melting temperatures, of the materials used in the glass mixtures, and leave the contents of the pot uncontaminated at the conclusion of the operation. This research would probably be a long and costly one, and is such as might most appropriately be undertaken by the National Physical Laboratory, especially as certain details in the manufacture of optical glass may come under review during the inquiry.

Authoritative Testing of Glass.

(5) In another direction evidence was submitted to the guild to the effect that it would be distinctly advantageous to the optical trade if *increased facilities for the authoritative determination of the optical constants* and *relative absorption of samples of glass* submitted for test could be provided at the National Physical Laboratory

THE FUTURE.

Facilities for Education a Matter of Great Urgency.

(6) In still another direction evidence, additional to evidence collected before the outbreak of hostilities, was elicited that the *facilities for education in technical optics are very inadequate.* It was shown that not only could some of the present difficulties connected with the supply of optical glass probably be diminished, but that the output of optical instruments for national purposes would be increased, and that the optical trade would be substantially benefited in other directions if such facilities were largely extended. The Technical Optics Committee of the guild was originally appointed, early in the year 1914, for the purpose of "inquiring into the need of an Institute of Technical Optics and the steps to be taken in connection therewith, and in due course it submitted a "statement" on the subject, which was published in the annual report of the guild, 1914, pp. 31–34. The committee had further reported to the Executive Committee of the guild at its July meeting, and the report was adopted with a few alterations, and is printed at the end of this report.

(7) When the Technical Optics Committee met after the vacation, the other matters, apparently of more immediate urgency, referred to in this report, had arisen and took precedence of the earlier matter. In investigating these new questions it has become strongly evident that the earlier matter is of *supreme and pressing importance.*

The guild therefore recommends:—

(A) That better provision should be made at the National Physical Laboratory for the testing of samples of glass as to their physical and optical properties, and that the director of the National Physical Laboratory be approached on the subject.

(B) That facilities should be provided as speedily as possible for the carrying out, at the National Physical Laboratory, or elsewhere, of the researches connected with the manufacture of optical glass referred to in this report.

(C) That steps should be taken as speedily as possible to give effect to the recommendations of the previous report of the Technical Optics Committee of the guild in the direction of providing facilities for systematic, scientific, and manual training in technical optics, and the guild, recognising that educational training requires time, is strongly of opinion that this question is urgent and that the organisation of optical training should be taken in hand at once.

PREVIOUS REPORT REFERRED TO IN PARAGRAPH C, DATED JULY 14, 1914.

Proposed Establishment of an Institute for Technical Optics.

The British Science Guild has had under consideration for some time the inadequate provision for, and the unsatisfactory state of, the technical training in optics in the British Isles. The subject was brought to its notice by Sir Thomas Barlow, formerly president of the Royal College of Physicians, in a communication to the president of the guild, and was considered to be of such importance by the Executive Committee that a Special Committee was formed to deal with it. The Special Committee has now reported.

The establishment of such an institute has been under discussion for some years, and there is a remarkable consensus of expert opinion, both as to the necessity and the urgency for action, from many and diverse points of view, scientific, industrial, and national.

The London County Council, which has gone into the matter very thoroughly, has not felt itself in a position to provide from the funds under its control the initial capital expenditure of some 40,000*l.* for the erection and equipment of the proposed institute, although a site was actually purchased for the very purpose by the governing body of the Northampton Polytechnic Institute so far back as 1908. The Finance Committee of the council is understood to be of opinion that the project is so essentially of national importance that it would be unfair to saddle London ratepayers with the whole cost. It is, however, believed that if the question of capital expenditure can be solved, the maintenance of the institute could be assured by grants from the Board of Education and from the London County Council, and by students' fees. As an additional reason for expedition, it may be pointed out that the governing body of the Northampton Polytechnic Institute may not be in a position to carry much longer the heavy burden of the mortgage interest on the purchase money above referred to, and the amortisation of the capital amount.

GEOLOGY IN RELATION TO THE EXACT SCIENCES, WITH AN EXCURSUS ON GEOLOGICAL TIME.[1]

IT is often said that figures can be made to prove anything; and certain it is that a series of arithmetical operations does sometimes serve as introduction to very strange conclusions. The fault, of course, is not in the tool, but in the hand that uses it. In the larger issues of geology especially, where the gulf to be bridged between data and conclusions is so often a wide one, ingenuity of reasoning ought surely to be accompanied by a due sense of responsibility in the handling of figures. Calculation, in such applications, is by no means so simple an art as it may appear. In wrestling with problems of the kind indicated, and, I must add, in reading some very fascinating speculations by geologists of high standing, I have often wished that some obliging mathematician would put forth a small manual of applied arithmetic for the guidance of workers in the descriptive sciences. There are absolutely necessary precautions to be observed when calculation is based upon data always partial and at best roughly approximate, and these precautions are too often neglected. To be safe, we must have some conception of the probable error attaching to our observations, and we must note how the initial errors may be multiplied in the process of calculation. Especially there is the cumulation of error which must ensue when results obtained in this fashion are used as links in a chain of deduction. Here it is quite inadequate to say that the chain is no stronger than its weakest link; it is of necessity far weaker than its weakest link.

[1] From a Presidential Address delivered before the Yorkshire Geological Society by Alfred Harker, F.R.S. Reprinted from the Proceedings of the Society.

Without entering into these matters, some of which, as I have suggested, call for expert aid, I will take for illustration a single point, the frequent abuse of *the average*. Say that we wish to determine the amount of mud annually carried down by the Nile. Since there are variations, both seasonal and casual, we must take a sufficient number of observations, properly distributed in time, and an average, duly weighted, will then give us as good a result as the nature of the case admits. But now suppose that we wish to know the amount of sediment carried by all the rivers of the world. We have data for nine rivers, data which are likely to differ much in respect of probable error. Accepting them, however, as they stand, it appears that the water of the Rio Grande carries one part in 291 of sediment, that of the Uruguay River only one in 10,000, the other seven rivers giving intermediate values. The highest figure is thus thirty-four times as great as the lowest. Some geologists will simply take a mean of the nine figures, and proceed contentedly to use this result in the most far-reaching conclusions. I do not believe that a mean of nine figures so discordant can afford any information of quantitative value. The average must be extended over a much wider area, before a result is obtained of which we can legitimately make use.

Where dynamical principles enter into the problem, the pitfalls which await the unwary are sometimes less evident. I will take as an illustration the case of models, such as have been constructed to elucidate the mechanism of folding and faulting. In no case, so far as I am aware, have geologists had regard to the conditions which are necessary in order that a model may correctly represent the working of the original. The various forces concerned must bear their proper ratios. Since the weight (for a given material) is reduced proportionally to the cube of the linear dimensions, the other forces must be reduced in the same ratio; and it is, in fact, impossible to make this adjustment as regards the internal forces which resist deformation and fracture. Moreover, the velocities of the moving parts should be reduced in proportion to the square root of the linear dimensions; and this makes it hopeless to think of imitating the slow processes of mountain-building. Models of this kind may afford useful *geometrical* illustrations, but can throw no light on *dynamical* problems. The same remark applies to models of glaciers; but here there is no need to go to artificial models to illustrate my point. Some geologists still argue from the behaviour of an Alpine valley-glacier to that of a continental ice-sheet, without perceiving how completely the different scale of magnitude must modify the mechanical conditions.

Experiment has undoubtedly afforded valuable help in the study of particular questions in the domain of physical geology, and this is to be recognised with gratitude. As regards the larger and more complex problems, however, imitative experiment labours under the same disadvantage as mathematical analysis. Any concrete problem can be treated only in an arbitrarily simplified form, and among the conditions which cannot be realised in the laboratory may be some which in nature are of vital importance. Especially will this be the case where the time element enters.

There is, however, another department of experimental geology in which we are justified in expecting results of very high value. I allude to the study of the conditions of formation and stability of different minerals, with the object of elucidating the mode of origin of igneous and other rocks. The artificial reproduction of many of the rock-forming minerals has engaged the attention of chemists, especially in France, during the last hundred years. Fouqué and Michel-Lévy succeeded even in imitating some of the simpler types of igneous rocks. These researches have furnished the petrologist with useful information, but it is information mostly of a very general kind. The laborious investigations now being carried out, more particularly in the Geophysical Laboratory of the Carnegie Institution of Washington, are of a different order, systematic and precise to the highest degree attainable. Their chief object is to apply to the crystallisation of igneous rock-magmas the methods which have proved so fruitful in other branches of physical chemistry. This necessitates working over a far wider range of temperatures than is usual in laboratory operations, and must sometimes include high-pressure work also. It involves, too, other practical difficulties, arising especially from the slowness with which equilibrium is established in many of the transformations investigated. Owing perhaps to these obstacles, and partly, it may be, to the scarcity of enlightened millionaires—for expense is here a weighty consideration—research on these lines has not yet been widely taken up. Meanwhile it is scarcely too much to say that Dr. Day and his colleagues at Washington are already laying the foundations of an exact science of petrogenesis.

Of all geological questions involving the numerical element, none has been more frequently canvassed than the problem of geological chronology, and none has excited more general interest. Since, moreover, it introduces several points germane to my subject, a brief glance at its history and present state will not be wasted. I suppose it has happened to most of us, when relating how in past times the mammoth roamed the plains of Holderness, or how coral-reefs once flourished where the Craven hills now stand, to be met by the inquiry: How long ago was that? The answer was perhaps to the effect that geology does not deal with the ordinary measures of time, but has its own system of chronology, not translatable into years and centuries. I must confess, however, to a sense of inadequacy in such a reply, and some sympathy with the lay inquirer who is thus silenced but not satisfied. It seems a matter of reasonable regret that a science which deals with the history of past events should have no definite time-scale, by which those events could be ranged in a correct perspective.

No such reflection, it is safe to say, disturbed the minds of the early Uniformitarians, the founders of modern geology. Their reaction against the older catastrophic school led them constantly to lay great stress on the extreme slowness of geological processes, and they thus came to assume unlimited time for the past changes to which the stratified rocks bear witness. To Hutton there was "no vestige of a beginning, no prospect of an end"; in other words, he regarded geological time as infinite, and could no more contemplate reckoning it in centuries than numbering the sands on the shore. Later this position was reinforced from another quarter, as Darwin's doctrines gained acceptance; for these were held to push back to an immeasurably remote epoch the beginning of life on the globe. Geologists and biologists alike saw no reason for limiting their prodigal drafts on the bank of time.

From this comfortable attitude they were startled, as by a bombshell, some fifty years ago, when William Thomson, afterwards Lord Kelvin, published the first of his contributions from the mathematical side to this and cognate subjects. He pointed out that, apart from any changes on the surface of the globe, our planet as a whole must be undergoing a change of a secular, and so irrevocable, kind; viz., a continual loss of energy in the form of heat, as proved by the observed temperature-gradient. Since the store of energy cannot be inexhaustible, we must deduce both a beginning and an end of the existing geological *régime;* and Thomson endeavoured to set a limit to its past duration from a discussion of the rate of cooling of the globe. A parallel line of argument was based on the cooling of the sun.

Now as regards the validity of the general criticism there can be, of course, no doubt. Huxley's halting defence of what was then the orthodox position was easily broken down, and a wholesome check was given to the extravagance of the geologists. When we turn, however, from the destructive to the constructive part of Kelvin's argument, the case is different. The time to be allowed for the geological record was stated at first with considerable latitude, but was afterwards narrowed down, until, in 1899, Lord Kelvin concurred in Clarence King's conclusion that the globe was a molten mass about twenty-four million years ago. It is rather remarkable that so many geologists were found willing to submit to this narrow limitation. Doubtless they were impressed by the prestige of Lord Kelvin's authority, and perhaps some of them were influenced by a vague feeling that a result arrived at by strict mathematical reasoning is thereby entitled to credence. But, as has been so often pointed out, and so often forgotten, what you get out of the mathematical mill depends upon what you put into it. The reasoning may be unimpeachable, but it merely proves that, if certain assumptions be granted, certain consequences will follow. It may be that Lord Kelvin himself, in the enthusiasm of enforcing his conclusions, did not always recall the foundations on which they rested, and it is to be suspected that many geologists read no more than the conclusions.

Kelvin's argument was based necessarily upon a number of assumptions. At the present time, in the light of fuller knowledge, it is sufficient to note one, which in 1862 seemed little open to question. Kelvin recognised that, while the earth is certainly losing heat, "it is possible that no cooling may result from this loss of heat, but only an exhaustion of potential energy, which in this case could scarcely be other than chemical affinity between substances forming part of the earth's mass." This, however, he dismissed as "extremely improbable," and proceeded on the assumption that heat is the only form of energy to be reckoned with. Since the discovery of radium we have learnt that the earth possesses a vast store of potential energy in a highly concentrated form then unsuspected. Strutt has calculated, from data of a very simple kind, that the observed temperature-gradient can be wholly accounted for by radio-activity, if the rocks to a depth of forty-five miles contain as much radium as those at the surface. In other words, the heat generated by radio-active changes within this relatively thin crust will, on that supposition, be sufficient to compensate that lost at the surface. Clearly, therefore, the actual rate of cooling of the globe—if indeed it is cooling—must be far less than that adopted in Kelvin's calculation, and his estimate of the age of the earth must be enormously increased.

This is not all. A study of the various radio-active elements contained in minerals and rocks has shown that it is possible, in certain favourable cases, to calculate directly their age in years. Some estimates of this kind have been made, and the results are liberal enough to satisfy the most exacting claims of what may be called the reformed Uniformitarian creed.

With this turning of the tables one might suppose that the old controversy would come to an end. But the reversal of the situation is, in fact, more complete; for meanwhile there has arisen a formidable minority of geologists who contend, on geological grounds, for estimates of time no more elastic than Lord Kelvin's. The question is still, in great part, one between geologists and physicists, but it is now the geologists who offer us the stinted measure and the physicists the more liberal one.

It is not my purpose to discuss in detail the various geological arguments which have been advanced for limiting the age of the earth to a span of 80 or 100 millions of years. The method of procedure is broadly the same in all. A computation is made of the rate at which some fundamental geological process is going on; it may be the lowering of the land-surface by erosion, or its destruction by solution, or the deposition of sediment, or the addition of salt to the sea. Some estimate is then made of the total result of the process throughout geological time. Having the annual rate of increment and the total amount, simple division gives the measure of the time in years. The observational data employed in these calculations are of a very precarious kind, and it would not be difficult to point out instances of that levity in the handling of figures to which I have adverted. But the fundamental weakness of all such reasoning lies in the assumption that the present rate of any of these geological processes can be adopted as equivalent to its average rate throughout the whole time.

The existing configuration of the globe, and all the physical conditions that go with it, have been attained in consequence of a prolonged evolution. If we believe that, as the net result of all its vicissitudes, the land-area has on the whole been growing in extent, in complexity of distribution, in boldness of relief, we must believe also that differences of temperature, of humidity, of climate generally, between different parts of the globe have become progressively more accentuated, and that all geological activities have been quickened as the world has grown older. While there is difference of opinion concerning these *secular* changes, there can be no doubt as regards the great *cyclical* changes which have been repeated several times in the history of the earth: the cycle beginning in each case with an epoch of important crust-movements and including the train of consequences which follow upon this new step in the evolution of the earth. Such a cycle was initiated at an epoch not long remote by geological reckoning, and we are living in consequence in a time of more than ordinary geological activity, with the continental masses rising higher than their average level, and with large tracts of newly deposited strata exposed to the attack of destructive agents.

For these reasons I am of opinion that the present rate of erosion, and of its correlative sedimentation, is much higher than the average rate, and that any calculation based upon it must greatly under-estimate the duration of geological time. I do not ask you necessarily to concur in this conclusion, but at least to suspend judgment in the matter: for it will assuredly be a misfortune if geology, so lately freed from one bondage, should fall straightway into another no less galling. This at least is certain, that every one of the various geological processes which have been discussed in this connection, is controlled by conditions which cause its rate to be very variable. It is a clock which now hurries and now creeps, or stands still, and it can never be trusted as a time-keeper. Even for the most recent chapter of geological history we can make no approach to certainty on these lines. Attempts have been made, for example, to estimate the time since the final retreat of the ice in North America from the rate of recession of the falls of Niagara; but the evidence shows that this rate has varied widely even during the last half-century, and Gilbert, after a careful study of all the data, refrains from offering any opinion on this point.

Must we then abandon all hope of any practicable measure of time in geology? I do not draw this conclusion, but rather that we must search outside strictly geological phenomena for some physical process of which the rate is not affected by any disturbing conditions. There are, I think, only two classes of changes for which so much can be claimed—the transformations of the radio-active group of elements and the astronomical movements. It seems not improbable that in one or other of these two directions the solution of the problem may eventually be found.

The chemists have taught us that radium is derived from the spontaneous breaking up of uranium, the change taking place apparently in two stages and involving the liberation of three atoms of helium. But radium itself disintegrates spontaneously, giving the radium-emanation named niton and liberating another atom of helium. Niton in its turn undergoes disintegration, and so on through a succession of changes. The final product is lead, and in the gradual conversion of uranium to lead eight atoms of helium in all are set free. Of these various spontaneous changes some proceed with extreme slowness, others with comparative rapidity; but in each case there is a constant rate which, so far as experiment has tested it, is independent of temperature or pressure.

Prof. Strutt has shown that this gradual liberation of helium can be made the basis of a method of estimating the absolute ages of minerals and rocks. For example, phosphates and some iron-ores are rich in radium, derived from uranium. They also contain helium, and the ratio of helium to uranium is found to be higher in the older deposits. Estimates of age calculated from these data give high figures: *e.g.* the age of the hæmatite overlying the Carboniferous Limestone in Cumberland is given as 140 millions of years, and even that of the Eocene iron-ores of Antrim thirty millions. The results show some irregularities, and it is, of course, admitted that the method has its own difficulties. If, however, the chief source of error is, as appears probable, the loss of helium by leakage, the figures found will be under-estimates. Helium comes from the thorium series of derivatives as well as from the uranium series, and this is to be taken into account where thorium is found. Zircons from various igneous rocks have also been examined by Strutt, and found to give consistent results as regards the helium-ratio. Mr. A. Holmes has approached the question in a different way, by considering the ratio of lead to uranium in various minerals rich in the latter element. The igneous rocks of the Christiania district, of Devonian age, are in this way calculated to be about 370 million years old. For the Archæan rocks of different countries the estimates range from 1000 to 1600 millions of years. Holmes's results are in general nearly twice as high as those of Strutt; but, if we bear in mind the error due to the escape of helium, which is proved to take place, a discrepancy to this extent is no more than should be expected at this early stage of the inquiry.

The other method which has been suggested for obtaining an absolute measure of geological time is of a more speculative kind, although the principle of it is sufficiently simple. It consists in detecting some clearly marked rhythm or cycle in the geological record, and correlating it with one of the known periodic movements of the earth. It was on these lines that Croll attempted to explain the recurrent glacial epochs; but more to our present purpose is the theory which Blytt has based upon a study of the alternations observed in a succession of sedimentary strata. The most important astronomical cycle of long period is doubtless that which depends upon the precessional movement, by which the relation of summer and winter to perihelion and aphelion is gradually changed. This involves a change in the relative lengths of summer and winter, and must undoubtedly exercise a marked effect upon climatic conditions, though there is much difference of opinion concerning the exact nature of this effect. Changes of climate may in their turn cause differences in the nature of the sediments deposited successively at a given place, differences which will repeat themselves in a cycle corresponding with that of the precession. Probably the most noticeable effect will be a recurrence of limestones and chemical deposits alternating with detrital sediments.

If the matter were no more complex than this, it would be sufficient, where such alternations can be detected, to count them, as we count the rings of growth of a tree, and reckon 21,000 years for each sedimentary cycle, that being the period of the precession corrected for the movement of the perihelion. If the alternations can be distinguished only in some parts of the succession, some hypothesis must be devised to take account of the intervals. Gilbert has discussed in this way a succession of beds, 3900 ft. thick, forming part of the Cretaceous system in Colorado. Alternations of calcareous beds with shales come in four times, being separated by unbroken thicknesses of shale. Gilbert calculates for the part of Cretaceous time represented a duration of about twenty million years, with an uncertainty indicated by the number 2 as a "factor of safety."

We have to remember, however, that sedimentation is controlled by other conditions besides climate, and climate depends upon other causes besides the precession of the equinoxes; and, further, that most of these contributing causes cannot be described as periodic in any intelligible sense. There is, it is true, a second astronomical movement to which both Croll and Blytt have made appeal, viz., the variation in the eccentricity of the earth's orbit. This goes through a period of about 90,000 years; but there are considerable irregularities which repeat themselves in the course of 1,450,000 years, giving a larger cycle which embraces sixteen of the smaller cycles. The change of eccentricity must modify the effect of the precessional movement; but Blytt argues that it will also react on the ellipsoidal shape of the globe itself, and so give rise to a displacement of shore-lines. He claims to have traced this effect, as well as the climatic cycle, in such cases as the succession of the Tertiaries in the Paris basin and the Isle of Wight. His conclusion is that Tertiary time comprises two of the larger cycles, *i.e.* about three million years.

It has usually been assumed that the year is too short a period to leave any recognisable mark on the geological record. This is probably true in general, but in certain favourable circumstances it may perhaps be possible to count annual layers of sediment. De Geer has recently attempted this in the case of certain finely laminated clays of late Glacial and post-Glacial age in Sweden. The material was brought down by sub-glacial streams at a time when the ice had retreated to the higher ground. Consequently the seasonal variations were strongly marked, and the accumulation of sediment was rapid enough to yield an appreciable thickness in each year. From such data De Geer has estimated that the recession of the last ice-sheet occupied a duration of about 5000 years; and he further gives 7000 years as the lapse of time since the recession of the ice.

As regards the longer astronomical cycles, it is clear that the argument involves a large element of hypothesis, and its application, as Blytt allows, is beset with practical difficulties. It possesses a special interest as lending a new significance to the details of stratigraphy, but as a means to the establishment of a geological chronology its value is at present only potential. The radium method of evaluating geological time seems to offer more immediate promise.

In conclusion, it is pleasant to note how these applications of chemistry, astronomy, and meteorology, not merely to general principles of geology but to a definite geological problem, emphasise the fundamental unity of the sciences, and illustrate the powerful aid that may be rendered by one to another.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

DR. H. ROY DEAN, professor of pathology in the University of Sheffield since 1912, has been appointed to the chair of pathology and pathological anatomy in the University of Manchester.

DR. ADA E. MILLER has been appointed lecturer on school hygiene by the Edinburgh Provincial Committee for the Training of Teachers, in succession to Dr. I. Douglas Cameron, who has resigned.

IT is stated in *Science* that the committee on education of the United States House of Representatives has reported favourably a Bill establishing a National University in Washington. According to the Bill an initial grant of 100,000*l.* would be made. The university would be devoted to research and graduate work and no degrees would be conferred.

A REUTER message from Delhi reports that on March 22 Sir Harcourt Butler introduced in the Imperial Legislative Council a Bill for constituting a teaching and residential university at Benares, with special facilities for instruction in the Hindu religion. He referred to the scheme as the commencement of a new era in university organisation in India.

DR. ALEX. HILL, principal of the Hartley University College, Southampton, is reported by the *Times* to have said in an address on Monday that he had recently been preparing a war-roll of the Empire universities, and had found that the average contribution in men from universities and university colleges was just above 50 per cent. He added:—"It is a surprising fact that the contribution of German universities to the forces of the German Empire in the field is less than 20 per cent." This statement as to German university students is not, however, correct, judging from the statistics we gave last week (p. 81). Seventy-five per cent. of the students of German universities are in the field, and about 80 per cent. of the students of the Technical High Schools are also on active service.

SIR PHILIP MAGNUS retires, we understand, to-day from his official connection with the City and Guilds of London Institute. It is now no fewer than thirty-five years ago since he was appointed organising director and secretary of the institute, a post which he held for eight years, during which he was responsible for the initiation of the institute's work and for the schemes of the Finsbury College and Central Technical College, which have since developed so successfully. In 1888 his activities were transferred to the examinations department, or, as it is now known, the department of technology of the institute, where they found a wider field in assisting and guiding the development of technical instruction all over the country. The ability of his organising powers is sufficiently evidenced by the manner in which the department, without any assistance from Government and without the power of the purse possessed by a department of State, has made the name of the City and Guilds of London Institute known to technical schools all over the British Isles, and, indeed, in the Dominions beyond the Seas. To the work of Sir Philip Magnus in the office which he is vacating, his careful insistence on the necessity of making technical instruction a true education in principles, his continual study of the best means of adapting courses to the needs of students and manufacturers alike, and his unceasing endeavours to raise the standard of teaching, the progress of technical education in this country is greatly indebted.

THE first annual report, for the period ended December 31, 1914, submitted by the executive committee to the trustees of the Carnegie United Kingdom Trust has now been published. Mr. Carnegie during many years prior to 1912 gave large sums to local authorities in this country for the erection of public libraries, and to churches for the acquisition of organs. As the applications for these grants increased and their administration became more difficult, Mr. Carnegie decided to place the future administration of grants under the control of a permanent body of trustees. In 1913 he placed 2,000,000*l.* in trust so that the income of about 100,000*l.* a year should be available "for the improvement of the well-being of the masses of the people of Great Britain and Ireland." The report is full of interesting particulars, but attention can be directed only to a few typical facts. Organ grants are to be discontinued for the present. Mr. Carnegie has already expended 550,000*l.* in this direction in the acquisition of some 3500 instruments. A total expenditure of nearly 2,000,000*l.* has been incurred already on the erection of public libraries in the United Kingdom. The executive committee has, we notice, intimated to the authorities of the Household and Social Science Department of King's College for Women, London, that it is prepared to meet half the cost of the erection of a library building, on certain conditions. The committee has also made an offer in the direction of endowment to the Central Bureau for the Employment of Women. The report throughout gives the impression of wise and sympathetic administration of a princely endowment.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 18.—Sir William Crookes, president, in the chair.—Prof. W. H. **Bragg**: Bakerian Lecture: X-rays and crystalline structure. The atoms of crystal may be conceived—in various ways—as arranged in a series of parallel planes, each capable of reflecting a small fraction of an incident pencil of X-rays. If the spacing of the planes is d, the wave-length λ, and the angle between the rays and the planes is θ, and if the relation $n\lambda=2d\sin\theta$ is satisfied, where n is any integer, then the various reflected pencils are in the same phase and combine to give an obvious reflection of the X-rays. If this relation is not satisfied there is no reflection. The X-ray spectrometer is designed to measure the various values of θ at which reflection occurs in a given case. The angle can easily be determined to a minute of arc. Given d we can compare the wave-lengths of different X-rays. Given λ we can compare the spacings of various sets of planes of the same or of different crystals. By certain considerations the experiments can be made absolute and not merely comparative. In this way the structures of several simple crystals have already been found, such as rock-salt, diamond, iron pyrites, and so on. The reflections for various values of n, the