Hope, and St. Helena. But even if the latter were the case, to give convincing proof of the correctness of his theory, he would at the same time have to reproduce the observations at points widely distant from these stations. We would like to ask Wilde to turn out the Ascension declinations and inclinations, say, from 1700–1834. L. A. BAUER.

Friedenau, bei Berlin, July 21.

Time-Gauge of Niagara.

IN the summer of 1890, I had the opportunity of spending some months in Canada, where I devoted what time I had to spare to the later geology of the country.

The time-gauge of the Niagara Falls struck me, and naturally led to further investigation.

We are fairly justified in the assumption, from historical sources in Egypt and elsewhere, that no distinguishable change of climate has occurred for, say, four thousand years. Our first knowledge of Britain, nearly two thousand years ago, would indicate that the climate of the south coast was then, at least in summer, a few degrees higher than now. Restore the conditions, reafforest the country lying north, and we should probably find this state of affairs restored. Four thousand years is a good stretch in the mind to seven thousand, so we may safely assume the "Glacial Epoch" must be put back an indefinite time beyond that.

Now we find, looking at the superficial geology of the lakes, that Erie must be dissociated from the other four. There is every reason to believe it was a river basin draining by the Wabash and Manmee valleys into the Mississippi. Ontario again in pre-glacial times drained by Syracuse into the Atlantic. During the Ice Age these drainage valleys were blocked, as was possibly the present discharge by the St. Lawrence past Montreal. In post-glacial times, on the retiring of the ice, Ontario stood at a much higher level, and probably discharged over the Niagara ridge into Erie.

It is well known that an old river channel exists, passing from above Niagara and tending west of Queenstown to Ontario. It has been assumed that flowing out of Erie the channel divided, one branch flowing west, the other east of Queenstown, and that owing to erosion at the extremity, one (the western) became closed, while the other survived as the Niagara.

If this were the case, there must have been, for a time, two falls over the escarpment near Queenstown, but there is absolutely no evidence of there having been a fall at the extremity of the western branch.

What seems to have happened was that for an indefinite time Ontario discharged westward into Erie, which again drained into the Upper Mississippi. A slight change of level may have occurred, or a local flood have carried away some of the débris closing the Lower St. Lawrence, and Ontario found a way of escape to the east. A rapid erosion of the old valley must have occurred with the result of lowering Erie sufficiently to reverse its outfall, when the river took the lowest channel, and first flowed, as now, over the escarpment.

The time-gauge represents then, not the close of the glaciated period, but the epoch when Ontario returned to its pre-glacial discharge. The intermediate period, when it flowed into Erie, has apparently left only the old western channel as evidence of what may well have been a protracted period.

Shanghai, June 22. THOS. W. KINGSMILL.

Late Appearance of the Cuckoo.

ON Friday last, July 27, as I was walking along the Sion Vista in Kew Gardens, towards the river, I heard, far off to my left, the cry of a cuckoo. There was but one cry, and that had not the duplication of the first sound which usually marks his later utterances with us. Clearly though I had heard it, I might almost have doubted the testimony of my ears if I had not, on turning suddenly to the direction from which the sound had come, *seen* the bird rise quickly and fly across the river.

August 1. E. HUBBARD.

Height of Barometer.

CAN any of your readers refer me to the maximum and minimum *authenticated* heights of the barometer, which have been hitherto recorded (1) in England, (2) in any part of the world? It would, of course, be necessary to know the height of the place of observation above sea-level in the case of the minimum, at least. KARL PEARSON.

University College, London, August 5.

Magnetisation of Rock Pinnacles.

MR. HILL will be glad to find that systematic observations on the magnetism of rock masses have been taken for the very district he mentions in his letter of July 28.

In vol. x., part 2, of the *Journal* of the Royal Institution of Cornwall, there appears a short paper on "The Magnetism of the Lizard Rocks," by Mr. Thomas Clark. In this he gives not only the results obtained, but his method of procedure. A subsequent paper (printed in vol. xi., part 2, of the *Journal*), on "The Magnetic Rocks of Cornwall," gives the results of his experiments, and is accompanied by a map of the county showing the position of its magnetic rocks. I understand that Mr. Clark is continuing his research in this direction.

If similar observations were taken throughout the whole of the country, especially in the neighbourhood of the coast, doubtless they would yield results of great value to commerce as well as to science. M. M. S.

IF Mr. Hill will refer to *Alpine Journal*, vol. xiii. p. 439, he will find mention of a magnetic peak in the Black Coolins: the mountain bears the name Bidein Druim nau Ramh.

Eccles, August 5. JAMES HEELIS.

THE BRITISH ASSOCIATION.

OXFORD, AUGUST 8.

THE sixty-fourth meeting of the British Association, and the fourth which has been held at Oxford, may now be fairly said to have begun. The reception-room was opened at 2 p.m. on Monday last, and at the moment of the opening of the doors there was an unexampled rush to obtain places in the Sheldonian Theatre for the President's address and the evening lectures. The places in the theatre have been filled with extraordinary quickness, and it is to be feared that late-comers, who have not availed themselves of the offer of the Local Secretaries to engage seats beforehand by letter, will be disappointed in the places which they obtain. This is an unusual occurrence, and demands some explanation. The Sheldonian Theatre is the largest building now standing in Oxford. The old Corn Exchange was larger, and could have comfortably accommodated the audience which assembled to hear Lord Salisbury on Wednesday night. But unfortunately it is no longer existent. It has been pulled down, with the other civic buildings, to make room for larger successors, which are only half completed, and the Local Committee must regret, without being able to remedy, the circumstance that the only available place of meeting is insufficient for the needs of the Association.

Lord Salisbury's address is fully reported in another part of this issue. Many of those who know Lord Salisbury only as a politician and as Minister for Foreign Affairs, will be surprised at the wide range of thought and reading displayed in this address, and more still at the keen critical faculty displayed in his handling of the diverse topics which he passes under review. Possibly the whole of his audience will not entirely agree with his views on current scientific problems, and his concluding remarks on the present position of the Darwinian theory offer almost a repetition of the controversy which made the last meeting at Oxford so famous four-and-thirty years ago. The two other evening addresses are not likely to fall far short in interest of the opening meeting. As is usual, Thursday morning is devoted to the addresses of the Presidents of Sections, and three of these are reported at length in this issue. The addresses at Oxford differ necessarily in one respect from those which are delivered at many other centres at which the Association meets. Oxford has in its relations to science a historical interest, as well as a more present interest in virtue of its being a seat of learning. It will accordingly be found that many of the Sectional Presidents touch upon the history of science as exemplified by Oxford, and enlarge upon its needs as an instrument of culture and education. Oxford indeed, though it is not generally supposed to be a scientific University, has a past which it may look on with pride. Few may remember that Roger Bacon, an early devotee and martyr to science, lived and worked at Oxford, and that this is the sex-centenary of the reputed year of his death. A second period, mentioned by Prof. Dixon in his opening address, is that of Robert Boyle and his colleagues, among whom was for a short time the illustrious Harvey, a band of men who were virtually the founders of the Royal Society.

The proceedings of the Sections derive great interest from the unusual number of communications by eminent foreign men of science. The proceedings of some of the Sections have already been indicated in previous numbers of NATURE; those of others are not even now settled into definite shape. In Section A (Mathematical and Physical Science), besides the joint meetings with Section G, which have already been mentioned, there are, amongst other important papers set down for Thursday, one on "Preliminary Experiments proving the Electrification of Air by the Subtraction of Water from it," by Lord Kelvin and Magnus McLean; another, by Lord Kelvin and Alexander Galt, on "Leyden Jar Discharges through Divided Channels," and a third, by Prof. G. Quincke, on "The Formation of Soap Bubbles by the Contact of Alkaline Oleates with Water." On Saturday Prof. Everett reads on "Some Jointed Frames or Linkages," and Dr. P. H. Schoute on "The Order of the Groups related to the Anallagmatic Displacements of the Regular Bodies in n-dimensional space." On Monday there is a paper by Lord Rayleigh, of which the title is not yet published, and others follow by Prof. H. H. Turner, Prof. Viriamu Jones, Mr. F. H. Newall, and Prof. O. J. Lodge.

In Section D, the Department of Botany, which meets by itself in Magdalen College School, has some very interesting matter. There are important papers by Prof. D. H. Campbell, of the University of California, and Prof. F. O. Bower, on "The Morphology of Vascular Cryptogams"; by Prof. E. Strasburger, on "Chromosomenzahl"; by Dr. Leopold Kny, on "Correlation between Root and Shoot"; by Prof. Green, on "Influence of Light on Diastase," and by Prof. Dukinfield Scott on "The Structure of Fossil Plants and their bearing on Botanical Problems."

The Anthropological Section, of which some account has been given in an earlier number, will devote the greater part of Friday and Monday to discussions on Early Man in Western Europe, in which M. Emile Cartailhac and Comte Goblet d'Alviella will take a leading part; and on Tuesday and Wednesday, various papers on Ethnography will range from North Africa to Australia.

At the soirée in the University Museum, on Thursday evening, there will be a few interesting exhibits, chief among which will be Prof. Henrici's linkage models, exhibitions by the Cambridge Scientific Instrument Company, by Prof. Everett, and demonstrations of anthropometrical methods, by Dr. J. G. Garson.

INAUGURAL ADDRESS BY THE MOST HON. THE MARQUIS OF SALISBURY, K.G., D.C.L., F.R.S., CHANCELLOR OF THE UNIVERSITY OF OXFORD, PRESIDENT.

MY functions are of a more complicated character than usually is assigned to the occupants of this chair. As Chancellor of the University it is my duty to tender to the British Association a hearty welcome, which it is my duty as President of the Association to accept. As President of the Association I convey, most unworthily, the voice of English science, as many worthy and illustrious Presidents have done before me; but in representing the University I represent far more fittingly the learners who are longing to hear the lessons which the first teachers of English science have come as visitors to teach. I am bound to express on behalf of the University our sense of the good feeling towards that body which is the motive of this unusual arrangement. But as far as I am personally concerned, it is attended with some embarrassing results. In presence of the high priests of science I am only a layman, and all the skill of all the chemists the Association contains will not transmute a layman into any more precious kind of metal. Yet it is my hard destiny to have to address on scientific matters probably the most competent scientific audience in the world. If a country gentleman, who was also a colonel of Volunteers, were by any mental aberration on the part of the Commander-in-Chief to be appointed to review an army corps at Aldershot, all military men would doubtless feel a deep compassion for his inevitable fate. I bespeak some spark of that divine emotion when I am attempting to discharge under similar conditions a scarcely less hopeless duty. At least, however, I have the consolation of feeling that I am free from some of the anxieties which have fallen to those who have preceded me as Presidents in this city. The relations of the Association and the University are those of entire sympathy and good will, as becomes common workers in the sacred cause of diffusing enlightenment and knowledge. But we must admit that it was not always so. A curious record of a very different state of feeling came to light last year in the interesting biography of Dr. Pusey, which is the posthumous work of Canon Liddon. In it is related the first visit of the Association to Oxford in 1832. Mr. Keble, at that time a leader of University thought, writes indignantly to his friend to complain that the honorary degree of D.C.L. had been bestowed upon some of the most distinguished members of the Association: "The Oxford Doctors," he says, "have truckled sadly to the spirit of the times in receiving the hodge-podge of philosophers as they did." It is amusing, at this distance of time, to note the names of the hodge-podge of philosophers whose academical distinctions so sorely vexed Mr. Keble's gentle spirit. They were Brown, Brewster, Faraday, and Dalton. When we recollect the lovable and serene character of Keble's nature, and that he was at that particular date probably the man in the University who had the greatest power over other men's minds, we can measure the distance we have traversed since that time; and the rapidity with which the converging paths of these two intellectual luminaries, the University and the Association, have approximated to each other. This sally of Mr. Keble's was no passing or accidental caprice. It represented a deep-seated sentiment in this place of learning, which had its origin in historic causes, and which has only died out in our time. One potent cause of it was that both bodies were teachers of science, but did not then in any degree attach the same meaning to that word. Science with the University for many generations bore a signification different from that which belongs to it in this assembly. It represented the knowledge which alone in the Middle Ages was thought worthy of the name of science. It was the knowledge gained not by external observation, but by mere reflection. The student's microscope was turned inward upon the recesses of his own brain; and when the supply of facts and realities failed, as it very speedily did, the scientific imagination was not wanting to furnish to successive generations an interminable series of conflicting speculations. *That* science—science in our academical sense—had its day of rapid growth, of boundless aspiration, of enthusiastic votaries. It fascinated the rising intellect of the time, and it is said—people were not particular about figures in those days—that its attractions were at one time potent enough to gather round the University thirty thousand students, who for the sake of learning its teaching were willing to endure a life of the severest hardship. Such a state of feeling is now an archæological curiosity. The revolt against Aristotle is now some three centuries old. But the mental sciences which were supposed to rest upon his writings have retained some of their ascendency even till this day, and have only slowly and jealously admitted the rivalry of the growing sciences of observation. The subject is interesting to us, as this undecided state of feeling coloured the experiences of this Association at its last Oxford visit, nearly a generation later, in 1860. The warmth of the encounters which then took place have left a vivid impression on the minds of those who are old enough to have witnessed them. That much energy was on that occasion converted into heat may, I think, be inferred from the mutual distance which the two bodies have since maintained. Whereas the visit of 1832 was succeeded by another visit in fifteen years, and the visit of 1847 was succeeded by another visit in thirteen years, the year 1860 was followed by a long and dreary interval of separation, which has only now, after four-and-thirty years, been terminated. It has required the lapse of a generation to draw the curtain of oblivion over those animated scenes. It was popularly supposed that deep divergences upon questions of religion were the motive force of those high controversies. To some extent that impression was correct. But men do not always discern the motives which are really urging them, and I suspect that in many cases religious apprehensions only masked the resentment of the older learning at the appearance and claims of its younger rival. In any case there is something worthy of note, and something that conveys encouragement, in the difference of the feeling which prevails now and the feeling that was indicated then. Few men are now influenced by the strange idea that questions of religious belief depend on the issues of physical research. Few men, whatever their creed, would now seek their geology in the books of their religion, or, on the other hand, would fancy that the laboratory or the microscope could help them to penetrate the mysteries which hang over the nature and the destiny of the soul of man. And the old learning no longer contests the share in education which is claimed by the new, or is blind to the supreme influence which natural knowledge is exercising in moulding the human mind.

A study of the addresses of my learned predecessors in this office shows me that the main duty which it falls to a President to perform in his introductory address, is to remind you of the salient points in the annals of science since last the Association visited the town in which he is speaking. Most of them have been able to lay before you in all its interesting detail the history of the particular science of which each one of them was the eminent representative. If I were to make any such attempt I should only be telling you with very inadequate knowledge a story which is from time to time told you, as well as it can be told, by men who are competent to deal with it. It will be more suitable to my capacity if I devote the few observations I have to make to a survey not of our science but of our ignorance. We live in a small bright oasis of knowledge surrounded on all sides by a vast unexplored region of impenetrable mystery. From age to age the strenuous labour of successive generations wins a small strip from the desert and pushes forward the boundary of knowledge. Of such triumphs we are justly proud. It is a less attractive task—but yet it has its fascination as well as its uses—to turn our eyes to the undiscovered country which still remains to be won, to some of the stupendous problems of natural study which still defy our investigation. Instead, therefore, of recounting to you what has been done, or trying to forecast the discoveries of the future, I would rather draw your attention to the condition in which we stand towards three or four of the most important physical questions which it has been the effort of the last century to solve.

Of the scientific enigmas which still, at the end of the nineteenth century, defy solution, the nature and origin of what are called the elements is the most notable. It is not, perhaps, easy to give a precise logical reason for the feeling that the existence of our sixty-five elements is a strange anomaly and conceals some much simpler state of facts. But the conviction is irresistible. We cannot conceive, on any possible doctrine of cosmogony, how these sixty-five elements came into existence. A third of them form the substance of this planet. Another third are useful, but somewhat rare. The remaining third are curiosities scattered haphazard, but very scantily, over the globe, with no other apparent function but to provide occupation for the collector and the chemist. Some of them are so like each other that only a chemist can tell them apart: others differ immeasurably from each other in every conceivable particular. In cohesion, in weight, in conductivity, in melting point, in chemical proclivities they vary in every degree. They seem to have as much relation to each other as the pebbles on a sea beach, or the contents of an ancient lumber room. Whether you believe that Creation was the work of design or of inconscient law, it is equally difficult to imagine how this random collection of dissimilar materials came together. Many have been the attempts to solve this enigma; but up till now they have left it more impenetrable than before. A conviction that here was something to discover lay beneath the persistent belief in the possibility of the transmutation of other metals into gold, which brought the alchemy of the Middle Ages into being. When the immortal discovery of Dalton established that the atoms of each of these elements have a special weight of their own, and that consequently they combine in fixed ponderable proportions from which they never depart, it renewed the hope that some common origin of the elements was in sight. The theory was advanced that all these weights were multiples of the weight of hydrogen—in other words, that each elementary atom was only a greater or a smaller number of hydrogen atoms compacted by some strange machinery into one. The most elaborate analyses, conducted by chemists of the highest eminence—conspicuously by the illustrious Stas—were directed to the question whether there was any trace in fact of the theoretic idea that the atoms of each element consist of so many atoms or even of so many half-atoms of hydrogen. But the reply of the laboratories has always been clear and certain—that there is not in the facts the faintest foundation for such a theory.

Then came the discovery of the spectrum analysis, and men thought that with an instrument of such inconceivable delicacy we should at last find out something as to the nature of the atom. The result has been wholly disappointing. Spectrum analysis in the hands of Dr. Huggins and Mr. Lockyer and others has taught us things of which the world little expected to be told. We have been enabled to measure the speed with which clouds of blazing hydrogen course across the surface of the sun; we have learnt the pace—the fabulous pace—at which the most familiar stars have been for ages approaching to or receding from our planet, without apparently affecting the proportions of the patterns which as far as historical record goes back they have always delineated on the evening sky. We have received some information about the elementary atoms themselves. We have learnt that each sort of atom when heated strikes upon the ether a vibration, or set of vibrations, whose rate is all its own; and that no one atom or combination of atoms in producing its own spectrum encroaches even to the extent of a single line upon the spectrum that is peculiar to its neighbour. We have learnt that the elements which exist in the stars and specially in the sun are mainly those with which we are familiar upon earth. There are a few lines in excess to which we can give no terrestrial name; and there are some still more puzzling gaps in our list. It is a great aggravation of the mystery which besets the question of the elements, that among the lines which are absent from the spectrum of the sun, those of nitrogen and oxygen stand first. Oxygen constitutes the largest portion of the solid and liquid substance of our planet, so far as we know it; and nitrogen is very far the predominant constituent of our atmosphere. If the earth is a detached bit whirled off the mass of the sun, as cosmogonists love to tell us, how comes it that in leaving the sun we cleaned him out so completely of his nitrogen and oxygen that not a trace of these gases remains behind to be discovered even by the sensitive vision of the spectroscope?

All these things the discovery of the spectrum analysis has added to our knowledge; but it has left us as ignorant as ever as to the nature of the capricious differences which separate the atoms from each other, or the cause to which those differences are due.

In the last few years the same enigma has been approached from another point of view by Prof. Mendeléeff. The periodic law which he has discovered reflects on him all the honour that can be earned by ingenious, laborious, and successful research. He has shown that this perplexing list of elements can be divided into families of about seven, speaking very roughly: that those families all resemble each other in this, that as to weight, volume, heat, and laws of combination, the members of each family are ranked among themselves in obedience to the same rule. Each family differs from the others; but each internally is constructed upon the same plan. It was a strange discovery—strangest of all in its manifest defects. For in the plan of his families there were blanks left; places not filled up because the properly constituted elements required according to his theory had not been found to fill them. For the moment their absence seemed a weakness in the Professor's idea, and gave an arbitrary aspect to his scheme. But the weakness was turned into strength when, to the astonishment of the scientific world, three of the elements which were missing made their appearance in answer to his call. He had described beforehand the qualities they ought to have; and gallium, germanium, and scandium, when they were discovered shortly after the publication of his theory, were found to be duly clothed with the qualities he required in each. This remarkable confirmation has left Mendeléeff's periodic law in an unassailable position. But it has rather thickened than dissipated the mystery which hangs over the elements. The discovery of these co-ordinate families dimly points to some identical origin, without suggesting the method of their genesis or the nature of their common parentage. If they were organic beings all our difficulties would be solved by muttering the comfortable word "evolution"—one of those indefinite words from time to time vouchsafed to humanity, which have the gift of alleviating so many perplexities and masking so many gaps in our knowledge. But the families of elementary atoms do not breed; and we cannot therefore ascribe their ordered difference to accidental variations perpetuated by heredity under the influence of natural selection. The rarity of iodine, and the abundance of its sister chlorine, cannot be attributed to the survival of the fittest in the struggle for existence. We cannot account for the minute difference which persistently distinguishes nickel from cobalt, by ascribing it to the recent inheritance by one of them of an advantageous variation from the parent stock.

The upshot is that all these successive triumphs of research, Dalton's, Kirchhoff's, Mendeléeff's, greatly as they have added to our store of knowledge, have gone but little way to solve the problem which the elementary atoms have for centuries presented to mankind. What the atom of each element is, whether it is a movement, or a thing, or a vortex, or a point having inertia, whether there is any limit to its divisibility, and, if so, how that limit is imposed, whether the long list of elements is final, or whether any of them have any common origin, all these questions remain surrounded by a darkness as profound as ever. The dream which lured the alchemists to their tedious labours, and which may be said to have called chemistry into being, has assuredly not been realised, but it has not yet been refuted. The boundary of our knowledge in this direction remains where it was many centuries ago.

The next discussion to which I should look in order to find unsolved riddles which have hitherto defied the scrutiny of science, would be the question of what is called the ether. The ether occupies a highly anomalous position in the world of science. It may be described as a half-discovered entity. I dare not use any less pedantic word than entity to designate it, for it would be a great exaggeration of our knowledge if I were to speak of it as a body or even as a substance. When nearly a century ago Young and Fresnel discovered that the motions of an incandescent particle were conveyed to our eyes by undulation, it followed that between our eyes and the particle there must be something to undulate. In order to furnish that something, the notion of the ether was conceived, and for more than two generations the main, if not the only, function of the word ether has been to furnish a nominative case to the verb "to undulate." Lately, our conception of this entity has received a notable extension. One of the most brilliant of the services which Prof. Maxwell has rendered to science has been the discovery that the figure which expressed the velocity of light, also expressed the multiplier required to change the measure of static or passive electricity into that of dynamic or active electricity. The interpretation reasonably affixed to this discovery is that, as light and the electric impulse move approximately at the same rate through space, it is probable that the undulations which convey them are undulations of the same medium. And as induced electricity penetrates through everything, or nearly everything, it follows that the ether through which its undulations are propagated must pervade all space, whether empty or full, whether occupied by opaque matter or transparent matter, or by no matter at all. The attractive experiments by which the late Prof. Hertz illustrated the electric vibrations of the ether will only be alluded to by me, in order that I may express the regret deeply and generally felt that death should have terminated prematurely the scientific career which had begun with such brilliant promise and such fruitful achievements. But the mystery of the ether, though it has been made more fascinating by these discoveries, remains even more inscrutable than before. Of this all-pervading entity we know absolutely nothing except this one fact, that it can be made to undulate. Whether outside the influence of matter on the motion of its waves, ether has any effect on matter or matter upon it, is absolutely unknown. And even its solitary function of undulating ether performs in an abnormal fashion which has caused infinite perplexity. All fluids that we know transmit any blow they have received by waves which undulate backwards and forwards in the path of their own advance. The ether undulates athwart the path of the wave's advance. The genius of Lord Kelvin has recently discovered what he terms a labile state of equilibrium, in which a fluid that is infinite in its extent may exist, and may undulate in this eccentric fashion without outraging the laws of mathematics. I am no mathematician, and I cannot judge whether this reconciliation of the action of the ether with mechanical law is to be looked upon as a permanent solution of the question, or is only what diplomatists call a *modus vivendi*. In any case it leaves our knowledge of the ether in a very rudimentary condition. It has no known qualities except one, and that quality is in the highest degree anomalous and inscrutable. The extended conception which enables us to recognise ethereal waves in the vibrations of electricity has added infinite attraction to the study of those waves, but it carries its own difficulties with it. It is not easy to fit in the theory of electrical ether waves with the phenomena of positive and negative electricity, and as to the true significance and cause of those counteracting and complementary forces, to which we give the provisional names of negative and positive, we know about as much now as Franklin knew a century and a half ago.

I have selected the elementary atoms and the ether as two instances of the obscurity that still hangs over problems which the highest scientific intellects have been investigating for several generations. A more striking but more obvious instance still is Life—animal and vegetable Life—the action of an unknown force on ordinary matter. What is the mysterious impulse which is able to strike across the ordinary laws of matter, and twist them for a moment from their path? Some people demur to the use of the term "vital force" to designate this impulse. In their view the existence of such a force is negatived by the fact that chemists have been able by cunning substitutions to produce artificially the peculiar compounds which in nature are only found in organisms that are or have been living. These compounds are produced by some living organism in the performance of the ordered series of functions proper to its brief career. To counterfeit them—as has been done in numerous cases—does not enable us to do what the vital force alone can effect—to bring the organism itself into existence, and to cause it to run its appointed course of change. This is the unknown force which continues to defy not only our imitation but our scrutiny. Biology has been exceptionally active and successful during the last half-century. Its triumphs have been brilliant, and they have been rich enough not only in immediate result but in the promise of future advance. Yet they give at present no hope of penetrating the great central mystery. The progress which has been made in the study of microscopic life has been very striking, whether or not the results which are at present inferred from it can be taken as conclusive. Infinitesimal bodies found upon the roots of plants have the proud office of capturing and taming for us the free nitrogen of the air, which, if we are to live at all, we must consume and assimilate, and yet which, without the help of our microscopic ally, we could not draw for any useful purpose from the ocean of nitrogen in which we live. Microscopic bodies are convicted of causing many of the worst diseases to which flesh is heir, and the guilt of many more will probably be brought home to them in due time; and they exercise a scarcely less sinister or less potent influence on our race by the plagues with which they destroy some of the most valuable fruits of husbandry, such as the potato, the mulberry, and the vine. Almost all their power resides in the capacity of propagating their kind with infinite rapidity, and up to this time science has been more skilful in describing their ravages than in devising means to hinder them. It would be ungrateful not to mention two brilliant exceptions to this criticism. The antiseptic surgery which we owe chiefly to Lister; and the inoculation against anthrax, hydrophobia, and perhaps some other diseases, which we owe to Pasteur, must be recorded as splendid victories over the countless legions of our infinitesimal foes. Results like these are the great glory of the scientific workers of the past century. Men may, perhaps, have overrated the progress of nineteenth-century research in opening the secrets of nature; but it is difficult to overrate the brilliant service it has rendered in ministering to the comforts and diminishing the sufferings of mankind.

If we are not able to see far into the causes and origin of life in our own day, it is not probable that we shall deal more successfully with the problem as it arose many million years ago. Yet certainly the most conspicuous event in the scientific annals of the last half-century has been the publication of Mr. Darwin's work on the "Origin of Species," which appeared in 1859. In some respects, in the depth of the impression which it made on scientific thought, and even on the general opinion of the world, its momentous effect can hardly be overstated. But at this distance of time it is possible to see that some of its success has been due to adventitious circumstances. It has had the chance of enlisting among its champions some of the most powerful intellects of our time, and perhaps the still happier fortune of appearing at a moment when it furnished an armoury of weapons to men, who were not scientific, for use in the bitter but transitory polemics of the day. But far the largest part of its accidental advantages was to be found in the remarkable character and qualifications of its author. The equity of judgment, the simple-minded love of truth and the patient devotion to the pursuit of it through years of toil and of other conditions the most unpropitious—these things endeared to numbers of men everything that came from Charles Darwin, apart from its scientific merit or literary charm. And whatever final value may be assigned to his doctrine, nothing can ever detract from the lustre shed upon it by the wealth of his knowledge and the infinite ingenuity of his resource. The intrinsic power of his theory is shown at least in this one respect, that in the department of knowledge with which it is concerned it has effected an entire revolution in the methods of research. Before his time the study of living nature had a tendency to be merely statistical; since his time it has become predominantly historical. The consideration how any organic body came to be what it is occupies a far larger area in any inquiry now than the mere description of its actual condition; but this question was not predominant—it may almost be said to have been ignored—in the Botanical and Zoological study of sixty years ago.

Another lasting and unquestioned effect has resulted from Darwin's work. He has, as a matter of fact, disposed of the doctrine of the immutability of species. It has been mainly associated in recent days with the honoured name of Agassiz, but with him has disappeared the last defender of it who could claim the attention of the world. Few now are found to doubt that animals separated by differences far exceeding those that distinguished what we know as species have yet descended from common ancestors. But there is much less agreement as to the extent to which this common descent can be assumed, or the process by which it has come about. Darwin himself believed that all animals were descended from "at most four or five progenitors"—adding that "there was grandeur in the view that life had been originally breathed by the Creator into a few forms or one." Some of his more devoted followers, like Prof. Haeckel, were prepared to go a step farther and to contemplate a crystal as the probable ancestor of the whole fauna and flora of this planet.

To this extent the Darwinian theory has not effected the conquest of scientific opinion; and still less is there any unanimity in the acceptance of natural selection as the sole or even the main agent of whatever modifications may have led up to the existing forms of life. The deepest obscurity still hangs over the origin of the infinite variety of life. Two of the strongest objections to the Darwinian explanation appear still to retain all their force.

I think Lord Kelvin was the first to point out that the amount of time required by the advocates of the theory for working out the process they had imagined could not be conceded without assuming the existence of a totally different set of natural laws from those with which we are acquainted. His view was not only based on profound mechanical reasoning, but it was so plain that any layman could comprehend it. Setting aside arguments deduced from the resistance of the tides, which may be taken to transcend the lay understanding, his argument from the refrigeration of the earth requires little science to apprehend it. Everybody knows that hot things cool, and that according to their substance they take more or less time in cooling. It is evident from the increase of heat as we descend into the earth, that the earth is cooling, and we know by experiment, within certain wide limits, the rate at which its substances, the matters of which it is constituted, are found to cool. It follows that we can approximately calculate how hot it was so many million years ago. But if at any time it was hotter at the surface by 50° F. than it is now, life would then have been impossible upon the planet, and therefore we can without much difficulty fix a date before which organic life on earth cannot have existed. Basing himself on these considerations Lord Kelvin limited the period of organic life upon the earth to a hundred million years, and Prof. Tait in a still more penurious spirit cut that hundred down to ten. But on the other side of the account stand the claims of the geologists and biologists. They have revelled in the prodigality of the ciphers which they put at the end of the earth's hypothetical life. Long cribbed and cabined within the narrow bounds of the popular chronology, they have exulted wantonly in their new freedom. They have lavished their millions of years with the open hand of a prodigal heir indemnifying himself by present extravagance for the enforced self-denial of his youth. But it cannot be gainsaid that their theories require at least all this elbow-room. If we think of that vast distance over which Darwin conducts us from the jelly-fish lying on the primeval beach to man as we know him now; if we reflect that the prodigious change requisite to transform one into the other is made up of a chain of generations, each advancing by a minute variation from the form of its predecessor, and if we further reflect that these successive changes are so minute that in the course of our historical period—say three thousand years—this progressive variation has not advanced by a single step perceptible to our eyes, in respect to man or the animals and plants with which man is familiar, we shall admit that for a chain of change so vast, of which the smallest link is longer than our recorded history, the biologists are making no extravagant claim when they demand at least many hundred million years for the accomplishment of the stupendous process. Of course, if the mathematicians are right, the biologists cannot have what they demand. If, for the purposes of their theory, organic life must have existed on the globe more than a hundred million years ago, it must, under the temperature then prevailing, have existed in a state of vapour. The jelly-fish would have been dissipated in steam long before he had had a chance of displaying the advantageous variation which was to make him the ancestor of the human race. I see, in the eloquent discourse of one of my most recent and most distinguished predecessors in this chair, Sir Archibald Geikie, that the controversy is still alive. The mathematicians sturdily adhere to their figures, and the biologists are quite sure the mathematicians must have made a mistake. I will not get myself into the line of fire by intervening in such a controversy. But until it is adjusted the laity may be excused for returning a verdict of "not proven" upon the wider issues the Darwinian school has raised.

The other objection is best stated in the words of an illustrious disciple of Darwin, who has recently honoured this city by his presence—I refer to Prof. Weismann. But in referring to him, I cannot but give, in passing, a feeble expression to the universal sorrow with which in this place the news was received that Weismann's distinguished antagonist, Prof. Romanes, had been taken from us in the outset and full promise of a splendid scientific career.

The gravest objection to the doctrine of natural selection was expressed by Weismann in a paper published a few months ago, not as agreeing to the objection, but as resisting it; and therefore his language may be taken as an impartial statement of the difficulty. "We accept natural selection," he says, "not because we are able to demonstrate the process in detail, not even because we can with more or less ease imagine it, but simply because we must—because it is the only possible explanation that we can conceive. We must assume natural selection to be the principle of the explanation of the metamorphoses, because all other apparent principles of explanation fail us, and it is inconceivable that there could yet be another capable of explaining the adaptation of organisms without assuming the help of a principle of design."

There is the difficulty. We cannot demonstrate the process of natural selection in detail; we cannot even, with more or less ease, imagine it. It is purely hypothetical. No man, so far as we know, has ever seen it at work. An accidental variation may have been perpetuated by inheritance, and in the struggle for existence the bearer of it may have replaced, by virtue of the survival of the fittest, his less improved competitors; but as far as we know no man or succession of men have ever observed the whole process in any single case, and certainly no man has recorded the observation. Variation by *artificial* selection, of course, we know very well; but the intervention of the cattle breeder and the pigeon fancier is the essence of artificial selection. It is effected by their action in crossing, by their skill in bringing the right mates together to produce the progeniture they want. But in natural selection who is to supply the breeder's place? Unless the crossing is properly arranged, the new breed will never come into being. What is to secure that the two individuals of opposite sexes in the primeval forest, who have been both accidentally blessed with the same advantageous variation, shall meet, and transmit by inheritance that variation to their successors? Unless this step is made good, the modification will never get a start; and yet there is nothing to insure that step, except pure chance. The law of chances takes the place of the cattle breeder and the pigeon fancier. The biologists do well to ask for an immeasurable expanse of time, if the occasional meetings of advantageously varied couples from age to age are to provide the pedigree of modifications which unite us to our ancestor the jelly-fish. Of course the struggle for existence, and the survival of the fittest, would in the long run secure the predominance of the stronger breed over the weaker. But it would be of no use in setting the improved breed going. There would not be time. No possible variation which is known to our experience, in the short time that elapses in a single life between the moment of maturity and the age of reproduction, could enable the varied individual to clear the field of all competitors, either by slaughtering or starving them out. But unless the struggle for existence took this summary and internecine character, there would be nothing but mere chance to secure that the advantageously varied bridegroom at one end of the wood should meet the bride, who by a happy contingency had been advantageously varied in the same direction at the same time at the other end of the wood. It would be a mere chance if they ever knew of each other's existence—a still more unlikely chance that they should resist on both sides all temptations to a less advantageous alliance. But unless they did so, the new breed would never even begin, let alone the question of its perpetuation after it had begun. I think Prof. Weismann is justified in saying that we cannot, either with more or less ease, imagine the process of natural selection.

It seems strange that a philosopher of Prof. Weismann's penetration should accept as established a hypothetical process the truth of which he admits that he cannot demonstrate in detail, and the operation of which he cannot even imagine. The reason that he gives seems to me instructive of the great danger scientific research is running at the present time—the acceptance of mere conjecture in the name and place of knowledge, in preference to making frankly the admission that no certain knowledge can be attained. "We accept natural selection," he says, "because we must—because it is the only possible explanation that we can conceive." As a politician, I know that argument very well. In political controversy it is sometimes said of a disputed proposal that it "holds the field," that it must be accepted because no possible alternative has been suggested. In politics there is occasionally a certain validity in the argument, for it sometimes happens that some definite course must be taken, even though no course is free from objection. But such a line of reasoning is utterly out of place in science. We are under no obligation to find a theory, if the facts will not provide a sound one. To the riddles which nature propounds to us the profession of ignorance must constantly be our only reasonable answer. The cloud of impenetrable mystery hangs over the development and still more over the origin of life. If we strain our eyes to pierce it, with the foregone conclusion that some solution is and must be attainable, we shall only mistake for discoveries the figments of our own imagination. Prof. Weismann adds another reason for his belief in natural selection, which is certainly characteristic of the time in which we live. "It is inconceivable," he says, "that there should be another principle capable of explaining the adaptation of organisms without assuming the help of a principle of design." The whirligig of time assuredly brings its revenges. Time was, not very long ago, when the belief in creative design was supreme. Even those who were sapping its authority were wont to pay it a formal homage, fearing to shock the public conscience by denying it. Now the revolution is so complete that a great philosopher uses it as a *reductio ad absurdum*, and prefers to believe that which can neither be demonstrated in detail, nor imagined, rather than run the slightest risk of such a heresy.

I quite accept the Professor's dictum that if natural selection is rejected we have no resource but to fall back on the mediate or immediate agency of a principle of design. In Oxford, at least, he will not find that argument is conclusive, nor, I believe, among scientific men in this country generally, however imposing the names of some whom he may claim for that belief. I would rather lean to the conviction that the multiplying difficulties of the mechanical theory are weakening the influence it once had acquired. I prefer to shelter myself in this matter behind the judgment of the greatest living master of natural science among us, Lord Kelvin, and to quote as my own concluding words the striking language with which he closed his address from this chair more than twenty years ago: "I have always felt," he said, "that the hypothesis of natural selection does not contain the true theory of evolution, if evolution there has been in biology. . . . I feel profoundly convinced that the argument of design has been greatly too much lost sight of in recent zoological speculations. Overpoweringly strong proofs of intelligent and benevolent design lie around us, and if ever perplexities, whether metaphysical or scientific, turn us away from them for a time, they come back upon us with irresistible force, showing to us through nature the influence of a free will, and teaching us that all living things depend on one everlasting Creator and Ruler."

SECTION A.

MATHEMATICS AND PHYSICS.

OPENING ADDRESS BY PROF. A. W. RÜCKER, M.A., F.R.S., PRESIDENT OF THE SECTION.

IT is impossible for a body of English scientific men to meet in one of our ancient university towns without contrasting the old ideal of the pursuit of learning for its own sake with the modern conception of the organisation of science as part of a pushing business concern.

We are, as a nation, convinced that education is essential to national success. Our modern universities are within earshot of the whirr of the cotton-mill or the roar of Piccadilly. Oxford and Cambridge themselves are not content to be centres of attraction to which scholars gravitate. They have devised schemes by which their influence is directly exerted on every market-town and almost on every village in the country. University extension is but a part of the extraordinary multiplication of the machinery of education which is going on all around us. The British Association, which was once regarded as bringing light into dark places, is now welcomed in every large provincial town by a group of well-known men of science; and we find ready for the meetings of our Sections, not only the chapels and concert-rooms which have so often and so kindly been placed at our disposal, but all the appliances of well-designed lecture-rooms and laboratories.

I do not propose, however, to detain you this morning with a discourse on the spread of scientific education, but you will forgive me if I illustrate its progress by two facts, not perhaps the most striking which could be selected, but especially appropriate to our place of meeting. It is little more than thirty years since the two branches of science with which our Section deals, Mathematics and Physics, have been generally recognised as wide enough to require more than one teacher to cope with them in an educational institution of high pretensions and achievement. In 1860 the authorities of the Owens College, Manchester, debated whether it was desirable to create a Professorship of Natural Philosophy in addition to, and independent of, the Chair of Mathematics. It was thought necessary to obtain external support for the opinions of those who advocated this step. An appeal was made to Profs. De Morgan and Stokes. The former reported that a "course of experimental physics is in itself desirable"; the latter, that "there would be work enough in a large institution for a mathematician and a physicist."

In the end the Chair of Natural Philosophy was established, and the fact that our host of to-day, Prof. Clifton, was its first occupant reminds us how little we have advanced in time and how far in educational development from the days when propositions such as those I have cited were only accepted on the authority of the names of Stokes and De Morgan.

The other fact to which I would refer is that the Clarendon Laboratory, in which the meetings of Section A are to be held, though erected barely a quarter of a century ago, was the first laboratory in this country which was specially built and designed for the study of experimental physics. It has served as a type. Clerk Maxwell visited it while planning the Cavendish Laboratory, and traces of Prof. Clifton's designs can be detected in several of our university colleges.

But though our surroundings remind us of the improvement which has been effected in the equipment of our science, it would not be difficult to indicate weak points which should forthwith be strengthened. On these, in so far as they affect education, I will not dwell—and that for two reasons. In the first place, we meet to-day not as teachers, but as students; and, secondly, I think that whereas we have as a nation awoke—though late in the day—to the importance of education, we are not yet fully awake to the importance of learning. Our attitude in such matters was exactly expressed by one of the most eminent of the witnesses who gave evidence before the "Gresham Commission." In his opinion the advancement of knowledge must in a university in London be secondary to the higher instruction of the youth of London. If this be so—and I will not now dispute it—we shall surely all agree that somewhere or other, in London or out of it, included in our universities or separate from them, there ought to be institutions in which the advancement of knowledge is regarded as of primary and fundamental interest, and not as a mere secondary by-product thrown off in the course of more important operations.

It is not essential that in such an institution research should be the only task. Investigation may be combined with the routine work of an observatory, with teaching, with the care of standards, or with other similar duties. It is, however, essential that, if the advancement of knowledge is seriously regarded as an end worth attaining, it should not be relegated to a secondary place.

Time and opportunity must be found for investigation, as time and opportunity are found for other tasks. It is not enough to refer to research in a prospectus and then to leave it to be accomplished at odd times and in spare moments not claimed by more urgent demands. Those to whom the future of the higher learning in England is dear must plan and scheme to promote the life-long studies of men, as in the last quarter of a century they have struggled, with marked success, to promote the preparatory studies of boys and girls. That the assignment of a secondary position to research is the more popular view, and that the necessity for encouraging it has as yet hardly been grasped by many of those who control our modern educational movements is, I fear, too true. It is therefore a matter for congratulation that within the last year Oxford has established a research degree, and has thus taken an important step towards gathering within her fold workers of mature years who are able and willing, not merely to gain knowledge, but to add to it.

We may also note, with pleasure and gratitude, that the stream of private munificence has recently been in part directed to the advancement of learning. Sir Henry Thompson has generously offered a sum of £5000 to provide a large photographic telescope for the National Observatory at Greenwich. The new instrument is to be of 26 inches aperture and 22 feet 6 inches focal length, or exactly double the linear dimensions of that which has been previously employed. Mr. Ludwig Mond, too, has added to his noble gifts to science by the new research laboratories which he is about to establish in connection with the Royal Institution. Albemarle Street is thronged with memories of great discoveries. The researches of Lord Rayleigh and the remarkable results of Prof. Dewar's studies of matter at low temperatures are maintaining the great reputation which the Royal Institution has gained in the past, and all English physicists will rejoice that prospects of new and extended usefulness are opening before it.

Another hopeful, though very embarrassing fact is that the growth in the number of scientific workers makes it increasingly difficult to find the funds which are necessary for the publication of their work. Up to the present the author of a paper has had to submit it to criticism, but, when it has been approved by competent judges, it has been published without ado and without expense to himself. This is as it should be. It is right that due care should be exercised to prune away all unnecessary matter, to reduce as far as may be the necessary cost. It will, however, be a great misfortune if judgment as to what curtailment is necessary is in future passed, not with the object of removing what is really superfluous, but in obedience to the iron rule of poverty. Apart from all other disadvantages, such a course would add to the barriers which are dividing the students of different sciences. A few lines and a rough diagram may suffice to show to experts what has been attempted and what achieved, but there is no paper so difficult to master as that which assumes that the reader starts from the point of vantage which months or years of study have enabled the author to attain. Undue pruning will not make the tree of knowledge more fruitful, and will certainly make it harder to climb.

Connected also with the vast increase of scientific literature is a growing necessity for the publication of volumes of abstracts, in which the main results of recent investigations are presented in a concentrated form. English chemists have long been supplied with these by the Chemical Society. The Physical Society, though far less wealthy than its elder sister, has determined to undertake a similar task. We are compelled to begin cautiously, but in January next the first number of a monthly pamphlet will be issued containing abstracts of all the papers which appear in the principal foreign journals of Physics. In this venture the Society will incur grave responsibilities, and I avail myself of this opportunity to appeal to all British physicists to support us in a work, the scope of which will be rapidly extended if our first efforts succeed.

From this brief glance at what has been or is about to be done to promote the study of Physics, I must now turn to the discussion of narrower but more definite problems, and I presume that I shall be most likely to deserve your attention if I select a subject in which I am myself especially interested.

During the last ten years my friend Dr. Thorpe and I have been engaged upon a minute magnetic survey of the United Kingdom. The main conclusions at which we have arrived are about to be published, and I do not propose to recount them now. It is, however, impossible to give so long a time to a single research without having one's attention drawn to a number of points which require further investigation, and I shall perhaps be making the best use of this opportunity if I bring to your notice some matters in the practical and theoretical study of terrestrial magnetism which deserve a fuller consideration than has yet been given to them.

In the first place, then, there is little doubt that the instruments at present used for measuring Declination and Horizontal Force are affected with errors far greater than the error of observation.

We employed four magnetometers by Elliott Brothers, which were frequently compared with the standard instrument at Kew. These measurements proved that the instrumental differences which affect the accuracy of the declination and horizontal force measurements are from five to ten times as great as the error of a single field observation. The dip circle which two generations ago was so untrustworthy is, in our experience, the most satisfactory of the absolute instruments.

In most cases these comparisons extended over several days, but the Astronomer Royal has described in his recent report observations made at Greenwich for two years and a half with two horizontal force instruments. These differ between themselves, and the discrepancy is of the same order of magnitude as those we have detected.

If such differences exist between instruments of the Kew pattern, it is probable that they will be still greater when the magnetometers under investigation are of different types.

This point has been investigated by Dr. Van Rijckevorsel, who five years ago visited Kew, Parc St. Maur, Wilhelmshaven, and Utrecht, and, using his own instruments at each place, compared the values of the magnetic elements determined by himself with those deduced from the self-registering apparatus of the observatory.

The discrepancies between the so-called standards, which were thus brought to light, were quite startling, and prove the necessity for an investigation as to their causes.

Magneticians had long been aware that the instruments used by travellers should be compared at the beginning and end of a journey with those at some fixed observatory, to make sure that the comparatively rough usage to which they are subjected has not affected their indications. But Dr. Van Rijckevorsel's expedition first drew general attention to the fact that there are serious differences between the standard observatory instruments themselves.

The importance of a careful comparison between them was at once recognised. The Magnetic Sub-Committee of the International Meteorological Conference, held at Munich in the autumn of 1891, resolved that it is "necessary that the instruments employed for absolute measurements at the different observatories should be compared with each other and the results published." As far as I am aware nothing has been done to give effect to this resolution, but the necessity for such an international comparison is urgent. The last few years have been a period of unexampled activity in the conduct of local magnetic surveys. To cite instances from the north-west of Europe only, observations have recently been made on a more or less extended scale in the United Kingdom, France, Holland, North Germany, and Denmark.

It will be absurd if these surveys cannot be collated and welded into a homogeneous whole, because we are in doubt whether the indications of our standard instruments for the measurement of declination and dip differ by five or six minutes of arc.

If, however, an official international comparison of the magnetic standards in use in different countries is instituted it is probable that only one observatory in each country will take part in it.

It may fairly be left to each nation to determine for itself the relations between the results of measurements made in its own institutions. Apart, therefore, from all other reasons, we in England would only be able to make the best use of an international comparison if we had beforehand set our own house in order, and were able at once to extend the results of experiments made at Kew or Greenwich to Stonyhurst, Valentia, and Falmouth.

This we are not at the present moment in a position to do. As far as I know nobody has ever carried a magnetometer backwards and forwards between Kew and Greenwich to test the concordance of the published results. During the recent survey single or double sets of observations have been made at Stonyhurst, Falmouth, and Valentia, with instruments which have been compared with Kew, but these measurements, though amply sufficient for the purposes of our research, were not numerous enough to serve as a firm basis for determining the discrepancies between the various standards, so that the exact relations between these important sets of apparatus are still unknown.

The first point, therefore, to which I wish to draw the attention of the Section is the necessity for a full primary comparison between the standard magnetic instruments in use at our different observatories.

But, if this were satisfactorily accomplished, the question would arise as to whether it should be repeated at regular intervals. We have at present only a presumption in favour of the view that the standards which we know are discordant are nevertheless constant. A single instance may suffice to show how necessary it may be—at all events in the case of outlying and isolated observatories—to put this belief to the test.

In the most recent account of the work of the observatory of the Bombay Government at Colába, the dips are discussed for the period of twenty years between 1872 and 1892. During this interval the adjustment of the agate plates upon which the dip needle rolls has thrice been modified. In 1877 the plates were renewed. In 1881 and 1887 the dip circle was taken to pieces and rebuilt. In the intervals the dip as determined by several needles, but always with this circle, remained approximately constant, but after each overhauling it suddenly altered, increasing by 12′ on the first occasion, by 23′ on the second, and by 20′ on the third. Mr. Chambers states that he "can give no satisfactory account of this behaviour of the instrument," but suggests that "the needle gradually hollows out a depression in the agate plates on which it rolls, and that this characteristic of the dip circle" has not before been discovered owing to the reluctance of magnetic observers to interfere with the adjustments of instruments which are apparently working well.

I do not think that this explanation will suffice. Dr. Thorpe and I employed a new dip circle in the earliest part of our survey work, which has remained in accord with Kew for ten years. During that time the dip has been measured some 700 times with it. This corresponds, I believe, to more than the amount of work done with the circle at Colába in six years, which in turn is longer than some of the intervals in which the Colába instruments gave results erroneous to the extent of 20′. I feel, therefore, quite sure that the difficulties which have been experienced at Bombay are not due to any "characteristic [defect] of the dip circle." But, whatever the cause may have been, surely the lesson is that, if such things can happen in so well-known an institution, it is desirable that we should take the moderate pains required to assure ourselves whether smaller—but, possibly, not unimportant—errors are gradually affecting the results at any of our observatories.

This brings me to my next point, namely, that if we are to draw conclusions from the minor differences between measurements of secular or diurnal change made in the observatories, it is not only necessary that we should know whether the instruments are strictly comparable and constant, but the observations must be reduced by precisely the same methods.

In 1886 the late Mr. Whipple drew the attention of the British Association to the fact that there was a systematic difference between the diurnal ranges of declination at Greenwich and Kew. His results were based on the three years 1870–72. In 1890 two of my students, Messrs. Robson and S. W. J. Smith, extended the comparison to three more recent years (1883-6-7), and obtained results in complete accord with those of Mr. Whipple.

It is well known that the average daily oscillation of the magnet is affected by the magnetic weather. Sabine showed that magnetic storms do not merely buffet the needle now in this direction and now in that—they affect its average behaviour, so that the mean swing east and west is different according as we deduce it only from days of magnetic calm or include those of storm.

Mr. Whipple reduced the Kew observations by two methods,[1] one of which depended on the calmest days only, while the other included those which were moderately disturbed. Neither agreed exactly with the method in use at Greenwich, but the difference between the results deduced from them was so small when compared with the difference between either and that obtained at Greenwich, that it seemed possible that the diurnal variations, even at these closely neighbouring places, might differ appreciably. The question whether this is so has now been answered. In 1890, at the resquest of the Kew Committee, the Astronomer Royal undertook to select early in each year five quiet days in each of the preceding twelve months. It was also agreed that, whether they adopted other methods or not, the chief English magnetic observatories should determine the diurnal variations from these days alone. The Greenwich[2] and Kew observations for 1890 have therefore been worked up in exactly the same way, with the result that the discrepancy, which had persisted for twenty years, has entirely disappeared, and that the two diurnal ranges at the two observatories are in as close accord as could be expected.

If, therefore, we may judge from a single year, the cause of the difference lay in the choice of days. Greenwich will in future give us two diurnal variations, one obtained from the most quiet days only, the other from all days except those of violent storm, and in these we shall have most valuable data for studying the mean effect of disturbances on the diurnal variation.

To this satisfactory conclusion I have only one suggestion to add. The Astronomer Royal and M. Mascart now publish for the same stormy days the photographic traces by which the history of a magnetic storm is mapped. Is it possible for Greenwich and Paris also to agree in their choice of calm days for the calculation of the diurnal variation, so that a precise similarity of method may obtain not only between the English observatories, but between England and France?

The importance of co-operation between institutions engaged on the same tasks having been illustrated, I am glad to be able to announce that another step is about to be taken in the same direction. For some years, in spite, I believe, of great financial difficulties, the Cornwall Royal Polytechnic Society has maintained a magnetic observatory at Falmouth. The results of the observations have hitherto been printed in the Journal of

[1] Sabine's and Wild's.
[2] The Greenwich observations for subsequent years have not yet been published.

the Society only, but the Royal Society has now consented to publish them in the *Proceedings*. Before long, therefore, the Kew and Falmouth records, which are already worked up in the same way, will be given to the world side by side. Is it too much to hope that this may be the first step towards the production of a British Magnetic Year Book, in which observations whose chief interest lies in their comparison, may be so published as to be easily compared?

We owe to private enterprise another advance of the same kind. The managers of the new journal *Science Progress* have made arrangements with the Kew Committee for the yearly publication of a table showing the mean annual values of the magnetic elements as determined at the various magnetic observatories of the world. It will therefore in future be possible to get a general idea of the rate of secular change in different localities without searching through a number of reports in different languages, which can only be consulted in the rooms of the few societies or institutions to which they are annually sent. The present state of our knowledge of the secular change in the magnetic elements affords indeed very strong support to the arguments I have already adduced in favour of a comparison between the instruments of our magnetic observatories.

The whole question of the cause of this phenomenon has entered on a new stage. It has long been recognised that the earth is not a simple magnet, but that there are in each hemisphere one pole or point at which the dip needle is vertical, and two foci of maximum intensity. A comparison of earlier with later magnetic observations led to the conclusion that one or both of the foci in each hemisphere is in motion, and that to this motion—however caused—the secular change in the values of the magnetic element is due. Thus the late Prof. Balfour Stewart, writing in 1883, says, "While there is no well-established evidence to show that either the pole of verticity or the centre of force to the North of America has perceptibly changed its place, there is on the other hand very strong evidence to show that we have a change of place on the part of the Siberian focus."[1] The facts in favour of this conclusion are there discussed. The arguments are based, not on the results of any actual observations near to the focus in question, but on the behaviour of the magnet at points far distant from it in Europe and Asia. The westerly march of the declination needle, which lasted in England up to 1818, and the easterly movement which has since replaced it, are connected with a supposed easterly motion of the Siberian focus, which, it is added, "there is some reason to believe . . . . has recently been reversed." In opposition, therefore, to the idea of the rotation of a magnetic focus round the geographical poles which the earlier magneticians adopted, Stewart seems to have regarded the motion of the Siberian focus as oscillatory.

A very different aspect is put upon the matter by a comparison of the magnetic maps of the world prepared by Sabine and Creak for the epochs 1840 and 1880 respectively. Captain Creak, having undertaken to report on the magnetic observations made during the voyage of the *Challenger*, supplemented them with the unrivalled wealth of recorded facts at the disposal of the Hydrographic Department of the Admiralty. He was thus able, by a comparison with Sabine's map, to trace the general course of the secular changes all over the world for forty years. The negative results may be shortly stated. There is no evidence of any motion either of magnetic pole or focus. The positive conclusions are still more curious. There are certain lines on the surface of the earth towards which in the interval under consideration the north pole of the needle was attracted. From each side the compass veered or backed towards them. Above them the north pole of dip needle moved steadily down.

There are other lines from which, as tested by compass and dip circle, a north pole was in like manner repelled. The two principal points of increasing attraction are in China and near Cape Horn; the chief points of growing repulsion are in the North of Canada and the Gulf of Guinea.

I am sure that my friend Captain Creak would be the first to urge that we should not generalise too hastily from this mode of presenting the facts, but there can be no doubt that they cannot be explained by any simple theory of a rotating or oscillating pair of poles. *Prima facie* they suggest that the secular change is due not so much to changes at the principal magnetic points, as to the waxing and waning of the forces apparently exerted by secondary lines or points of attraction or repulsion.

All down the west coast of America, close—be it noted—to one of the great lines of volcanic activity, north hemisphere magnetism has since 1840 been growing in relative importance. Near Cape Horn a weak embyronic pole is developing of the same kind as the well-known pole at the other end of the continent near Hudson's Bay. Along a line which joins Newfoundland to the Cape of Good Hope, precisely the reverse effects have been experienced; while in the Gulf of Guinea a south hemisphere pole is growing within the tropics. Of course I do not suggest that these secondary systems can ever determine the principal phenomena of terrestrial magnetism, or reverse the magnetic states of the hemispheres in which they occur. These are no doubt fixed by the rotation of the earth. I do, however, wish to emphasise the fact that they show that either secular change is due to the conjoint action of local causes, or that if some single agent such as a current system within the earth, or a change of magnetic conditions outside it, be the primary cause, the effects of this cause are modified and complicated by local peculiarities.

Mr. Henry Wilde has succeeded in representing with approximate accuracy the secular change at many points on the surface of the earth by placing two systems of currents within a globe, and imparting to the axis of one of them a motion of rotation about the polar axis of the earth. But he has had to supplement this comparatively simple arrangement by local features. He has coated the seas with thin sheet iron. The ratio between the two currents which serves to depict the secular change near the meridian of Greenwich fails in the West Indies. Thus this ingenious attempt to imitate the secular change by a simple rotation of the magnetic pole supports the view that local peculiarities play a powerful part in modifying the action of a simple first cause, if such exist. I need hardly say that I think the proper attitude of mind on this difficult subject is that of suspended judgment, but there is no doubt that recent investigation has, at all events, definitely raised the question how far secular change is either due to, or modified by, special magnetic features of different parts of the earth.

It is possible that light may be thrown upon this point by observations on a smaller scale. Assuming for the moment that the difference in the secular changes on opposite sides of the Atlantic is due to a difference of local causes, it is conceivable that similar causes, though less powerful and acting through smaller ranges, might produce similar though less obvious differences between places only a few miles apart. For testing this Greenwich and Kew are in many respects most favourably situated. Nowhere else are two first-class observatories so near together. Differences in the methods of publishing the results have made it somewhat difficult to compare them, but the late Mr. Whipple furnished me with figures for several years which made comparison easy. Without entering into details it may be sufficient to say that the declination needles at the two places do not from year to year run parallel courses. Between 1880–82 Kew outstripped its rival, between 1885 and 1889 it lost, so that the gain was rather more than compensated. The difference of the declination of the two places appears to increase and diminish through a range of five minutes of arc.

This evidence can be supplemented by other equally significant examples. No fact connected with terrestrial magnetism is more certain than that at present the rate of secular change of declination in this part of Europe increases as we go north. This is shown by a comparison of our survey with those of our predecessors fifty and thirty years ago, by M. Moureaux's results in France, and by Captain Creak's collation of previous observations. Yet, in spite of this, Stonyhurst, which is some 200 miles north of Greenwich and Kew, and should therefore outrun them, sometimes lags behind and then makes up for lost time by prodigious bounds. Between 1882 and 1886 the total secular change of declination at Stonyhurst was about 3'·5 less than that at Greenwich and Kew, whereas in the two years 1890–1892 it reached at Stonyhurst the enormous amount of 28', just doubling the corresponding alteration registered in the same time at Kew. If these fluctuations are caused by the instruments or methods of reduction, my argument in favour of frequent comparisons and uniform treatment would be much strengthened, but, apart from the inherent improbability of such large differences being due to the methods of observation,

[1] *Encyclopædia Brit.*, 9th edition Art. "Meteorology—Terrestrial Magnetism."

the probability of their physical reality is increased by the work of the magnetic survey.

The large number of observations at our disposal has enabled us to calculate the secular change in a new way, by taking the means of observations made about five years apart at numerous though not identical stations scattered over districts about 150 miles square. The result thus obtained should be free from mere local variations, but as calculated for the south-east of England for the five years 1886-91 it differs by nearly 5 from the change actually observed at Kew.

We have also determined the secular change at twenty-five stations by double sets of observations made as nearly as possible on the same spot at intervals of several years. The results must be interpreted with caution. In districts such as Scotland, where strong local disturbances are frequent, a change of a few yards in the position of the observer might introduce errors far larger than the fluctuations of secular change. But when all such changes are eliminated, when all allowance is made for the possible inaccuracy of field observations, there are outstanding variations which can hardly be due to anything but a real difference in the rate of change of the magnetic elements.

A single example will suffice. St. Leonards and Tunbridge Wells are about thirty miles apart. Both are situated on the Hastings Sand formation, and on good non-magnetic observing ground. At them, as at the stations immediately around them —Lewes, Eastbourne, Appledore, Etchingham, Heathfield, and Maidstone—the local disturbing forces are very small. All these places lie within a district about forty miles square, at no point of which has the magnet been found to deviate by 5' from the true magnetic meridian. No region could be more favourably situated for the determination of the secular change, yet according to our observations the alteration in the declination at St. Leonards in six years was practically equal to that at Tunbridge Wells in five. It is difficult to assign so great a variation to an accumulation of errors, and this is only one amongst several instances of the same kind which might be quoted.

We find, then, when we consider the earth as a whole, grave reason to question the old idea of a secular change caused by a magnetic pole or focus pursuing an orderly orbit around the geographical axis of the earth, or oscillating in some regular period in its neighbourhood. It would, of course, be absurd to admit the possibility of change in the tropics and to deny that possibility in the arctic circle, but the new facts lead us to look upon the earth not as magnetically inert, but as itself—at the equator as well as at the pole—producing or profoundly modifying the influences which give rise to secular change. And then, when we push our inquiry further, accumulating experience tells the same tale. The earth seems as it were alive with magnetic forces, be they due to electric currents or to variations in the state of magnetised matter. We need not now consider the sudden jerks which disturb the diurnal sweep of the magnet, which are simultaneous at places far apart, and probably originate in causes outside our globe. But the slower secular change, of which the small part that has been observed has taken centuries to accomplish, is apparently also interfered with by some slower agency the action of which is confined within narrow limits of space. Between Kew, Greenwich, and Stonyhurst, between St. Leonards and Tunbridge Wells, and I may add between Mablethorpe and Lincoln, Enniskillen and Sligo, Charleville and Bantry, the measured differences of secular variation are so large as to suggest that we are dealing not with an unruffled tide of change, which, unaltered by its passage over continent or ocean, sweeps slowly round the earth, but with a current fed by local springs or impeded by local obstacles, furrowed on the surface by billows and eddies, from which the magnetician, if he will but study them, may learn much as to the position and meaning of the deeps and the shallows below. But if this is the view which the facts I have quoted suggest, much remains to be done before it can be finally accepted; and in the first place—to come back to the point from which I started—we want, for some years at all events, a systematic and repeated comparison of the standard instruments in use at the different observatories. That they are not in accord is certain; whether the relations between them are constant or variable is doubtful. If constant, the suggestions I have outlined are probably correct; if variable, then the whole or part of the apparent fluctuations of secular change may be nothing more than the irregular shiftings of inconstant standards.

I cannot myself believe that this is the true explanation, but in any case it is important that the doubt should be set at rest, and that if the apparent fluctuations of secular change are not merely instrumental, the inquiry as to their cause should be undertaken in good earnest.

The question is interesting from another point of view. It is now fully established that even where the surface soil is non-magnetic, and even where geologists have every reason to believe that it lies upon non-magnetic strata of great thickness, there are clearly-defined lines and centres towards which the north-seeking pole of a magnet is attracted, or from which it is repelled. To the magnetic surveyor fluctuations in secular change would appear as variations in the positions of these lines, or as changes in the forces in play in their neighbourhood.

Greenwich and Kew are both under the influence of a widespread local disturbance which culminates near Reading. At both places the needle is deviated to the west of the normal magnetic meridian, and if the westerly declination diminishes sometimes faster and sometimes more slowly at one observatory than at the other, this must be, or, at all events, would in the first instance appear to be, due to local changes in the regional disturbing forces. The questions of the nature of the irregularities of secular change and of the causes of local disturbances are therefore intermingled; and information gained on these points may in turn be useful in solving the more difficult problem of world-wide secular variations.

Two causes of regional and local disturbances have been suggested, viz. earth currents, and the presence of visible or concealed magnetic rocks. The two theories are not mutually exclusive. Both causes of the observed effects may, and probably do, coexist. I have, however, elsewhere explained my reasons for believing that the presence of magnetic matter, magnetised by induction in the earth's field, is the principal cause of the existence of the magnetic ridge-lines and foci of attraction which for so many years we have been carefully tracing. I will only now mention what appears to me to be the final and conclusive argument, which, since it was first enunciated, has been strengthened by the results of our more recent work. We find that every great mass of basic rock, by which the needle is affected at considerable distances, attracts the north-seeking pole. Captain Creak some years ago showed that the same statement is true of those islands in the northern hemisphere which disturb the lines of equal declination, while islands in the southern hemisphere repel the north pole and attract the south. In other words, these disturbances are immediately explained if we suppose that they are due to magnetic matter magnetised by induction. The theory of earth currents would, on the other hand, require that round the masses of visible basalt, and round the island investigated by Captain Creak, currents, or eddies in currents, should circulate in directions which are always the same in the same hemisphere, and always opposed on opposite sides of the equator. For this supposition no satisfactory explanation is forthcoming, and, therefore, with all reserve and a full consciousness that in such matters hypothesis differs but little from speculation, it appears to me that the theory that induced magnetism is the main cause of the disturbance has the greater weight of evidence in its favour.

If this be granted, it is evident that the positions of the main lines and centres of attraction would be approximately constant, and, so far as it is possible to form an opinion, these conditions seem to be satisfied. There has certainly been no noticeable change in the chief loci of attraction in the five years which have elapsed between the epochs of our two surveys. Mr. Welsh's observations made in Scotland in 1857-8 fit in well with our own. Such evidence is not, however, inconsistent with minor changes, and it is certain that as the directions and magnitude of the inducing forces alter, the disturbing induced forces must alter also. But this change would be slow, and as the horizontal force is in these latitudes comparatively weak, the change in the disturbing forces would also be small, unless the vertical force altered greatly. It is at all events impossible to attribute to this cause oscillations which occupy at most eight or ten years. It is possible to suggest other changes in the state of the concealed magnetic matter—alterations of pressure, temperature, and the like—to which the oscillations of secular change might be due, but probably there will be a general consensus of opinion that if the slowly changing terms in the disturbance function are due to magnetic matter, the more rapid fluctuations of a few years' period are more likely to be connected with earth currents. It becomes, therefore, a matter of interest to disentangle the two constituents of local disturbances; and there is one question to which I think an answer might be obtained without a greater expenditure than the importance of the investigation warrants. Are the local variations in secular change waves which move from place to place, or are they stationary fluctuations, each of which is confined to a limited area beyond which it never travels? Thus, if the annual decrease in the declination is at one time more rapid at Greenwich than at Kew, and five years afterwards more rapid at Kew than at Greenwich, has the maximum of rapidity passed in the interval through all intervening places, or has there been a dividing line of no change which has separated two districts which have perhaps been the scenes of independent variations? The answer to this question is, I take it, outside the range of our knowledge now, but if the declination could be determined several times annually at each of a limited number of stations in the neighbourhood of London, to this inquiry, at all events, a definite answer would soon be furnished.

There are two other lines of investigation which I hope will be taken up sooner or later, for one of which it is doubtful whether the United Kingdom is the best site, while the other is of uncertain issue.

If, however, it be granted that the principal cause of local and regional magnetic disturbances is the magnetisation by the earth's field of magnetic matter concealed below its surface, the question as to the nature of this material still remains to be solved. Is it virgin iron or pure magnetite, or is it merely a magnetic rock of the same nature and properties as the basalts which are found in Skye and Mull? There is, of course, no *à priori* reason why all these different materials should not be active, some in one place and some in another.

As regards the United Kingdom I have, both in a paper on the Permeability of Magnetic Rocks and in the description of the recent survey, made calculations which tend to prove that, if we suppose that the temperature of the interior of the earth is, at a depth of twelve miles, such as to deprive matter of its magnetic properties, and if we further make the unfavourable assumption that down to that limit the susceptibility is constant, the forces which are observed on the surface are of the same order of magnitude as those which could be produced by large masses of ordinary basalt or gabbro. It would not, however, be wise to generalise this result, and to assume that in all places regional disturbances are due to basic rocks alone.

We know that local effects are produced by iron ore, for the Swedish miners seek for iron with the aid of the magnet, and in some other cases magnetic disturbances of considerable range are so intense as to suggest that material of very high magnetic permeability must be present.

If the concealed magnetic matter were iron, and if it were present in large quantity, it is evident that the results of experiments with the magnetometer and dip circle might be supplemented by observations made with the plumb-line or pendulum. In such a case the region of magnetic disturbance would also be a region of abnormal gravitational attraction. An account of a suggested connection between anomalies of these two kinds occurring in the same district has lately been published by Dr. Fritsche.[1]

Observations made about thirty years ago by a former director of the Astronomical Observatory in Moscow led to the conclusion that throughout two large districts to the north and south of that city the plumb-line is deviated in opposite directions. The deflections from the vertical are very considerable, and indicate a relative defect in the attraction exerted by the rocks in the neighbourhood of Moscow itself, and the suggestion has been made that there is either a huge cavity—a bubble in the earth-crust—under the town, or that the matter beneath it is less dense than that which underlies the surface strata on either side at a distance of ten or twelve miles.

As long ago as 1853, Captain Meyen made magnetic observations in order to determine whether the same district is also the seat of any magnetic irregularity. His stations were hardly sufficiently numerous to lead to decisive results, but the magnetic elements have recently been measured by Dr. Fritsche at thirty-one places within fifty miles of Moscow. The experiments were all made within eleven days, so that no correction for secular change is required. They indicate a locus of magnetic attraction running through Moscow itself. South of the town the disturbance again changes in direction so as to show either that repulsive forces are in play, or that there is another magnetic ridge line still further to the south. Dr. Fritsche thinks that these observations explain the gravitational anomalies without recourse to the somewhat forced hypothesis of a vast subterranean cave. He assumes that there is a concealed mass of iron, which approaches near to the surface at Moscow, and also along two loci to the south and north of the city. He attributes the magnetic irregularities to the attraction of the central iron hill, the deflections of the plumb-line to the flanking masses. It is perhaps not inconceivable that such results might follow in a special case, but without the support of calculation it certainly appears that the magnetic experiments point to the existence of the principal attracting mass under the town. This is in fact the arrangement shown in the figure with which Dr. Fritsche illustrates his hypothesis. If this is so, the theory would *prima facie* seem to require that the bob of a plumb-line should be attracted towards and not—as is actually the case—away from the centre of the magnetic disturbance. On the whole, then, though the coexistence of large magnetic and gravitational disturbances in the same place is suggestive, I do not think that they have as yet been proved to be different effects of the same hidden mass of magnetic matter.

[1] "Die magnetischen Localabweichungen bei Moskau und ihre Beziehungen zur dortigen Local-Attraction," *Bulletin de la Société Impér. des Naturalistes de Moscou*, 1894, No. iv.

In a few weeks an International Geodetic Conference will meet at Innsbruck, at which the Royal Society will be represented. It is, I believe, intended to extend the detailed investigation of the relations between the nature of the earth's crust and the gravitational and magnetic forces to which it gives rise. We may therefore hope that special attention will before long be given to localities where both may combine to give information as to facts outside the range of the ordinary methods of geology.

The second phenomenon on which more light is desirable, is the permanent magnetisation of magnetic rocks. It is known that fragments of these are strongly but irregularly magnetised, but that the effect of very large masses at a distance appears to be due to induced rather than to permanent magnetism. There are three questions to which I should like an answer. Are underground masses of magnetite ever permanently magnetised? Are large areas of surface masses, say a few hundred square yards in extent, ever permanently and approximately uniformly magnetised in the same sense? Is there any relation between the geological age and the direction of the permanent magnetism of magnetic rocks?

Inquiries such as these can only be taken up by individual workers, but I venture to think that the comparison of the observatory instruments and the fluctuations of secular change outside the observatories could best be investigated under the auspices of a great scientific society. The co-operation of the authorities of the observatories will no doubt be secured, but it is most important that the comparison should in all cases be made with one set of instruments, and by the same methods. Whether the British Association, which for so long managed a magnetic observatory, may think that it could usefully inaugurate the work, it would be improper for me in a presidential address to forecast. Who does it is of less importance than that it should be done, and I cannot but hope that the arguments and instances which I have to-day adduced may help to bring about not only the doing of the work, but the doing of it quickly.

SECTION B.

CHEMISTRY.

OPENING ADDRESS BY PROF. H. B. DIXON, M.A., F.R.S., PRESIDENT OF THE SECTION.

"An Oxford School of Chemists."

IT has been said, and no doubt with truth, that few Presidents of Sections start writing an address without referring to that of their predecessor who held office on the last occasion when the Association met in the same city. By such reference each new President gains the advantage of many points of perspective and contrast; for in the interval a generation of workers has passed away, and the last new thing of the old meeting is the ancient instance of to-day. In the present case I turned to the Report of 1860 with a lively hope of drawing inspiration from it; for my predecessor at the last Oxford meeting was no less a master of experiment and expression than the late Prof. Brodie. Judge of my disappointment when I found that Brodie had written no address at all. Whether that great man, knowing there were better things to do here than listen to addresses, had the courage to make an innovation he thought desirable in itself, or whether, as others say, he was but obeying the etiquette of the Oxford professoriate—the fact remains the assembled chemists went away unaddressed, and the natural spring of inspiration for the address of 1894 is found dry at its source. Of course you will say, "Why do you not follow such a good example?" I wish I had the courage. As it is, I can but urge the vacuum of 1860 as some excuse for the emptiness of the address I now present—compelled to do so partly by the force of fashion and the demands of the assistant general secretary, and (shall I add?) partly by the gratification of holding forth, with a little brief authority, in my old academic home, endeared to me personally by so many happy memories, and hallowed in the minds of chemists by the traditions of such great achievements in the science we pursue.

I say *traditions* advisedly, for the chemical achievements spoken of were largely forgotten, or put on one side as guesses and half-truths. No chemist here will need reminding that I refer to *the first school of scientific chemistry*, the school founded two centuries and a half ago by Robert Boyle with his disciples Hooke and Mayow—a group whom I will venture to call "the Oxford school of chemists." And now that chemists are met together once more in Oxford it seemed to me not inappropriate for us to consider what this school of chemists accomplished, and wherein it failed, what led to the sudden growth and what to the decline of chemical investigation here, and what lessons for modern Oxford may be read in the history of that rise and fall.

The intellectual awakening which followed the re-discovery of the ancient world of literature gave rise to the scientific interrogation of nature. In Italy first, and then in France, England, and in Germany, the diffusion of classical learning broke down the ancient barriers of restraint, and developed a spirit of free inquiry. It was not so much that ignorance had to be dispelled, but that the right of search had to be established. Here and there during the middle ages some man of genius had arisen—learned beyond all his contemporaries, intrepid in the pursuit of truth—only to be crushed by a political and mental despotism. The name of Roger Bacon arises at once in our thoughts, who from his Oxford cell sent forth that great appeal for experimental science that nearly converted a Pope of Rome and won three centuries for intellectual freedom. But his labour bore no fruit. I know no better index to the dominant sentiment of the time than the following words from a papal rescript reproving the members of an Italian university for scientific presumption: "They must be content with the landmarks of science already fixed by their fathers, and have due fear of the curse pronounced against him who removeth his neighbour's landmark." Under such conditions no wonder philosophy was at a standstill. "The same knots were tied and untied; the same clouds were formed and dissipated."[1] The cramped philosophy of the middle ages had in alchemy a fitting colleague—with its mysticism, its sordid ideals, its trickery, and its arrogance. The revival of learning was thus an emancipation of the mind, and in the new freedom the sciences of mechanics, physics, and chemistry arose. The first necessity for progress was enlightenment, the second was experiment; in the year that Francis Bacon died Robert Boyle was born.

The common pursuit of experimental inquiry and the need for constant criticism and discussion among its followers led to the foundation of scientific societies. Such societies, which have greatly influenced the progress of knowledge, sprang up in Florence and Padua, in Paris and Oxford—wherever, among bodies of learned men, some were found in sympathy with natural philosophy. Among these associations the Philosophical Society of Oxford has played no unimportant part, and, however much Oxford may have undervalued its work, for one thing all chemists are grateful, and Oxford herself may feel proud—that here, under her influence, first grew up the idea that chemistry was no mere drudge of medicine, or genii of the alchemist, but a science to be studied purely for itself.

The origin of this Oxford Society has been well told by Dr. Wallis, one of its founders:—

"About the year 1645, while I lived in London (at a time when, by our civil wars, academic studies were much interrupted at both Universities), besides the conversation of eminent divines, I had the opportunity of being acquainted with divers worthy persons inquisitive into natural philosophy, and particularly of what hath been called experimental philosophy. We did by agreements meet weekly in London to treat and discourse of such affairs; of which number were Dr. John Wilkins, Dr. Jonathan Goddard, Dr. Ent, Dr. Merret, Mr. Samuel Foster, then Professor of Astronomy in Gresham College, and Mr. Theodore Haak, and many others.

"These meetings we held sometimes at Dr. Goddard's lodgings, on occasion of his keeping an operator at his house for grinding glasses for telescopes and microscopes; sometimes at a convenient place in Cheapside, and sometimes at Gresham College. Our business was (precluding matters of theology and State affairs) to discourse and consider of philosophical inquiries. . . . . About the year 1648, some of our company being removed to Oxford (first Dr. Wilkins, then I, and soon after Dr Goddard), our company divided. Those in London continued to meet there as before, and those of us at Oxford, with Dr. Seth Ward (since Bishop of Salisbury), Dr. Ralph Bathurst, President of Trinity College, Dr. Petty, Dr. Willis (an eminent physician in Oxford), and divers others, continued such meetings in Oxford, and brought those studies into fashion there, meeting first at Dr. Petty's lodgings (in an apothecarie's house), because of the convenience of inspecting drugs, and, after his removal, at the lodgings of Dr. Wilkins, then Warden of Wadham College, and, after his removal, at the lodgings of the Honourable Mr. Robert Boyle, then resident for divers years in Oxford."

Robert Boyle, the youngest child of the great Earl of Cork, was born at Lismore in 1626. His mother died when he was a child. Always delicate, he was sent at twelve years of age with a tutor to the Continent; he remained abroad for six years. He studied chiefly at Geneva and at Florence, where he read the works of Galileo. Returning to England, in 1644, he busied himself with chemistry at Stalbridge, a manor in Dorsetshire left him by his father. On his visits to London he became one of the members of the "Invisible College," the germ of the Royal Society. "Vulcan has so bewitched me," he writes at the age of twenty-three, "as to make me fancy my laboratory a kind of elysium."

Drawn to Oxford in 1654, Boyle spent here the most active years of his life in experimental research. Of Boyle's scientific writings much has been said in extravagant praise and much in ridicule. Boerhaave wrote: "To him we owe the secrets of fire, air, water, animals, vegetables, and fossils." This phrase is not more grotesque than that of a recent writer, who says, "Boyle's name is identified with no great discovery." Dr. Johnson has very justly remarked, in a number of the *Rambler*: "It is well known how much of our philosophy is derived from Boyle's discoveries, yet very few have read the details of his experiments. His name is indeed reverenced, but his works are neglected." It is, indeed, rather hard to read through one of Boyle's papers, even in the abridged form. Though clear, they are discursive. The writer cannot rid himself entirely of the essences and qualities of the alchemists; and it is only when we compare these records with the works of Van Helmont, his immediate predecessor, that we recognise the enormous advance that has been made by Boyle. I must pass over his physical work on the elasticity of the air. It must suffice to say that he established by most careful experiment the law which is known by his name—that the volume of a given mass of air varies inversely as the pressure upon it. He determined the density of the air, and pointed out that bodies altered in weight according to the varying buoyancy of the atmosphere. One of his most important chemical papers—certainly the one most frequently cited—is "The Sceptical Chemist," published anonymously in 1661. I will attempt the briefest account of it. The opening words of the dialogue strike the keynote of the whole:—

"Notwithstanding the subtle reasonings of the Peripatetics and the pretty experiments of the Chymists, I am so diffident as to think that, if neither can produce more cogent arguments than are usually given, a man may reasonably doubt as to the number of those material ingredients of mixed bodies which some call elements and others principles." He proceeds, through the mouth of one of the supposed disputants, to attack the doctrine of the three elements, the *tria prima* of the alchemists—sulphur, mercury, and salt. "There are some bodies," he says, "from which it has not yet been made to appear that any degree of fire can separate either salt, or

[1] Whewell, "Hist. of Ind. Sci."

sulphur, or mercury, much less all the three. Gold is the most obvious instance. It may be heated for months in a furnace without losing weight or altering in character, and yet one of its supposed constituents is volatile and another combustible. Neither can water or solvents separate any of the three principles from gold; the metal may be *added to*, and so brought into solution and into crystalline compounds, but the gold particles are present all the time; and the metal may be reduced to the same weight of yellow, ponderous, malleable substance it was before its mixture." He points out the confusion which earlier chemists had made between calcination in the open air and distillation in retorts; he shows that in compounds, *e.g.* copper nitrate, the particles retain their nature, although disguised, in the combination, for the nitric acid may be separated by heat, the copper by precipitation. But the sceptical chemist, though pouring ridicule on the *tria prima*, could not but admit the power of water to produce organic substances. He quotes Van Helmont's famous experiment of growing a shoot of willow in baked earth moistened with distilled water, and he repeats the experiment in various forms. Ignorant of the existence of carbonic acid in the air (discovered a century later by Black), he is driven to conclude that the plant is fashioned out of the pure water. But he rejects the doctrine—as old as Thales and as modern as Van Helmont—that water is the foundation of all things. M. de Rochas had published a remarkable experiment on water. By artificial heat, by graduations of coagulations and congelations, he had turned it into earth which produced animals, vegetables, and minerals. The minerals began to grow and increase, and were composed of much salt, little sulphur, and less mercury; the animals moved and ate, and were composed of much sulphur, little mercury, and less salt. "I have some suspicions," says Boyle, "concerning this strange relation; though as for the generation of living creatures, both vegetable and sensitive, it need not seem incredible, since we find that our common water, which is often impregnated with a variety of seeds, long kept in a quiet place, will putrefy, and then, too, produce moss and little worms according to the nature of the seeds that were lurking in it."

I will give two short quotations from the "Sceptical Chemist," which show the author at his best and his worst. In the first he is discussing the nature of chemical combination between elementary particles: "There are clusters wherein the particles stick not so close together, but they may meet with corpuscles of another denomination, disposed to be more closely united with some of them than they were among themselves; and in such case two corpuscles thus combining, losing that shape, size, or motion upon whose account they exhibited such a determinate quality, each of them really ceases to be a corpuscle of the same denomination as it was before; and from the coalition of these there may result a new body, as really one as either of the corpuscles before they were confounded. . . . If you dissolve minium in good spirit of vinegar and crystallise the solution, you shall not only have a saccharine salt exceedingly different from both its ingredients, but the union is so strict that the spirit of vinegar seems to be destroyed . . . for there is no sourness at all, but an admirable sweetness to be tasted in the concretion." In this passage we can distinctly see the germ of the modern theory of chemical affinity uniting atoms into chemical compounds. In the second quotation Boyle is arguing that fire is not only an analyser of mixtures, but compounds the ingredients of bodies after a new manner; mercury, for instance, may be turned into a liquid, from which the mercury cannot be reduced again, and consequently is more than a "disguise" of it. "Two friends of mine," he says, "both of them persons of unsuspected credit, have solemnly assured me that after many trials they made to reduce mercury into water, they once, by several cohobations, reduced a pound of quicksilver into almost a pound of water, and this without the addition of any substance, but only by urging the mercury with a fire skilfully managed. Hence it appears that by means of fire we may obtain from a mixed body what did not pre-exist therein." Boyle has sometimes been charged with credulity, and chemists who know how mercury has a way of disappearing without leaving even its weight of water behind will smile to hear that the persons of unsuspected credit responsible for this experiment were "the one a physician, the other a distinguished mathematician."

Boyle's writings contain the record of numerous important chemical observations, *e.g.* the synthesis of nitre, and the preparation of nitric acid by the distillation of nitre with oil of vitriol. He discovered several of the delicate tests we still use, *e.g.* solution of ammonia as a test for copper, silver nitrate as a test for chlorides, gallic acid as a test for iron. But I wish especially to refer to the work done by Boyle on the air and its relation to combustion. The air, according to him, was composed of three different kinds of particles: (1) exhalations from water and animals; (2) a very subtle emanation from the earth's magnetism, which produces the sensation of light; and (3) a fluid compressible and dilatable, having weight, and able to refract light. It is this third portion of air which plays an active part in many chemical operations. Like Van Helmont, Boyle recognised differences in gases, but did not distinguish them as being something different in kind from air. He prepared hydrogen by the action of hydrochloric and sulphuric acids on iron, but his chief concern was to show that the new gas was compressible and was dilatable by heat; in other words, that it was really *air*. His observations are worth quoting; they contain, I believe, the first undoubted description of hydrogen, and the first method devised for collecting and examining freshly prepared gases.

"Having provided a saline spirit . . . exceedingly sharp and piercing, we put into a vial a convenient quantity of filings of steel, purposely filed from a piece of good steel. This metalline powder being moistened with the menstruum was afterwards drenched with more, whereupon the mixture grew very hot, and belched up copious and stinking fumes. . . . Whencesoever this stinking smoak proceeded, so inflammable was it, that upon the approach of a lighted candle it would readily enough take fire, and burn with a blewish and somewhat greenish flame at the mouth of the viol; and that, though with little light, yet with more strength than one would easily suspect."[1]

And again: "We took a clear glass vial, capable of containing three ounces of water, with a long cylindrical neck; this we filled with oil of vitriol, and fair water, of each a like quantity, and casting in six small iron nails we stopped the mouth of the glass, and speedily inverting it, we put the neck of it into a wide-mouthed glass with more of the same liquor in it. . . . And soon after we perceived the bubbles, produced by the action of the menstruum upon the metal, ascending in swarms; by degrees they depressed the liquor till, at length, the substance contained in these bubbles possessed the whole cavity of the vial. And for three or four days and nights together the cavity of the glass was possessed by the air, since by its spring it was able for so long a time to hinder the liquor from regaining its former place. Just before we took the vial out of the other glass, upon the application of the warm hand to the convex part of the glass, the imprisoned substance readily dilated itself like air, and broke through the liquor, in several succeeding bubbles."

The importance of this experiment will be evident when we consider that Van Helmont had declared that gases could be made artificially in many ways, but could not be caught and held in vessels.[2]

Armed with the air-pump which he had so greatly improved, Boyle in 1660 began many experiments on combustion, which he afterwards published under the title "New Experiments touching the Relation betwixt Flame and Air." In these researches he shows that sulphur will not burn when the air is removed. The sulphur was lowered on to a hot iron plate in a receiver made vacuous by the pump; it smoked, but did not ignite. On allowing a little air to enter "divers little flashes could be seen": these were extinguished on sucking out the air again. A candle flame and a hydrogen flame under a receiver were gradually extinguished when the air was pumped away. On the other hand, on dropping gunpowder on to a hot iron plate *in vacuo* there appeared "a broad blue flame like that of brimstone, which lasted so very long we could not but wonder at it"; and fulminating gold detonated *in vacuo* when heated by a burning glass, or when dropped on heated iron. Gunpowder also he found to burn under water. He is driven to the conclusion "that flame may exist without air." But it may be supposed that air is mechanically enclosed in the crystals of nitre—"in its very formation the corpuscles may intercept store of little aereal particles. . . . According to this surmise,

1 "On the Difficulty of preserving Flame without Air," 1672.

2 "Gas, vasis incoercibile, foras in aerem prorumpit."—*Ortus Medicinæ.* The epithet "sylvestre" was applied by Van Helmont to all artificially prepared gases. He meant by it "untameable" and "non-condensible"—"quod in corpus cogi non potest visibile."

though our mixture burns under water, yet it does not burn without air, being supplied with enough to serve the turn by the numerous eruptions of the aereal particles of the dissipated nitre." However, he "removes this suspicion" by obtaining nitre crystallised *in vacuo*. He then suggests the possibility of the nitre supplying "vehemently agitated vapours" which are no true air, but being exceedingly rarefied by the fire "emulate air." Boyle never grasped the true function of air in combustion. From his later experiments on the calcination of metals he drew the same conclusion that we find in the "Sceptical Chemist," namely, that igneous particles combine with other corpuscles to form new bodies. And yet he saw there was a real connection between air and fire. In his tract on Artificial Phosphori, Boyle showed that a piece of phosphorus sealed up in a glass vessel gradually lost its light. "It seems," he wrote, "that the air included with the phosphorus either had some vital substance preyed upon thereby, or else was tamed by the fumes of the phosphorus and rendered at length unfit to continue the particular flame of our noctiluca."

The genius of Robert Hooke was in sharp contrast with that of Boyle. Quick, restless, imaginative, he sprang from discovery to discovery. With extraordinary acuteness and powers of invention, he lacked the steady purpose of Boyle, the calm judgment and completeness of Newton—his two great scientific contemporaries. It might be said of Hooke, as was said of a great poet, he touched nothing he did not strike fire from; and some would add that his touch had the same effect on persons as on things. We can hardly name a discovery of this age which Hooke had not in part anticipated and claimed as his own. Like a prospector in a newly discovered mining district, he hurried from spot to spot, pegging in his claims and promising to return to work out the ore. And what rich lodes he struck! The particular claim we are concerned with here is the discovery of the relation between air and flame. In 1665 Hooke published in the "Micrographia" a description of flame and the phenomena of combution which in my judgment has never been surpassed. How far he was indebted to Boyle will appear directly.

Born in 1635, Hooke spent five years at Westminster School, then under Dr. Busby, and proceeded to Christ Church in 1653. At school and college it is related of him that he devoted his time to designing flying machines. These mechanical inventions attracted the notice of Dr. Wilkins, Warden of Wadham, and a leading member of the Philosophical Society. This led to his introduction to Dr. Willis, to whom he became assistant in chemistry and natural philosophy. Willis recommended him to Boyle, whose assistant he became. His first work in Boyle's laboratory was the construction of the improved air-pump. In 1662 Boyle obtained for him the position of curator of experiments in the London Society, soon to be known as the Royal Society. Hooke was thus Boyle's assistant when those experiments on combustion I have described were being carried on. Among other experiments made by Boyle were some on the distillation of wood in retorts.

"Having sometimes distilled such woods as box, whilst our *caput mortuum* [*i.e.* the residue] remained in the retort it continued black like charcoal, though the retort were kept red hot in a vehement fire; but as soon as ever it was brought out of that vessel into the open air the burning coals would degenerate or fall asunder into pure white ashes."[1] Hooke saw the experiment and a new light flashed on him. "From the experiment of charring coals," he writes "(whereby we see that, notwithstanding the great heat, the solid parts of the wood remain, whilst they are preserved from the free access of the air, undissipated) we may learn that which has not been published or hinted, nay, not such much as thought of by any; and that in short is this:—

"That the air is the universal dissolvent of all sulphurous [*i.e.* combustible] bodies. . . .

"That this action of dissolution produces a very great heat, and that which we call fire.

"That this action is performed with so great a violence, and does so rapidly agitate the smallest parts of the combustible matter, that it produces in the diaphanous medium of the air the action, or pulse of *Light*.

"That this dissolution is made by a substance inherent and mixed with the air, that is like, if not the very same with, that which is mixed in saltpetre.

"That the dissolving parts of the air are but few . . . whereas saltpetre is a menstruum . . . that abounds more with these dissolvent particles.

"It seems reasonable to think that there is no such thing as an element of fire, . . . but that that shining transient body which we call flame is nothing else but a mixture of air and volatile parts of combustible bodies, which are acting upon one another whilst they ascend; which action . . . does further rarifie those parts that are acting or are very near them, whereby they, growing very much lighter than the heavy parts of that *menstruum* they are more remote, are thereby protruded and driven upwards."

Hooke quotes no other experiments in support of his theory of flame. He states that he has made many; he has, however, only time "to hint an hypothesis," which, if he is permitted opportunity, he will "prosecute, improve, and publish." Some years later he returned to his subject of flame in his tract called "Lampas," published in 1677. "The flame, as I formerly proved, being nothing but the parts of the oyl rarified and raised by heat into the form of a vapour or smoak, the free air that encompasseth this vapour keepeth it into a cylindrical form, and by its dissolving property preyeth upon those parts of it that are outwards, . . . producing the light which we observe; but those parts which rise from the wick which are in the middle are not turned to shining flame till they rise towards the top of the cone, where the free air can reach and so dissolve them. With the help of a piece of glass anyone will plainly perceive that all the middle of the cone of flame neither shines nor burns, but only the outward superficies thereof that is contiguous to the free and unsatiated air."

What is practically the same theory of flame was worked out experimentally by John Mayow, Fellow of All Souls: this was published a few years after the "Micrographia."

But Mayow went further, and distinctly showed the dual nature of the air. One constituent of air, the nitre air, is concerned in respiration and combustion; the other will neither support flame nor animal life. The ideas, the names, proposed by Hooke and Mayow are so exactly similar that it is impossible to imagine that the work was done independently. The two were working at the same time at Oxford, and Mayow, having been an undergraduate at Wadham under Dr. Wilkins, became the pupil of Willis. Yet Mayow nowhere mentions Hooke's name. A writer in the "Dictionary of National Biography"[1] has shrewdly observed that Hooke has brought no charge of plagiarism against Mayow, and even proposed him for the Royal Society four years after the publication of the "Five Tracts." Knowing what we do of Hooke's jealousy, it seems exceedingly unlikely that Mayow was merely working out Hooke's ideas. It seems to me probable that Hooke and Mayow worked together under Boyle between 1660 and 1662; that in Boyle's laboratory they saw and assisted in the experiments which led them jointly to their theory; that Hooke, busy with other work in London, published the hypothesis in 1665 without further verification: and that Mayow in Oxford systematically worked through the experiments on which he based his conclusions.

Let me briefly show what the experiments were on which Mayow relied. Combustible bodies will not burn in the vacuous receiver of Boyle's air-pump; they will burn *in vacuo* or under water when mixed with nitre. There is, therefore, something common to air and to nitre which causes combustion. The fiery particles in air and in nitre both form oil of vitriol by their union with sulphur; they both form iron vitriol by their union with pyrites. Rust of iron is produced both by the air and by acid of nitre; the acids of sugar and honey are formed, and wine is soured in the same way. The nitre-air (spiritus nitro-aereus), the supporter of combustion and the acid producer, is therefore the same chemical substance whether it exist in the gaseous form in air or is condensed in saltpetre.

Mayow heated a weighed quantity of antimony by means of a burning glass, and found it increased in weight during the calcination;[2] the calcined antimony, he adds, has the same properties as the body prepared by heating antimony with nitric acid; it is impossible to conceive, he says, whence the increase in weight arises except by the fixation of the particles of nitre-air during the heating.

The nitre-air does not make up the whole of the air, but only its more active and subtle part, for a candle under a glass will

1 "The Sceptical Chemist."

1 Mr. P. J. Hartog.

2 This experiment seems to have been first described by Poppius, *Basilica Antimonii*, 1625.

cease to burn while there is still plenty of air left. The experiment by which Mayow shows this is so important that I will quote his words:—

"Let a lighted candle be so placed in water that the burning wick shall rise about six fingers' breadth above the water; then let a glass vessel of sufficient height be inverted over the candle. Care must be taken that the surface of the water within the glass shall be equal in height to that without, which may be done by including one leg of a bent syphon within the vessel while the other opens outside. The object of the syphon is that the air, enclosed by the vessel and compressed by its immersion into the water, may escape through the hollow syphon. When the air ceases to issue, the syphon is immediately withdrawn, so that no air can afterwards get into the glass. In a short time you will see the water gradually rising into the vessel while the candle still burns."

In other experiments he burnt camphor and sulphur supported on a shelf in the inverted vessel. The water rose, he says, because, owing to the disappearance of the fire-air, the air left could not resist the pressure of the atmosphere outside. When the combustibles were extinguished it was impossible to kindle them again by means of the sun's rays concentrated on them by a burning glass. The residual air was no more able to support combustion than the vacuum of Boyle's engine. Again, the respiration of animals in the closed space was shown to diminish the air, and to render it incapable of supporting combustion; the fire-air was as necessary for life as for flame. The larger portion of the air was something entirely different from fire-air, and incapable of supporting life or combustion. I believe this to be the first definite statement founded on experiment that the air is composed of two distinct gases.

I have given the fundamental facts in chemistry we owe to Mayow; the limits of his work are sufficiently obvious. He detected the existence of what we call oxygen gas in the air, and demonstrated some of its most remarkable properties. He did not isolate the gas, or show what became of it in combustion; he did not always distinguish between the gas itself and the heat produced by its action. But the advance he made was extraordinary—not so much in the conclusions he drew as in the experiments and arguments he founded them on. Compare him for a moment with another writer who had previously expressed similar views concerning the calcination of metals. Jean Rey, of Perigourd, a witty and shrewd physician, published in 1630 a series of essays attributing the increase in weight of metals on calcination to the fixation of the air. "When asked," he writes, "why tin and lead increase in weight on calcination, I reply and gloriously maintain that this increase comes from the air, which is thickened and made heavy and adhesive by the long and continued heat of the furnace. This air mingles with the calx and attaches itself to the smallest particles." The reply is good, but the reasons that gloriously maintain it are not altogether conclusive. I can only give two of them: (1) *The air has weight.*—This is shown by the increase in velocity of heavy bodies falling to the earth, because as the body approaches the earth it subtends a wider angle from the centre of the earth, and receives more shocks from the particles of air. Again, although the air appears to weigh nothing on the balance, this is because we weigh it in the air; it loses its weight, just as water weighs nothing in water. Fire has weight too, and should we ever find ourselves in a region where fire is the predominant element, we shall be able to prove the statement in the same way. (2) *Fire can thicken and make air heavy.*—Stand a cannon upright and put a red-hot ball into it. You must admit that the air in the gun is so small in quantity that it will be heated to the same temperature as the ball. Nevertheless you can hold your hand in the mouth of the gun at first, but in a short time you cannot do so. Not that the air has got hotter, it is cooling all the time; it is because the air is thickened. Now if you drop a fleece of wool into the mouth, it will not descend, and if you push it in, it will come up again, proving the air is heavier. Lastly, the air is seen to tremble over the mouth of the gun, and objects seen through it are blurred. This is due to the thickening, it cannot be due to a motion of the air; "for I see," he says. "a lady's beauty quite distinctly through the air she flutters with her fan."

From what has been stated it will be clear that the Oxford School of Chemistry was a *school of research.* Boyle gave no instruction in the ordinary sense; and, indeed, had no official connection with the University. But that he thought instruction in chemistry should be given in the University is obvious from the fact that he brought over a chemist from Strasburg, and set him up as a lecturer with rooms next his own and the use of his laboratory. Of these lectures we find a quaint account in Anthony Wood's diary:—

"An. Dom. 1663.

"Began a course of chemistry under the noted chemist and rosicrucian, Peter Sthael, of Strasburg, brought to Oxon. by the hon. Mr. Rob. Boyle, an. 1659. He took to him scholars in the house of John Cross next on the w. side to University Colle. The club consisted of 10 at least, whereof Francis Turner of New Coll. was one, Ben Woodroff of Ch. Ch. another, and John Lock of the same house, afterwards a noted writer. This John Lock was a man of turbulent spirit, clamorous and never contented. The club wrote and took notes from the mouth of their master, who sat at the upper end of the table, but the said J. Lock scorned to do it; so that while every man besides were writing, he would be prating and troublesome. After the beginning of the year 1663 Mr. Sthael removed his elaboratory to a draper's house, called John Bowell, afterwards mayor of the city, situate in the parish of All Saints. He built his elaboratory in an old hall in the back, for the house itself had been an ancient hostle; therein A. W. and his fellows were instructed. The chemical club concluded, A. W. paid Mr. Sthael 30 shill: having paid 30 shill: beforehand. A. W. got some knowledge and experience, but his mind still hung after antiquities and musick."

In spite of Boyle's private position, his blameless life, his devoutness, and his charity, his work aroused bitter animosity in Oxford. He was attacked in the University pulpit, in public orations, in private squibs; his theories were described as destructive of religion, his experiments as undermining the University. But what chiefly drew the indignation of his opponents was that he, a gentleman by birth and fortune, should concern himself with low mechanical arts. Against these attacks Boyle replied with irresistible logic. His vindication of the nobility of scientific work constitutes one of his greatest claims on our gratitude.

Boyle left Oxford in 1668. Mayow died in 1679. In 1683 Anthony Wood informs us that "the Oxford elaboratory was quite finished"; but the impulse given to the study of Chemistry in Oxford gradually died out. I do not know the history of the Chair of Chemistry in Oxford (if there was one) in the eighteenth century. Richard Frewin, of Christ Church, is described as Professor of Chemistry in 1708. He does not seem to have taken himself too seriously in this capacity. Uffenbach, who visited Oxford in 1710, says he found the stoves in fair condition, but everything else in the laboratory in dirt and disorder. Frewin himself was elected Camden Professor of Ancient History in 1727. He seems to have thrown himself into his new work with greater ardour; for Hearne relates that, on his election, he at once bought one hundred pounds' worth of books in chronology and history to fit himself for his duties. For a companion picture to this we may glance at the appointment in 1764 of Richard Watson (afterwards Bishop of Llandaff) to the Chair of Chemistry at Cambridge, which had been founded in 1702. Dr. Watson, we are told, knew nothing at all of chemistry; had never read a syllable nor seen a single experiment on the subject. On his election he sent to Paris for an "operator," and set to work in his laboratory. In fourteen months he began to lecture to a large audience.

But Watson at Cambridge was succeeded by Wollaston. We had to wait till Brodie for a successor to Boyle.

II.

We have seen what a vigorous effort Chemistry made to plant itself in Oxford in the seventeenth century. If the soil had been prepared the roots must have struck deep. But the University paid little heed, and after a few years of prodigal growth the plant withered and died out. It would seem that the positions are reversed at the present day. The University spends large sums for supervision and appliances; the young plants are brought here and nurtured at great expense, but the fair blossoms produce little fruit. Even our best friends admit that the results are somewhat disappointing. If these are the facts—and I speak as one who shares the responsibility for the present condition of chemistry here—it is the duty of those concerned to speak out; and I can conceive no more fitting opportunity than the present for pointing out some of the causes that appear to hinder our growth. Let no one think I wish to disparage the University. I should be the last person to do so. I owe to my old college the opportunity, the help, and the example which made me a chemist, and gave me an interest in life. I only wish to see more general the advantages it was my luck to meet with in Christ Church.

Chemistry in modern Oxford is accorded a place side by side with older studies. No one can complain that scholarships are not offered broadcast, that money has not been freely given for laboratories; and yet I think the student does not feel around him the atmosphere in which an experimental science should be cultivated. We see Chemistry endowed and extended, we do not see it respected by the bulk of students and of learned men. In my undergraduate days a rhyme was current here (I think it was coined in Cambridge—the Parnassus of parodies) expressing views which were undoubtedly held concerning the claims of chemistry as a subject for a degree. One verse ran—it was from the Lamentation of a would-be Bachelor—

> "I thought to pass some time before, but here, alas, I am,
> Having managed to be plucked in every classical exam.
> I cannot get up Plato, so my reverend tutor thinks
> I had better take up Chemistry, which is commonly called 'Stinks.'"

I do not quarrel with the versifier (except as a poet), I do not even quarrel with the reverend tutor, whose opinion of us is obviously small, because I do not think myself that Chemistry as it is taught is a very good subject for a degree. Still less is it a subject which we should allow to monopolise the schoolboys' time. While holding strongly that the elements of Physics and Chemistry form a necessary part of a liberal education, I believe we have made two mistakes with regard to the teaching of science. We have by our science scholarships encouraged too early specialisation at school; we have overburdened our undergraduates here with a multitude of facts they cannot retain. A boy specialises for two years at school; he learns a prodigious array of facts from the latest text-book, and also acquires some skill in the art of quickly reproducing what he has learnt. He wins a science scholarship. We then tell him he must go back to, or begin, the study of the classical languages we look on as essential for our degrees. By a certain time he must reach a certain (rather low) standard, or his scholarship lapses. He learns that it is advisable to get assistance from those who have made a special study of preparing candidates for pass examinations. He crams; or he goes to a crammer and is crammed. Let us suppose, as is usually the case, that the obstacle is Greek. I will not deny that the standard of Greek demanded may imply some important discipline at school, and some real culture of the mind, provided the instruction given is on wholesome lines and forms part of a liberal course. Got up in a hurry as it too often is, solely with the object of passing, it means time and effort wasted and worse than wasted. It is of no value in itself, for it is forgotten in less time than it took to acquire; and it gives the student the first pernicious taste of that superficiality and false knowledge it should be our special aim to remove. Is it not desirable that scholarships should be the reward of progress and ability *in the general subjects of school education* among which the elements of science should have a place? The brightest and most persevering boys would come to the University, and there make choice of the special course they wished to pursue.

My second complaint is that we teach too many facts. They are not all important. After three or four years' steady accumulation our men go into the schools walking dictionaries of chemistry. Parents not unnaturally think that their sons, after four years of college training, should be fit to take responsible places wherever chemists are in demand. But manufacturers, as a rule, do not care for University graduates. I cannot blame them. We cannot guarantee that the men we send out with honours in Chemistry can attack a new problem, can work out new processes, can prepare new dyes. German manufacturers, on the other hand, *prefer* a University graduate, for they have in *their* degree a guarantee that the student has successfully attacked some unknown problem, and added to the store of knowledge.

The influence of science on the nation's industry has been recognised and insisted on by those who can make their voices heard. The country has at length awakened to the fact that something is wanting, and cries out for Technical Instruction. It is not afraid of spending money: indeed, many well-meaning bodies are spending—and in some cases I fear, wasting—money with a prodigal hand. And what, after all, is the great need? Speaking for the subject I know best, I say unhesitatingly that we want scientific chemists who can and will make discoveries; we want men trained, not only in what has been done, but taught how to set about winning new knowledge. The Universities, I urge, should teach the art of research. This is what is wanted, and this, as all experience shows, is what the Universities can do better than anyone else. And no exorbitant amount of time need be demanded for this purpose. If the student has learnt the elements of science at school, three years at most should suffice for the preliminary degree course. The graduate, armed with the necessary manipulative skill, would then start research work under proper guidance as the second and more valuable portion of his University training. And here the new research degree (by whatever name it may be called) may give us most valuable help. I hope that serious work will be demanded for it, and that the research course will become the recognised avenue to science fellowships and lectureships in the University. Two years would show what the man had in him. In that time either he would have proved himself no chemist, or he would have made some useful advance in our knowledge, and would have secured a testimonial of fitness such as no examination could confer. Five years in all—the minimum time now laid down for a medical qualification—would surely be not too much to ask for the chemist's training.

No extra expense need be incurred to carry out this plan. Some of the college scholarships at present offered on entrance might be reserved for research studentships on graduation. These studentships should be the reward of the successful undergraduate career. On this point, which I have urged for many years, I am glad to find myself in entire agreement with the President of the Chemical Society. At Owens College our most successful endowment in chemistry has been the Dalton Scholarship, awarded for a research done in the College laboratories. In the Victoria University we have lately founded scholarships for the encouragement of research, which are awarded on the results of the final examination in the several Honours Schools. The winners are entitled to hold their scholarships at any university at home or abroad where they can continue their special studies.

I plead, then, for greater encouragement of chemical research in Oxford. Make it part of the normal course of training for everyone who wishes to be a chemist in fact as well as in name. Consider, not only the country's need, but the value of research itself as a mental training, as stimulating and strengthening the activities, as creating that sense of devotion and discipleship which becomes the tradition of every great school of learning.

Lastly, let us own that we ourselves—the teachers here—have been perhaps too critical, too much afraid of making mistakes, forgetting that the witty American's remark—that he who never makes mistakes never makes anything—has a far wider application in science than in politics. Only by practice and drill can we learn to collect our strength and swing it with precision into acts. Without that training, no matter how much faculty of seeing a man has "the step from knowing to doing" is rarely taken. There is nothing, I believe, in Oxford antagonistic to our cause. The genius of the place has not declared against scientific research; and if it be a true saying that men here imbibe a liberal education from the very air breathed by Locke and Berkeley, surely we also may draw scientific inspiration from this air, not only breathed, but first explained by Boyle and Hooke and Mayow.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY L. FLETCHER, M.A., F.R.S., F.G.S., PRESIDENT OF THE SECTION.

WITH an anxious desire to conform to the traditions of the past, I have sought in the Reports of the Association for guidance in my present difficulty; and have remarked that it is customary for a president, on first taking the chair, to express a deep sense of unworthiness for the position to which he has been called. My first duty, then, seemed a simple and obvious one; till I further remarked, to my dismay, that the more distinguished the president the more humble have been the terms in which such expression has been made. Hence I feel that it may appear to you presumptuous on my part if I myself make any apology at all, and it would doubtless imply a claim to the highest distinction if I were to make that humble apology which would be most appropriate to the circumstances of the case.

Instead, however, of dispensing with the apology altogether—that might be too radical an innovation to be introduced this year—I propose, with your sanction, to make a lesser change, and merely to defer the apology from the first to the last day of our session. I may reasonably hope to be able, at that later stage, to make clear to you, by simple reference to your own experience during the meeting, that any apology I may feel it to be then my duty to make is of no merely formal character, but one which is worthy of your serious consideration.

I would ask that in the meantime your continuous sympathy be extended to one who now finds himself in a position he would have been the last to seek, and whose ordinary duties in life involve speechless communion with inanimate nature rather than oral address to an assembly of fellow-workers.

This matter of apologetic precedent being thus disposed of to our common satisfaction, I should have preferred to have brought the delay of the normal business of the Section to an immediate end by calling upon the author of the first paper to now address you. Such, indeed, was the ordinary course of procedure in the earlier, and perhaps presidentially happier, years of the Association; but the occasion of taking the chair having been once seized upon, in absence of mind, by a mathematical president for the delivery of an address, it has come about that each president now feels it his bounden duty, not merely to give an address, but to make the address at least as long and at least as elaborate as any which has preceded it.

We shall all agree that a presidential address, if there is to be any at all, should be elaborately short and elaborately simple; it should deal, not with technical details such as are only intelligible, even to the president himself, after much study, but with general principles such as can be immediately grasped by every member of an audience; an opening address which is so long that it can be only partly read, and is written to be studied afterwards in the Reports of the Association, may more appropriately be issued as an ordinary memoir. I make this remark to safeguard the interests of future audiences, for the example of technicality which I am now about to set is one which I cannot recommend my successors to follow.

As for subject, an account of the progress of scientific work is always interesting and instructive, and immediately suggests itself as the natural basis of a presidential address. But seeing that, so lately as in February last, the geologists have had the advantage of an address from the retiring president of their Society, Mr. Hudleston, which has been virtually exhaustive in its survey and criticism of the British geological work of the last seven years, the time has scarcely yet arrived when a continuation of that review by the president of this Section can be of service to the members of the Association.

For this and other still more weighty reasons which I need not directly mention, I feel myself debarred from undertaking any review of recent geological progress, and shall therefore ask you to allow me to confine myself, in the remarks it is my duty to make, to a science which, though it is not purely geological and in the Reports of the Association has long been associated with another science, chemistry, is yet very closely related to the science of our own Section, Geology.

I trust that the members of the Section of Chemistry and Mineralogy are now so closely engaged in another place that they will fail to discover, or at any rate to resent, the technical trespass on their own domain: as for yourselves, you will perhaps be more ready to pardon the temporary excursion from the domain of pure geology if I remind you that the fathers of the Geological Society defined their sole object to be "the investigation of the mineral structure of the earth"; and I may add, if further defence be desired, that in the first half of this century the relationship of mineralogy and geology was so intimate that it was possible for a Section of the British Museum to be officially designated "the Department of Mineralogy, including Geology."

I was the more impelled to choose this subject for our consideration to-day when I reflected that pure mineralogy has been hitherto almost completely out of sight, and therefore probably out of mind, at the meetings of the Association. It is true that at the first meeting, held sixty-three years ago, Dr. Whewell, then the Professor of Mineralogy at Cambridge, was invited to draw up a report on the state of knowledge of the science, and that his report was submitted and printed in the following year. But in the course of the sixty-three years during which the Association has flourished, it has chanced that a mineralogist has on only one occasion, that of 1862, been seated in a presidential chair; and since at that time presidential addresses had not yet come to be regarded as necessary to the existence of the Sections, Prof. Miller refrained from inflicting a mineralogical dissertation on an audience which, he had reason to presume, would consist entirely, or almost entirely, of chemists. Perhaps you might be tempted to think that the want of prominence of the mineralogists at our previous meetings has been due to a becoming sense of modesty resulting from the study of that science: this would be a mistake. The fact is that a mineralogical memoir, dealing largely with numerical quantities and involving great variety of experiment and technicality, may be read and studied, but should never be heard; like the mathematician, the mineralogist despairs of making clear to an audience, especially a mixed one, the bearing of any researches which have been made in his subject. But now that sixty-two years have elapsed since the issue of Prof. Whewell's Report, the time has perhaps at length arrived when it is advisable, notwithstanding the difficulties surrounding an oral treatment of mineralogy, to attempt to give to the Association a faint idea of the present position of the study of the subject. And if most of my hearers find that the remarks are too technical to be in any great part intelligible, let them console themselves with the reflection that, if the future at all resembles the past, only Shalum and Hilpa can have to endure again that particular kind of *mauvais quart d'heure* which is to precede the geological feast of to-day.

The Systems of Crystallisation.—At the time of the publication of Prof. Whewell's report it had already been established by the researches of Romé de l'Isle, Haüy, Mohs, and Weiss that the position of any single face of any crystal can be exactly defined by means of two sets of quantities: firstly, three lines or axes, of which the lengths and mutual inclinations are characteristic of the substance itself; secondly, three whole numbers or indices, rarely rising higher in magnitude than the number 6: further an empirical arrangement of crystals into systems had been based by Mohs and Weiss on the relative lengths and inclinations of the axes. And a long series of observations of the optical characters of crystals had revealed to Brewster the fact that the boundaries of the classes of optically isotropic, uniaxal and biaxal crystals form part of the boundaries of the empirical systems. But whereas only three optical classes of crystals had been recognised, it was certain that there were at least four geometrical systems, and it was a matter of controversy as to whether the independence of two others should not be regarded as geometrically established.

The first important discovery following the issue of Whewell's Report was one which proved that the two doubted systems are natural ones. It was found by Herschel and Neumann that the biaxal crystals are not optically similar, as had hitherto been supposed, but are of three kinds. In crystals of one kind—for example, barytes—the two lines bisecting the angle of the optic axes internally and externally, and a third line perpendicular to both, are constant in direction in the crystal whatever the colour of the light; in a second kind—for instance, selenite—only one of these lines is constant when the colour varies; in a third kind—for instance, borax—none of the three lines has any constancy of direction. And these three kinds of biaxal crystal correspond exactly in their facial development to the three systems of crystallisation of which the independence had already been asserted by some crystallographers on geometrical grounds. From this time the arrangement of crystals into the six systems has been regarded as a natural one; and the optical method based on the figures seen in plates when examined in convergent polarised light has been in constant use, and is an invaluable aid in the determination of the system of crystallisation.

Crystallographic Notation.—For a simple method of expressing the relative positions of crystal faces by a symbol, crystallographers are infinitely indebted to the late Prof. Miller, of Cambridge. The symbols introduced by Mohs, Weiss, Lévy, Naumann, and the modification of the latter suggested by Dana, though interesting, are not to be compared for legibility, pronounceability, or utility in calculation, with the simple symbol which is associated with the name of Prof. Miller. Though the symbol was not invented by him, he was the one who, so to say, gave it life. He discovered and made known its many advantages; and in his treatise published in 1839—a treatise which is a masterpiece of mathematical terseness and simple elegance—he gave the methods of crystallographic calculation which render the advantages of the symbol particularly manifest. It may be here remarked that in that treatise the rationality of the anharmonic ratios of any four tautozonal planes of a crystal was first made known, and the property was largely used in the simplification of the methods of calculation: the fact that the fraction was of the knd which had been already termed an anharmonic ratio, however, had escaped the attention of the author.

But the change of a method of notation, like a change in the system of weights and measures, involves such serious practical difficulties that many years passed away before the Millerian symbol received abroad the consideration which it deserved. Now, at last, no continental text-book of mineralogy fails to introduce the Millerian indices, even if the symbols of Lévy or of Naumann are given in addition; and it is evident that within a few more years the mineralogist will be completely relieved from the tiresome necessity of translating each crystalline symbol into another form to make it intelligible to him, and the student will be able to make a more advantageous use of the time which has been hitherto devoted to acquiring a mastery over a second and unnecessary form of crystallographic notation. For this result credit is largely due to Prof. Groth, of Munich, whose adoption of the Millerian symbol in the *Zeitschrift für Krystallographie* has done much to bring home its advantages to the foreign worker. It is to be hoped that Prof. Groth will earn the further gratitude of students by encouraging the adoption of the true Millerian symbol in the still outstanding case of the Rhombohedral System.

Rationality of Indices and the Law of Zones.—It may here be pointed out that, although the importance of zones for the simplification of crystallographic calculation had been recognised by Weiss, it was only later that Neumann proved that the fact that all possible crystal faces can be derived by means of the intersection of zones is a necessary consequence of the rationality of the indices; that, indeed, the law of zones is mathematically identical with the law of rationality. To the same able physicist and mathematician we owe the development of the method of stereographic projection now in common use by crystallographers for the representation of the poles of crystal faces.

Symmetry.—We have said that the recognition of six systems of crystallisation was a result of consideration of the lengths and mutual inclinations of certain lines called axes. Now, it had long ago been remarked that any one face of a crystal is accompanied by certain others similarly related to the geometrically similar parts of what may be regarded as a fundamental figure: such a group of concurrent faces is called a simple form. It came to be recognised, too, that all the faces of such a form can be geometrically derived from any one of them by repetition, according to certain laws of symmetry, and that the same laws of symmetry are binding for every simple form or combination of forms exhibited by crystals of the same substance. Hence it came to be perceived, though very slowly, that the essential differences of the systems of crystallisation are not mere differences of lengths and mutual inclinations of lines of reference, but are really differences of symmetry. Ever since his appointment to the professorship of Mineralogy in this University, now thirty-eight years ago, Mr. Maskelyne has been persistent in directing attention to the importance of symmetry, and such importance now receives universal recognition.

Thirty-two Types of Symmetry in Crystals.—But in each system of crystallisation it becomes necessary to recognise both completely and partially symmetrical types. In the latter, the symmetry is in abeyance relative to various planes or lines which in other crystals of the same system are active as planes or axes of symmetry. But this abeyance of symmetry is itself found to be subject to a law, for all planes or axes of symmetry which are geometrically similar are either simultaneously active or simultaneously in abeyance. By means of this law relating to partial symmetry, it has been inferred that altogether thirty-two types of symmetry are possible in the six crystalline systems.

The possible existence of these thirty-two types of symmetry of crystals is thus an induction from observation: the question naturally arises as to why only these thirty-two exist, or are inferred by analogy to be possible. Axes of symmetry are observed, round which faces of crystals are symmetrically repeated by twos or threes or fours or sixes; why is it that in crystals no axis of symmetry is ever met with round which the faces are symmetrically repeated by fives or sevens? A few words as to how this most important problem has been attacked and solved may be of interest.

We know that the characters of a crystal relative to any line in it vary with the direction of the line, but are the same for all lines parallel to each other. Such a property will result, if we imagine with Bravais that in a crystal elementary particles are arranged at equal distances from each other along every line, and are similarly arranged in all those lines which are parallel to each other; the distances separating particles being, however, in general different for lines which are inclined to each other. Such an arrangement of particles is termed parallelepipedal: space may be imagined to be completely filled with equal and similarly disposed parallelepipeds, and an elementary particle to be placed at every corner or quoin of each. Further, each particle is regarded, not as being spherical, but as having different characters on its different side; and the particles must be similarly orientated—that is, have similarly sides in similar positions.

Now, it will be seen on an examination of a model or figure that with such an arrangement any plane containing three particles will contain an infinite number, all arranged at the corners of parallelograms. Further, any such plane will clearly have whole numbers for the indices which fix its position, for along any line the distance between two particles is by hypothesis a whole multiple of the common distance between any two adjacent ones in the same line. Thus the first great crystallographic law—the law of the rationality of the indices—is an immediate consequence.

In the next place, it was found that the possible modes of symmetry of arrangement of the particles of such a system depend on the form of the paralleliped, and that any possible arrangement of the particles must present a symmetry which is identical with one or other of the six completely symmetrical types already referred to. And calculation shows that any other mode of grouping—a repetition by fives or sevens, for example—round an axis of symmetry, would involve the presence of planes having irrational indices; and this according to the first law is impossible.

The abeyance of symmetry, however, met with in the partially symmetrical types required the aid of an auxiliary hypothesis—namely, that the abeyance of symmetry belongs to the particle itself, and not to the arrangement of the particles.

But the parallelepipedal arrangement imagined by Bravais is unnecessarily special. Our actual observations of physical characters relate not to single lines of particles, but to groups of parallel lines of particles: the identity of character observed in parallel directions is thus not necessarily due to actual identity of each line with its neighbour, but may be due to statistical equality, an equality of averages. If, for example, a plane were divided into regular hexagons, and a particle were placed at each corner of each of these figures, the physical properties of the system of particles would be the same along all lines parallel to each other as far as experiment could decide, and yet the arrangement of the particles in the plane, though possibly crystalline, is not that of a Bravais system. In any straight line passing along the sides of a series of the hexagons, the particles will not be equidistant from each other: they are in equidistant pairs, and the two nearest particles of adjacent pairs are twice as far from each other as the particles of the same pair.

Sohncke accordingly suggested a more general definition than that of Bravais for the regularity of the arrangement, a definition which had been proposed some years before by Wiener—namely, that the grouping relative to any one particle is identical with that relative to any other. This definition admits of the possibility of the hexagonal arrangement just mentioned; further, it allows of the orientation of the particles themselves being different in adjacent lines. Following a mathematical process which had been already employed by Jordan, Sohncke deduced all the possible modes of grouping consistent with the new definition, and for a time was under the impression that the types of symmetry found by him to be mathematically possible are exactly identical with those already referred to; and this without introducing the auxiliary hypothesis relative to partial symmetry of the elementary particles of merosymmetrical crystals, except in cases of hemimorphism. It was, however, pointed out by Wulff, who has himself made valuable contributions to the subject, that though no unknown crystallographic type belongs to such a regular arrangement, one type of symmetry, that presented by dioptase, is missing; and it seems that, in this case at least, the merosymmetry can only be accounted for by the merosymmetry of the particle, or something equivalent to it, if the definition of regularity suggested by Sohncke is to be accepted. It was recognised by Sohncke that each of his point-systems can be regarded as a composite Bravais system, one of the latter being repeated in various positions corresponding with the symmetry of the parallelepiped itself.

More recently, Schönflies has made a more general hypothesis still—namely, that in each substance, whether its crystals be completely or partially symmetrical in facial development, the particles are not of a single kind, but of two kinds, related to each other in form in much the same way as a right-hand glove and a left-hand glove. With this hypothesis he finds that all the thirty-two known types are accounted for without any specialisation of the characters of the particle, and that no other type of symmetry is mathematically possible.

It now only remained to discover that Prof. Hessel had already arrived at the thirty-two types of crystallographic symmetry by mathematical reasoning more than sixty years ago; his work, being far in advance of his time, appears to have attracted no attention, and the memoir remained unnoticed till more than half a century after its publication.

Starting from Sohncke's definition of a regular point-system, and proceeding, though independently, by a method which closely resembles that of the regular partitioning of space by Schönflies, Mr. William Barlow has given in a paper just issued a general definition applicable to all homogeneous structures whatever, and has shown that every such homogeneous structure falls into one or other of thirty-two types of symmetry, coinciding exactly with the thirty-two types of crystal-symmetry. He points out that each of those homogeneous structures which possess planes of symmetry or centres of symmetry does so by reason of its having an additional property beyond mere homogeneity, namely, that if we disregard mere orientation, it is identical with its own image in a mirror. Mr. Barlow further discovers that every one of the Sohnckian point-systems can be geometrically constructed by finite repetition of some one of a certain ten of them.

Lord Kelvin, who, with characteristic versatility, has lately enlightened us with his researches on Molecular Tactics, has quite recently attacked another problem of the same group, and has sought to discover the most general form of cell which shall be such that each cell encloses a single point of a Bravais system, while all the cells resemble the parallelepipeds, of which we have already spoken, in being equal, similar, similarly orientated, and in completely filling up space. He finds that in the general case the cell can have at most fourteen walls, which may be themselves either plane or curved, and may meet in edges either plane or curved. Having regard, however, to the limited time at our disposal. we may hesitate before following Lord Kelvin into his curious and many-walled cells.

The deduction of the thirty-two types of symmetry by mathematical reasoning was also made independently by both Gadolin and Viktor von Lang thirty years ago from the law of rationality of indices; while Fedorow points out that the method of deduction recorded in the recent German treatise of Schönflies is remarkably similar to the one independently published by himself in Russia. Both Curie and Minnigerode have also lately given comparatively brief solutions of the problem.

Nor must I omit to mention to you the elaborate memoir dealing with the symmetry of parallelepipedal point-systems which was written by the late Prof. Henry Stephen Smith, whose too early death this University has so much reason to deplore. To the outer world he was perhaps best known as one of the most perfect mathematicians of the age, but those who had the good fortune to find themselves among his pupils will always treasure up in their memory rather the kindly courtesy, the warm sympathy of the man, than the genius, however anscendent, of the mathematician.

To sum up this part of the subject—it is now established that a definition of the regularity of a point-system can be so framed that thirty-two, and only thirty-two, types of symmetry are mathematically possible in a regular system, and that these are identical with the types of symmetry that have been actually observed in crystals, or are inferred by analogy to be crystallographically possible.

It remains for subsequent investigators to determine what the points of the system really correspond to in the crystal; according to Schönflies, the physicist and the chemist can be allowed in each crystal absolute control within a definite elementary region of space, and the crystallographer is only entitled to demand that the features of this region are repeated throughout space according to one or other of the thirty-two types of symmetry already referred to; or, what appears to be the same thing, the crystallographer requires mere homogeneity of structure.

Simplicity of Indices.—We have seen that the planes containing points of a regular point-system have rational indices. But there still remains unaccounted for the remarkable fact that the indices of the natural limiting faces, and also of the cleavage-planes of a crystal are not merely whole numbers, but are in general extremely simple whole numbers. Bravais and his followers have sought to account for this by the hypothesis that both the natural limiting planes and the cleavage-planes are those planes of a point-system which are most densely sprinkled with points of the system. Curie and Liveing, independently of each other, have been led to the same result from considerations relative to capillary constants. Sohncke, however, pointing out that there are many cases—for instance, calcite—where an excellent cleavage-plane is rarely a limiting plane, suggests that his generalised point-system is more satisfactory than a Bravais system in that not only the density of the sprinkling must be had regard to, but also the tangential cohesion of the particles in the plane, and that in his system these may be independent of each other; while Wulff remarks that Sohncke's arrangement is identical with that of Bravais for the anorthic system, where the same objection holds, and he denies the legitimacy of the reasoning by which the hypothesis of a relation between the density of the sprinkling of points on a plane and the likelihood of the natural occurrence of the plane as a limiting face is supported.

Complexity of Indices.—Doubtless, however, crystal faces are observed of which the symbols involve indices far exceeding 6 in magnitude—so complex, in fact, that one is tempted to doubt the rigidity of the experimental proof that indices are necessarily rational. Often, though the numbers are high, their ratios differ by only small amounts from simple ones. A most patient and detailed study of such faces was made for danburite by the late Dr. Max Schuster of Vienna, and the results were brought by him some years ago to the notice of this Section. From careful examination of similar faces in the case of quartz, Molengraaf has been led to conclude that it is extremely probable that such faces are of secondary origin and have been the result of etching; they would in such case correspond, not to original limiting planes, but to directions in which the crystal yields most readily to solvent or decomposing influences.

Optical Characters.—Passing from the purely geometrical characters of crystals to the optical, we may in the first place remark that the relationship between crystalline form and circular polarisation discovered by Herschel in the case of quartz, has been generalised since the issue of Whewell's Report. We now know that many crystallised substances belonging to different systems give circular polarisation, and that all of them are merosymmetrical in facial development or structure; further, they belong to types of symmetry which have a common feature, though this is only a necessary, not a sufficient, condition.

The importance of the discovery of the dispersions of the mean lines has already been referred to.

We may recall attention to the fact noticed by Reusch that when cleavage-plates of biaxal mica are crossed in pairs and the pairs are piled one upon another in similar positions, the optical figure yielded by the combination approaches nearer and nearer to that of a uniaxal crystal the thinner the plates and the more numerous the pairs: in the same way, by means of triplets of plates, each plate being turned through one-third of a complete revolution from the position of the preceding one, it is found possible to closely imitate the optical figure of a right-handed or a left-handed circularly polarising crystal.

And it has been observed that repeated combinations of differently orientated parts actually occur in crystals. Large crystals of potassium ferrocyanide, for example, are really composite, and the different parts are differently orientated: on the one hand, a thick slice may give an optical figure which is uniaxal; on the other hand, a thin slice shows two optic axes inclined to each other at a considerable angle.

It has been suggested that the circular polarisation of quartz and other crystals is due to a spiral molecular arrangement.

corresponding to that of the mica-triplets as arranged by Reusch. Such a spiral arrangement is shown by the points of the corresponding Sohnckian system.

Optical Anomalies.—As already mentioned, we owe to Brewster the establishment of the relation between the optical behaviour of crystals and the systems of crystallisation. But in the course of his long research Brewster met with numerous puzzling exceptions, and to the investigation of the origin of their peculiar optical behaviour he devoted much study; subsequent workers have concurred in expressing their admiration of the accuracy of his observations and descriptions, more especially when regard is had to the extreme simplicity of the apparatus available in those early days.

It was recognised by Brewster that some of these optical anomalies are due to a condition of strain, of the crystal as in the case of the diamond. But in other minerals, as analcime and apophyllite, the hypothesis of strain was not entertained by him: he regarded the crystals as being truly composite and not simple; and, recognising optically different kinds of apophyllite, went so far as to give to one of them the specific name of tesselite by reason of its distinctive characters. Biot, on the other hand, sought to account for this kind of optical behaviour in another way, by the hypothesis of lamellar polarisation: a crystal of alum, for example; he held to be built up of thin laminæ arranged parallel to the octahedral planes, and imagined that light which had traversed such a crystal is polarised by its passage through the aggregation of laminæ in the same way as by passage through a pile of glass plates. But in the latter case there is a frequent passage of the light from air to glass and glass to air, whereas in the case of alum there is no evidence of the existence of atmospheric intervals. Frankenheim sought to overcome this difficulty by the further hypothesis that the successive layers of a composite posassium- and ammonium-alum are of different chemical composition, but such a difference of material would be insufficient for the desired object by reason of the nearness to each other of the refractive indices of alums of different composition. Still it is a remarkable fact that neither a pure potassium-alum nor a pure ammonium-alum shows any depolarisation-effects at all; these belong only to the alums of mixed composition, and yet there is no visible difference in the physical structure of the crystals of simple and composite material.

An epoch was made in the history of the so-called optical anomalies by the publication in 1876 of an elaborate memoir by Prof. Ernest Mallard of Paris, whose death last month deprived Mineralogy of its greatest philosopher. To make the position more clear, we may take as a definite illustration the mineral boracite. In development of faces and magnitude of angles the crystals of this mineral are, as far as measurement with the goniometer can decide, precisely cubic in their symmetry. But an apparently simple crystal of boracite, when examined in polarised light, behaves exactly like a regularly composite body. If the crystal be a rhombic dodecahedron in external development, all the twelve pyramids which can be formed by drawing lines from the centre to the angular points are found to be exactly similar to each other in everything but orientation; and, further, each of them has the optical characters of a biaxal crystal, the optic bisectrix of each individual pyramid being perpendicular to the corresponding base, and thus having a different direction for each of the six pairs of parallel faces of the dodecahedron. Hence Mallard inferred that boracite belongs really, not to the cubic, but to the orthorhombic system, and that its crystallographic elements are so nearly those of a cubic crystal that the molecular structure is in stable equilibrium, not only when different molecules have their similar lines parallel, but also when only approximately similar lines have the same orientation: further, the cubic symmetry of the external form was regarded by him as a consequence of the approximation of the crystallographic elements to those of a cubic crystal and of the variety of orientation of the constituent molecules. Variety of orientation of constituent molecules is, in fact, already recognised in the case of ordinary interpenetrant twins. The variation of optical character in different crystals of the same substance or different parts of the same crystal was then explained as being due to the variation in the number of molecules belonging to each mode of orientation.

According to another view, it was contended that a crystal of boracite is really cubic and simple, but that, like unannealed glass, it is in a state of strain related to the external form. It was replied that the optical characters of such unannealed glass are changed with the change of strain which follows the fracture of the specimen, while those of boracite are unaltered when the crystal is broken. To this it was rejoined that a once compressed gum retains its depolarising character unchanged on fracture of the specimen, and that the same permanence may very well be a character of some strained crystallised bodies.

The controversy, however, passed to a fresh stage when it was discovered that boracite becomes optically isotropic when sufficiently heated, and resumes an optically composite character on cooling. Mallard showed that the temperature at which the change takes place is a definite one, 265° C., and that a definite amount of heat is absorbed or given out during the change of condition.

It is now agreed that boracite is really dimorphous; that above 265° it is cubic in symmetry, below 265° orthorhombic: the only remaining point of controversy as regards boracite seems to be whether the external form owes its cubic symmetry to the crystallisation having taken place at a temperature higher than 265°, and therefore when the structure itself was truly cubic—or at a temperature below 265°, in which case the cubic character of the form would be ascribed to the fact that the orthorhombic constituent particles are so nearly cubic in their dimensions that at any temperature they may by variety of orientation combine to form a structure having practically cubic symmetry, and naturally limiting itself by faces corresponding to such a symmetry.

In exactly the same way leucite and tridymite become respectively optically isotropic and uniaxal when sufficiently heated, and the optical characters then correspond exactly to the symmetry of the external form.

Three years ago Dr. Brauns prepared a most useful summary of the ninety-four memoirs which had up to that time been contributed relative to the much-discussed subject of the optical anomalies of crystals, and added many new experimental results which had been obtained by himself. He concludes that the original view of Mallard—namely, that an optically anomalous structure consists merely of differently orientated particles of the same kind and of symmetry approximating to a higher type—is only applicable to a very limited number of crystals, such as those of prehnite; that dimorphism is the true cause in others, boracite being an example; that in the remaining minerals the cause is strain, which in some of them is due to foreign enclosures, as in the case of the diamond, and in others is due to a molecular action between isomorphous substances, as in the mixed alums and the garnets.

Planes of Gliding.—One of the most startling of crystallographic discoveries was one made by Reusch, who found that if a crystal of calcite is compressed in a certain way each particle springs into a new but definite position, exactly as if the crystal had undergone a simple shear and the particles at the same time had each described a semi-somersault: a simpler method of producing the same result was discovered afterwards by Baumhauer. If only part of the calcite crystal is sheared, the two parts of the structure itself are related to each other in the same way as the two parts of a twin growth; but in general the external form is different from that of a twin, since after the shearing of the material few of the faces retain their former crystallographic signification. The property has since been shown by Bauer, Liebisch, and more especially Mügge, to be a very general one; and doubtless the so-called twin lamellæ met with in rock-constituents have in many cases resulted from pressure during earth-movements long subsequent to the epoch of formation of the crystals. Similar lamellæ have been produced artifically in anhydrite and some kinds of felspar by exposure of the crystals to a high temperature.

Piezo-electricity.—The most remarkable addition to our knowledge of the relation of minerals and electricity has been the recent discovery of the electrification produced by strain (piezo-electricity). It has been shown by J. and P. Curie that if a quartz-plate, with faces cut parallel to the axis and silvered to make them conductive, be strained in a certain direction, the two faces either become oppositely electrified or show no signs of electrification at all, according as the faces of the plate are cut to be perpendicular to the prism-faces, or to pass through the prism-edges. Lord Kelvin says that this result is explicable by electric eolotropy of the molecule and by nothing else, a character which he had suggested for the molecule thirty-four years ago: experiments confirmatory of this hypothesis of the permanent electrification of the molecule were made some time ago by Riecke.

Pyro-electricity.—The development of opposite electricities at different parts of a crystal during changing temperature (pyro-electricity) has long been known in the case of tourmaline. We owe to Hankel a long series of investigations of this kind relative to boracite, topaz, and various other minerals, but it seems to be now established that most of the electrifications observed by means of his method are really piezo-electric, and are due to strains caused by inequality of temperature in different parts of the cooling crystal. A model has been lately made by Lord Kelvin which gives a perfect mechanical representation of the elasticity, the piezo-electricity, and also the pyro-electricity of a crystal.

Electrical Methods.—A delightfully simple method of investigating the difference of electrical condition of the parts of a cooling crystal and of making the distribution of electricity visible to the eye has been invented by Kundt. Mixed particles of (red) minium and (yellow) sulphur are oppositely electrified by their passage through the meshes of a small sieve; falling on the cooling crystal, each particle adheres to the oppositely electrified region, and the electrical condition of the latter is thus immediately indicated by the colour of the adherent powder. Mr. Miers remarks that this method is practically useful as a means of discrimination even when the crystals are extremely minute.

Other Physical Characters.—Of other physical characters much studied since the issue of Whewell's Report, I may recall to you more especially the dilatation of crystals on change of temperature, in which the observations of Mitscherlich have been extended by Fizeau and Beckenkamp; the forms of the isothermal surfaces of crystals, as determined by Sénarmont, and afterwards by Röntgen; the magnetic induction treated of by Faraday, Lord Kelvin, Plücker, and Tyndall; the hardness of crystals for different directions lying in the same faces, by Grailich, Pekárek, and Exner; the elasticity of crystals, investigated by Neumann, Lord Kelvin, Voigt, Baumgarten, and Koch; the distortion of crystals in an electro-magnetic field, by Kundt, Röntgen, and MM. Curie.

Chemical Relations.—In the short time I can reasonably ask you to allow me it is clearly impossible to enter upon any discussion of the increase of our knowledge of the chemical relations of minerals, and to treat of the much-investigated subjects isomorphism, polymorphism, and morphotropy, nor can I attempt to give you any idea of the advance which has been made towards a natural classification: nor must I mention the experiments which have been made relative to the growth of crystals, the etching of their faces, or their directions of easiest solution.

As regards systematic mineralogy an immense amount of progress has been made. The condition of affairs in 1832 was described by Whewell as follows:—"We have very few minerals of which the chemical constitution is not liable to some dispute; scarcely a single species of which the rules and limits are known, or in which two different analyses taken at random might not lead to different formulæ; and no system of classification which has obtained general acceptation or is maintained, even by its proposer, to be free from gross anomalies." An idea of the extent of the improvement will be best obtained from a comparison of the first edition of Dana's Treatise, published in 1837, and that treasury of information, the sixth edition, which appeared in 1892. The names of Miller and Descloizeaux are to be honourably mentioned in connection with this detailed work on species. In the interval of time under consideration the number of well-established species has been more than doubled, and the rate at which new species are discovered shows as yet no sign of diminution. In particular, I may remind you of the work which has been done in the correlation of the members of large groups, like the felspars, amphiboles, pyroxenes, scapolites, micas, tourmalines, and garnets. A paper just published by Penfield relative to topaz furnishes an excellent illustration of the important results which are still to be arrived at from a careful study of a common mineral. It has long been known that the mutual inclination of the optic axes of topaz is very different in different specimens, and it has been suspected that the variation might depend on the percentage of fluorine. Prof. Penfield has carefully determined, not only the fluorine, but also the water yielded in the course of analysis of specimens from different localities, and finds that the analytical results are best explained by the hypothesis of an isomorphous replacement of fluorine by hydroxyl; further, he discovers that the magnitude of the angle between the optic axes is a function of the amount of that replacement.

The successes achieved in the artificial formation of minerals, the advances made in the methods of discrimination of minerals by the blowpipe and micro-chemical reactions, the increase in our knowledge of the modes of alteration of minerals, of their association, of their modes of occurrence, must all be left undiscussed.

Instruments.—I may add a word relative to the instrumental appliances which have been placed at the service of the mineralogist since the issue of Whewell's Report. As regards goniometers, the provision of two mechanical circular movements in perpendicular planes for the easier adjustment of a crystal-edge parallel to the axis of the instrument, first suggested by Viktor von Lang when assistant at the British Museum, has proved a great convenience and is now in general use. The employment of a collimator with interchangeable signals, of a telescope with interchangeable eyepieces, and the provision of lenses and diaphragms for obtaining images from faces so small as to be invisible to the unassisted eye, would seem to have brought the reflective goniometer, the invention of our distinguished countryman Dr. Wollaston, to a degree of perfection where further improvement is scarcely to be looked for; though two crystallographers, Fedorow and Goldschmidt, have recently constructed instruments with an additional telescope and entirely different arrangements. It may be worthy of remark that, though reflective goniometers are generally made for use with very small specimens, one was constructed for the British Museum some years ago by which it is possible to measure the angles of a valuable crystal without removal of the specimen from a matrix of several pounds' weight.

The polariscope for use with convergent light, the stauroscope, the employment of polarised light with the microscope, the adaptation of the microscope for the observation of the interference-figures yielded by extremely minute crystals, the spectroscope in the investigation of selective absorption, have all proved of great service in the advancement of our knowledge of the characters of minerals.

Worthy of special mention is that recent addition to our resources, the total reflectometer, an instrument by which it is possible to determine with wonderful accuracy the refractive index or indices from observation of the reflected light. The process was long ago suggested by Wollaston; but it is only within the last few years that forms of instrument have been devised by Kohlrausch, Soret, Liebisch, Pulfrich, and Abbe, which make the method as precise in its results as that which depends on refraction by a prism. In its more refined forms the total reflectometer has been used to test the accuracy of the form of Fresnel's wave-surface: in the convenient, though less precise, form devised by Bertrand, the instrument is useful in the discrimination of the species of minerals.

For the measurement of the optic axal angle, when the angle is so large that the rays corresponding to the optic axes are totally reflected at the surface of the plate and do not emerge into air from the crystal, Prof. W. G. Adams made the valuable suggestion that the crystal-plate should be interposed between two hemispheres of glass; several instruments on this principle have been constructed abroad, and have only been imperfectly satisfactory, but one lately made in this country for the British Museum, under the superintendence of my excellent colleague Mr. Miers, proves to be most efficient for the intended purpose. Mr. Tutton's apparatus for supplying monochromatic light of any desired wave-length is a noteworthy addition to the instrumental resources of the mineralogist. The meldometer of Joly for the more accurate determination of the fusing point of minerals should also be recalled to you.

In this slight sketch it has been possible to make only the barest mention of some of the more important results which have been arrived at since the issue of Whewell's Report. You will doubtless think that it must have been possible in the year 1832 to look forward enthusiastically to the progress which was about to be made. But though Professor Whewell was himself confident that valuable discoveries would reward the mineralogical worker, he was sadly depressed, and, I think I may venture to say, with good reason, by the neglect of mineralogical study in this country. His own words are: "This decided check in the progress of the science has, I think, without question, damped the interest with which Mineralogy, as a branch of Natural Philosophy, has been looked upon in England. Indeed, this feeling appears to have gone so far that all the general questions of the science excite with us scarcely any interest whatever. But a more forward and hopeful spirit appears to have prevailed for some time in other countries, especially Sweden, Germany, and more recently France." Those are the words of despair. I may add that in the same year he resigned his professorship of Mineralogy, and directed his vast energy to the advancement of other subjects.

Now, I think, that a country like our own, which aims at taking and maintaining a high place in the scale of civilisation, ought in some way or other to secure that in every important branch of learning there is a group of men in the country who will make it the main purpose of their lives to render themselves familiar with all that has been and is being discovered in the subject, will do whatever is possible to fill up the gaps in the science, and, last but not least, will make the more important results accessible to other workers for whom so complete and original a survey is impracticable.

No one will doubt that Mineralogy should be such an important branch of learning. Minerals existed before man was thrust upon the scene; they will possibly continue to exist long after he himself has passed away; at least as persistent as himself, they will have an interest for every age.

The continental nations have not only long recognised the importance of mineralogical study, but have acted accordingly. The difference between action and inaction will be most clearly grasped if we compare the position of Mineralogy in Germany with that in this country.

In Freiberg, the centre of a mining district in Saxony, an institute was opened in the year 1766 for the scientific training of those students whose interest was in minerals, and the lectures on Mineralogy given there by Prof. Werner became a prominent feature; of the many pupils of this remarkable man, Von Buch Breithaupt, Haidinger, Humboldt, Mohs, Naumann, and Weiss may be especially mentioned as having afterwards distinguished themselves by their scientific work. Of other Germans, who have likewise gone to their rest after much labour given to the advancement of Mineralogy and Crystallography, we may especially recall Beer, Bischof, Blum, Credner, Hessel, Klaproth, Kobell, Lasaulx, Mitscherlich, Neumann, Pfaff, Plattner, Plücker, Quenstedt, Vom Rath, Reusch, Gustav Rose, Heinrich Rose, Sadebeck, Scheerer, Sartorius von Waltershausen, Websky, and Wöhler. Of the many Germans who are now contributing to our knowledge of minerals it is an invidious task to make a selection, but we may mention Arzruni, Bauer, Beckenkamp, Bücking, Cathrein, Cohen, Goldschmidt, Groth, Haushofer, Hintze, Hirschwald, Klein, Klockmann, Knop, Laspeyres, Lehmann, Liebisch, Lüdecke, Mügge, Osann, Rosenbusch, Sandberger, Streng, Voigt, Weisboch, and Zirkel: most of them are University Professors of Mineralogy; all of them hold important positions as teachers of the subject. Further, the laboratories and instruments available for the teaching of practical work are in many cases, notably at Strassburg, Munich, Göttingen, and Berlin, of an elaborate character.

So much for Germany; let us now look at home. In the Universities of England, Wales, Scotland, and Ireland there is a grand total of—two Professorships of Mineralogy, one of them at Cambridge, the other, and younger one, at Oxford. Further, the stipends are nearly as low as they can be made; in the former case, according to the University Calendar, the stipend paid from the University Chest to the present holder of the office amounts to 300*l.* a year; in the more ancient but less extravagant University of Oxford, the Calendar states that the present professor receives, subject to previous deduction of income-tax, the annual sum of 100*l.*, and the necessary instruments and many of the specimens have presumably been provided from his private resources; in case of residence he is to be allowed another 150*l.* a year for the luxuries which University life involves. And these are the only teaching appointments in his own subject that a successful investigator of minerals can look forward to being a candidate for! The result is that all those students who intend to earn their own living, all those who feel anxious to undertake professorial work, conclude that, however much they may be interested in the investigation of the characters of minerals, they will do well to follow the example of Prof. Whewell and turn to other branches of science in which there is a more hopeful prospect of their studies meeting with practical recognition.

It cannot be expected that advanced Mineralogy will ever be able to command the attention of more than a limited number of students, seeing that its successful pursuit requires a preliminary knowledge of at least three other sciences—mathematics, physics, and chemistry—sciences which must be assigned a fundamental importance in any scheme of education; if geology can be added, so much the better. Only few students can find time in their undergraduate days to acquire a competent knowledge of the preliminary sciences and to proceed afterwards to the study of Mineralogy. But the comparatively flourishing condition of the science in Germany, France, and other countries indicates that this is not a sufficient reason for refraining from giving proper facilities and encouragement to those who wish to enter upon its study. Some years ago the University of Cambridge took a step in the right direction, and introduced Mineralogy into their examinational system in such a way that the students of Physics, Chemistry, and Geology could give time to the acquisition of a knowledge of Crystallography and Mineralogy, and obtain credit for that knowledge in the examination for a degree.

It is clear that if in the future there is to be an honourable rivalry between this and other countries in the advancement of the knowledge of minerals, each of our Universities should be enabled in some way or other to found Professorships of Mineralogy, and be prevailed upon to follow the example of Cambridge in encouraging the students of Physics, Chemistry, and Geology to acquire a knowledge of Crystallography and Mineralogy before their education is regarded as complete. Even where a student has no intention of devoting himself to advanced mineralogical study, an elementary knowledge of Crystallography and Mineralogy will be extremely useful in giving him a better grasp of his own special subject.

And if, perchance, any of you are anxious to reduce the amount of those unmentionable duties of which we have heard so much of late, and feel that you can best do this by the endowment of Professorships of Mineralogy in our Universities, I would advise you not to do what has been so long practicable at this Association, couple Mineralogy with any other science—that would be an unwise economy. Each of the sciences is now so vast in its extent that no professor can be thoroughly master of what has been done, and is now being done, by other workers, in more than one of them. I remember that in my younger days it was held by some at Oxford that the Professor of Mineralogy, a so-called subordinate subject, should continue to be paid on a lower scale than his brother professors, and that he should obtain a living wage by adding a college-tutorship or a lectureship in some other subject to his professorial duties. It is not by the prospect of such appointments that you can expect the most capable men to be attracted to the study of minerals. The practical effect of such an arrangement would only be that a college lecturer would give formal teaching in Mineralogy while devoting his real energy to another subject in which the pupils are more numerous.

It only remains to thank you for the way in which you have listened to a technical address relative to a science for the study of which very few facilities have been offered to you in our own country. Not often does the mineralogist present himself before an audience; he sees only too clearly that

> The applause of listening senates to command,
> To read his history in a nation's eyes,
> His lot forbids;

but I shall not have broken the long silence in vain if I have made clear to you that, though the Science of Mineralogy is itself making great progress, we have hitherto given too little encouragement to its study in our own Universities, and lag far behind both Germany and France in the recognition of its importance.

NOTES.

WE notice with much regret that Dr. C. R. Alder Wright died on July 25, at the early age of forty-nine. He was elected a Fellow of the Royal Society in 1881.

DR. M. FILHOL has been appointed to the chair of Comparative Anatomy in the Paris Muséum d'Histoire Naturelle, in succession to the late Prof. Pouchet.

ACCORDING to a telegram from Prjevalsk (formerly Karakol), a monument to the Russian traveller Prjevalsky has been