a long course of time; and they compelled naturalists to accept Mr. Darwin's views whether they liked them or not. Coupled with the facts derived by Mr. Wallace from the Indian Archipelago, he thought considerable probability was given to the submergence theory.—Prof. Dickson could not see that the occurrence of representative forms on different oceanic islands was any stronger proof of evolution than the facts relating to the grouping of plants about geographical centres; but Prof. Williamson maintained that the occurrence of distinct yet analogous species on contiguous islands of very recent geological age was a striking evidence of modification produced by new physical conditions, unless indeed distinct new creative acts were admitted within a comparatively modern period.

Prof. Williamson gave an account of his recent discoveries among the fossil seeds of the coal measures, and partly confirmed and partly controverted Brogniart's views on some of the same seeds. He (Prof. Williamson) gave the name Lagenostoma to a form of seed larger and more bulky than a grain of rice, which had a flask-shaped cavity above the nucleus, between it and the micropyle. This cavity was surrounded by a membrane quite distinct from that investing the nucleus. Prof. Williamson believed that he had found pollen grains in this cavity, and that the only difference between this and an ordinary coniferous seed consisted in the presence of this chamber, which protected the pollen and brought it into contact with the nucleus. Another seed of the same general type had the upper part of the nucleus contracted, forming a sort of mammilla: thus the cavity above became of a different shape. He named it Physostoma. Another type he called Æthiotesta. All these were from the Lancashire coal-field. A specimen from Burntisland showed a transition from the extremely small and narrow micropyle of ordinary angiospermous seeds; and the large chamber of Lagenostoma. Prof. Williamson also referred to Cardiocarpum, which he found to have the nucleus thickened, and to have a prolonged spur containing the micropyle. Antholithes and Cardiocarpum were but portions of the same flowering plant. He found that Trigonocarpum had really a long projection at the end, of a similar nature, but from some Newcastle specimens he inferred that it had a large investing sarcocarp. The type was not at all dissimilar to Cardiocarpum.

Prof. Balfour, in a *Notice of Rare Plants from Scotland*, drew attention to the discovery of *Naias flexilis* in Perthshire, hitherto only found in Ireland. He exhibited the original specimen of *Salix sadleri* and *Carex frigida*, discovered in Scotland last year by Mr. Sadler.—Dr. I. Bayley Balfour contributed some notes on Turneriaceæ from Rodriguez, especially referring to one new form.—Prof. A. Dickson exhibited a *Primula vulgaris* with interpetaline lobes, and pointed out its relations to Soldenella and other Primulaceæ; he also described a monstrosity in *Saxifraga stellaris*, in which there occurred a calyx, no corolla, many stamens, and many carpels. Two specimens were found, each with a single terminal monstrous flower.

It is to be regretted that there was a paucity in the attendance of distinguished zoologists and botanists, and that the number and importance of the papers read was not so great as to furnish any idea of a widespread existence or encouragement of research in natural history. It might be well for naturalists to put themselves in evidence a little more strongly, and to show the value of their results more prominently, if they desire to be aided in their researches by public funds, or to win general sympathy, especially when geologists and anthropologists make such vigorous displays of their conquests.

Department of Anatomy and Physiology.

Prof. Rolleston, in moving a vote of thanks to Prof. Cleland for his presidential address to the department, said he had rarely spent an hour with more pleasure than in listening to that address. He would show the value he set upon it by saying that Prof. Cleland's old master, the great John Goodsir, would have been glad to hear it. He believed much of what the President had said would take its date from that meeting as of permanent authority and value.

Dr. McKendrick read the important report *On the Physiological Action of Light*, by himself and Prof. Dewar. We hope to publish it in full in an early number.

Mr. W. J. Cooper, in a paper *On the Physiological Effects of various Drinking Waters*, referred to the experiments of M. Papillon on various animals, described before the French Academy of Science in 1870–73, by which it was shown that not only the ash of the food eaten affects the composition of the bones, but also that mineral matter in dilute solution is capable of being assimilated. Consequently, alterations in the composition of the water supply of a community might be of very great importance to the organic structure of the human body, if the very composition of the bones is affected by the quality of the water. The inorganic impurities of water had been too much overlooked, notwithstanding the serious consequences which sometimes follow. Mr. Cooper insisted that one of the first conditions in the inauguration of a water-supply should be to ensure perfect freedom from excess of any mineral except those comparatively harmless ingredients, chloride of sodium and carbonate of lime.

Mr. T. G. P. Hallett read a paper *On the Conservation of Forces*, devoted to a long argument against this principle being extended to vital phenomena. He endeavoured to prove that life, whether tested by its origin or its effects, was a force, and that the laws of that force were not such as the conservation principle required and declared. Dr. Allen Thomson, at the close of the discussion which followed, thought it best to suspend judgment on the points that had been mooted, and to continue the quiet investigation of physical phenomena; his impression, derived from long observation, being that the more the phenomena of life were attended to, the more fully they were explained by known laws.

Among other papers may be mentioned Messrs. L. C. Miall and F. Greenwood's, *On Vascular Plexuses in the Elephant and some other Animals*, and Mr. Greenwood's *On the Preservation of the Larger Animals for Anatomical Examination*.

If the papers read before the Department of Anatomy and Physiology had to be taken as an index of the activity of research and thought concerning these subjects in Great Britain, we should have to confess ourselves to be at a low ebb. The department only sat on three days out of five, and those three days were certainly not crowded with valuable papers. The physiological investigations of Drs. McKendrick, Lauder Brunton, and Pye-Smith, and Prof. Dewar, were of high interest and great value; but the subjects they referred to cover only a very small part of the wide domain of Physiology. Morphology was represented most worthily by the President's address, but there was a plentiful lack of memoirs on descriptive anatomy, morphology, embryology, and histology. It is of course difficult to make the details of morphological investigation interesting in a spoken narration, but expositions of new or improved principles, and results of research in all departments, could be usefully brought forward at these meetings and receive illumination from discussion by those in authority. Are our anatomists and physiologists less willing to make such efforts than other scientific men, or have they a greater fondness for remaining in their own special haunts without emerging on any common ground?

Department of Anthropology.

Miss A. W. Buckland, of Bath, read a paper *On Rhabdomancy and Belomancy*, in which she endeavoured to show that rhabdomancy, or divination by means of a rod, still practised in England in some localities, was a survival of a very ancient superstition, originating in the use of rods as symbols of power.

Mr. John Evans described fully the proposed code of symbols for archæological maps which has been drawn up by a committee of leading archæologists on the continent of Europe, and will probably be extensively used. Suggestive crude symbols are adopted for the leading varieties of ancient remains, and a series of modifications of each chief form is to be used, to denote as far as possible the exact nature of the remains.

Mr. Hyde Clarke furnished a notice of the prehistoric names of weapons, in continuation of a note laid before the British Association in 1873, which showed that there was a community of aboriginal names of weapons in the prehistoric epoch. He now added that further research had confirmed these views.

Mr. Hyde Clarke also read a paper *On Prehistoric Culture in India and Africa*. After referring to his investigations as to the evidence of the successive migration and distribution of languages in Asia, Africa, North, Central, and South America, and in some cases in Australia, he proceeded to give the result of later special investigations as to the community of culture in India and Africa. The philology of the aboriginal languages of India could only be effectually studied from those of Africa, and Mr. Hyde Clarke suggested that it would be a great advantage if some of the missionaries of the two regions could interchange stations.—Prof. Rolleston remarked upon the desirableness of a complete work being prepared on the present ethnology of India, under the superintendence and at the cost of the Indian Government.

Dr. Phené, in his paper *On the Works, Manners, and Customs of the Prehistoric Inhabitants of the Mendip Hills*, adopted the theory of a similarity of race in the people who formerly occupied the caves on the Atlantic seaboard of Europe and of Britain; and identified the inhabitants of the Mendips with them.

Mr. D. Mackintosh read a paper *On Anthropology, Sociology, and Nationality*, which referred especially to distinctions of race in the British Isles, and defended his previously expressed views. He believed that the various colonising tribes had either continued in certain localities with little interblending, or that the process of amalgamation had not been sufficient to prevent the persistence of the more hardened characteristics. He tried to show that between the north-east and south-west the difference in the character of the people, irrespectively of circumstances, is so great as to give a semi-nationality to each division—restless activity, ambition, and commercial speculation predominating in the north-east, and contentment and leisurely reflection in the south-west.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.—DETROIT MEETING.

LAST week we gave a general account of the meeting of the American Association, from an American correspondent. The following are brief notices of some of the principal papers read.

We have already referred to the presidential address of Prof. Le Conte, and to the address of Prof. Dawson, both of which were anti-evolutionary, the latter more distinctly so than the former. Prof. Dawson's views are so well known that we need not refer at length to his Association address.

Prof. Augustus R. Grote, Director of the Museum of the Buffalo Academy of Sciences, undertook the task of throwing light upon past geological eras by showing the present distribution of certain North American insects. He described the glacial epoch as occurring at the close of the Tertiary by a continuous loss of heat. The winters gradually lengthened, the summers shortened. The tops of mountains that now bear foliage were then covered with snow, which, in time consolidating, formed glacial ice that flowed into the valleys. Gradually an icy sea extending from the north spread southward, even over the Southern States and down the Valley of the Mississippi. Existing insects of the Pliocene, no matter how gradually they were affected by the change, must have eventually left their haunts, and doubtless many species were exterminated. At the present day there are found in the tops of the White Mountains, and in the lofty ranges of Colorado, certain species of butterflies and moths which are completely isolated. To find others of the same kinds we must explore the Plains of Labrador and the northern portions of our continent; there and there only do we find similar or analogous species. A White Mountain butterfly, *Oeneis Semidea*, was cited as an instance in point, and other butterflies and moths were mentioned, whose isolated habitats served to prove the general proposition. The retirement of the glacial seas at the close of the epoch was then considered. Then the summers were lengthening, while the winters were shortened. Then ice-loving insects, such as the White Mountain butterfly, hung on the edge of the ice sheet which supplied their food, and followed its retreat—not all, but some of their forms surviving. Straying upon the local glaciers of the mountain ranges, they were left behind in some instances, while the main body followed the retiring ice sheet to the far north. Those that were left behind still find the conditions of their existence in the snow-covered summits of the present day. As the valleys became warmer and glaciers fewer, the chances of their escape from their isolated positions gradually diminished till their removal became impossible.

Prof. E. S. Morse, of Salem, Mass., has for a long time made a study of the bones of embryo birds. At this meeting he recalled briefly the evidence he had shown last year regarding the existence of the intermedium in birds by citing the embryo tern, in which he had distinctly found it. This year he made a visit to Grand Menan expressly to study the embryology of the lower birds, and was fortunate in finding the occurrence of this bone in the petrel, sea-pigeon, and eider duck. This additional evidence showed beyond question the existence of four tarsal bones in birds, as well as four carpal ones. In these investigations he had also discovered embryo claws on two of the fingers of the wing —the index and middle finger. Heretofore in the adult bird a single claw only had occurred in a few species, such as the Syrian blackbird, spur-winged goose, knob-winged dove, jacana, mound bird, and a few others, and in these cases it occurred either on the index or middle finger or on the radial side of the metacarpus. All these facts lent additional proof of the reptilian affinities of birds.

Prof. S. P. Langley, of Alleghany Observatory, detailed some of the conclusions at which he had arrived after years of study of the solar surface. Prof. Langley first showed by comparative experiments that an absorptive atmosphere surrounds the sun. Little attention has in recent years been paid to the study of this atmosphere. The earlier efforts to tabulate its absorptive power, produced with different observers, though men of eminence, strangely discordant results. Their methods and deductions were given in detail. Secchi's results, making the neighbourhood of the edge of the sun about half the brightness of the centre, are probably near the fact. Prof. Langley applied well-known photometric methods to the problem. By attaching a circle of cardboard to the equatorial telescope, a solar image is received on the board, plainly showing spots, penumbræ, &c., if the image be one foot in diameter. From holes in this cardboard, pencils of rays issue, which being caught on a screen give a second series of images. If these images are caught upon separate mirrors, instead of a screen, their relative light can be made the subject of comparison with that of a disc of flame from Bunsen's apparatus, and thereby their relative intensity determined. Between each aperture and its respective mirror a lens was interposed which concentrated the pencil of rays. By suitable additions this apparatus can be converted to a Rumford photometer, and in this form it proved most available in Prof. Langley's hands. He found a value for the brilliancy of the umbra in sun-spots, considerably higher than that hitherto computed. The blackest umbra, he finds, is betweeen 5,000 and 10,000 times as bright as the full moon. The light of the sun is absorbed by its atmosphere not in the same, but in a greater proportion than its heat. A long series of experiments shows that not much more or less than one-half of the radiant heat of the sun is absorbed or suffers internal reflection by the atmosphere of the sun itself. Observations indicate that this atmosphere is (speaking comparatively) extremely thin; Prof. Langley is inclined to regard it as identical with the "reversing layer" observed by Dr. Young,