

recently made at Lord Rosse's request to provide further instrumental power for his lunar work.

Lord Rosse had been Chancellor of the University of Dublin since 1885, and he served as president of the Royal Dublin Society (1887-1892) and president of the Royal Irish Academy (1895-1900). He was also one of the visitors of Greenwich Observatory.

Lord Rosse married in 1870 the Hon. Frances Cassandra Hawke, only child of the fourth Lord Hawke. He is succeeded by his eldest son, Lord Oxmantown, who was born in 1873. His second son is the Hon. Geoffrey Laurence Parsons, and his daughter, Lady Muriel Parsons, was married in 1906 to Colonel H. M. Grenfell, C.B.

THE DUBLIN MEETING OF THE BRITISH ASSOCIATION.

ONE of the largest and most successful among recent meetings of the British Association has just been concluded in Dublin. The following return shows the number of tickets issued in the various classes of members:—

Old life members ... ..	288
New life members... ..	24
Old annual members ... ..	459
New annual members ... ..	111
Associates ... ..	1,152
Ladies ... ..	222
Foreign members ... ..	14
<hr/>	
Total ... ..	2,270

The various sections began work on Thursday, September 3. Most of them were located within the ample walls of Trinity College, and outlying centres were easily reached by means of the free motor service organised by an indulgent local committee. That service, creditable as it was, was greatly surpassed in value by the indicator boards announcing what papers were "up" in the various sections. These boards were mounted in places visible to everybody present. The letters A to L were written in large type in a horizontal row, and underneath each letter was hung a card bearing a number indicating the paper just being read in the section denoted by the letter. The service was maintained by four special operators per section, and it enabled members to bide their time comfortably in any section until their favourite paper took its turn in another. This useful innovation must be put to the credit of Prof. W. H. Thompson, one of the local hon. secretaries.

Thursday's sections began in a downpour of rain which contrasted unfavourably with the sunshine of the day before. The intersectional motor service was little in request, as nobody cared to leave the sheltering roof once it was reached. The various sectional meetings were, however, well attended, notably those of geology, educational science, and agriculture, the average attendance being about 150 per section.

About fifty members visited Guinness's Brewery at St. James's Gate at noon, and were shown over the vast works by the principal members of the scientific staff.

The Provost's garden party in the afternoon was largely attended in spite of the prevailing drizzle, though many members were absent. The Provost of Trinity College, Dr. Anthony Traill, braved the wintry blast manfully, and stood at the gate of the Fellows' Garden to receive his unexpectedly numerous guests. The latter kept to the marquees and the gravel walks, and enjoyed themselves prodigiously.

The conversazione given in the evening by the Royal Dublin Society at Leinster House proved one of the largest receptions on record. The 3000 mem-

bers of the Royal Dublin Society were, of course, all invited, and as practically all the members of the British Association present in Dublin attended, the number of guests was more than 4000. The queue of carriages extended along several streets, and took two hours to discharge the occupants. On arrival the guests were received by Lord Ardilaun (president of the Society), the Right Hon. Frederick Trench, and Sir Howard Grubb. There were numerous scientific exhibits by local men.

In the lecture theatre Mr. W. H. Vipond Barry gave an organ recital, while subsequently lantern demonstrations were given by Rev. W. S. Greene and Dr. E. MacDowel Cosgrave, the subjects being "Scenes and Incidents in the West of Ireland" and "Old Dublin" respectively.

The Lord Lieutenant, accompanied by several members of his staff, arrived at 9.30, and spent some time inspecting the exhibits.

The sections started in full force on Friday, the weather having cleared up completely. The encounter between Sir William Ramsay and Prof. Rutherford in Section A drew a large attendance of distinguished physicists, but the keenest local interest was evoked by Mr. T. W. Russell's appearance at the section for Economic Science and Statistics, and the discussion on land purchase and the nationalisation of railways in which he took part.

A special meeting of the Senate of Dublin University was held at 2 p.m. for the purpose of conferring honorary degrees. The University Caput consisted of Mr. Justice Madden, Vice-Chancellor, Dr. Anthony Traill, Provost, and Mr. Frederick Purser, Senior Master. As each candidate was summoned to the dais, the Public Orator, Dr. L. C. Purser, proclaimed his titles and qualifications in Latin. The names of those who received degrees are to be found on p. 471 of the present issue under the head of University and Educational Intelligence.

Meeting of the General Committee.

At a quarter-past three o'clock a meeting of the general committee was held at Trinity College, Mr. Francis Darwin occupying the chair, when it was decided to hold the meeting of 1910 at Sheffield, and that of 1911 at Portsmouth.

On the motion of Sir Arthur Rücker, Prof. J. J. Thomson, F.R.S., was elected President for the Winnipeg meeting in 1909, the date of which was fixed for August 25 to September 1.

The afternoon engagements of Friday were divided between the general committee, a garden party at Dunsink Observatory, another at Saint Patrick's Cathedral, a visit to Messrs. Jacob and Co.'s biscuit and cake factory, and a special *matinée* of Irish plays at the Abbey Theatre, where Mr. W. B. Yeats gave an address on the recent development of native Irish drama.

The drive to Dunsink in special brakes was enjoyed by 200 members, who accepted the invitation of the Astronomer Royal and Mrs. Whittaker. The Observatory is situated to the north of Phoenix Park, and is best known to the Dublin public as the centre from which "Irish time" is furnished to the public time-pieces of Ireland. The transit circle was, naturally, inspected with special interest.

Dean Bernard's garden party at St. Patrick's was also well attended, and was distinguished by the presence of the Lord Lieutenant of Ireland.

The Lord Mayor of Dublin took sixty members of the Association in his "flagship," the *Shamrock* (he is admiral of the Port of Dublin), down the Liffey to see the main drainage and electric light works at the Pigeon House, and gave a luncheon on board after the works had been inspected.



In the evening, Prof. H. H. Turner, F.R.S., gave an address on Halley's Comet at the Royal University before a crowded audience.

Saturday was devoted to excursions, in which 1100 persons took part. The objectives chosen were (1) the Boyne valley; (2) Bray, Powerscourt, and Kiltruddery; (3) Glendalough; (4) the Rock of Cashel; (5) the Shannon and Clonmacnoise.

In the evening, the Classical Association of Ireland gave a reception in the Royal College of Physicians, and Dr. A. E. Tutton, F.R.S., gave the annual lecture to the operative classes before a large audience in the Royal University, choosing for his subject "The Crystallisation of Water."

Sunday was observed by special services in the Episcopalian, Presbyterian, and Roman Catholic churches, the attitude of the respective churches towards science being expounded by the various preachers.

The afternoons of Monday, Tuesday, and Wednesday were set apart for garden parties at St. Anne's, Clontarf (Lord Ardilaun's home), the Zoological Gardens, and the Viceregal Lodge respectively.

On Monday, September 7, Prof. W. M. Davis, of Harvard University, gave a largely-attended lecture on "The Lessons of the Colorado Cañon."

The final meeting of the Association took place on Wednesday, September 9, at 3 p.m., at the Royal University.

E. E. FOURNIER.

Subjoined is a synopsis of grants of money appropriated for scientific purposes by the general committee at the Dublin meeting.

<i>Section A.—Mathematical and Physical Science.</i>	
Turner, Prof. H. H.—Seismological Observations.....	60
Shaw, Dr. W. N.—Kites Committee.....	10
Preece, Sir W. H.—Magnetic Observations at Fal-mouth .....	50
Gill, Sir David—Establishing a Solar Observatory in Australia .....	50
<i>Section B.—Chemistry.</i>	
Roscoe, Sir H. E.—Wave-length Tables of Spectra ...	10
Divers, Prof. E.—Study of Hydro-aromatic Substances .....	15
Armstrong, Prof. H. E.—Dynamic Isomerism.....	35
Kipping, Prof. F. S.—Transformation of Aromatic Nitramines .....	10
Kipping, Prof. F. S.—Electro-analysis .....	30
<i>Section C.—Geology.</i>	
Lamplugh, G. W.—Fossiliferous Drift Deposits.....	11
Herdman, Prof. W. A.—Fauna and Flora of British Trias .....	8
Harker, Dr. A.—Crystalline Rocks of Anglesey.....	1
Gregory, Prof. J. W.—Faunal Succession in the Carboniferous Limestone in British Isles .....	10
Kendall, Prof. P. F.—Erratic Blocks .....	12
Lapworth, Prof. C.—Palaeozoic Rocks .....	15
Watts, Prof. W. W.—Composition of Charnwood Rocks .....	2
Watts, Prof. W. W.—Igneous and Associated Sedimentary Rocks of Glensaul.....	20
Joly, Prof. J.—Investigations at Briskra .....	50
<i>Section D.—Zoology.</i>	
Woodward, Dr. H.—Index Animalium .....	75
Hickson, Prof. S. J.—Table at the Zoological Station at Naples .....	75
Herdman, Prof. W. A.—Hereditary Experiments.....	10
Shipley, A. E.—Feeding Habits of British Birds .....	5
<i>Section E.—Geography.</i>	
Murray, Sir John—Rainfall and Lake and River Discharge .....	10
Murray, Sir John—Investigations in the Indian Ocean	35

<i>Section F.—Economic Science and Statistics.</i>	
Palgrave, R. H. Inglis—Gold Coinage in Circulation in the United Kingdom .....	6
Cannan, Prof. E.—Amount and Distribution of Income .....	15
<i>Section G.—Engineering.</i>	
Preece, Sir W. H.—Gaseous Explosions .....	75
<i>Section H.—Anthropology.</i>	
Munro, Dr. R.—Glastonbury Lake Village .....	5
Myres, Prof. J. L.—Excavations on Roman Sites in Britain .....	5
Read, C. H.—Age of Stone Circles .....	30
Read, C. H.—Anthropological Notes and Queries.....	40
Hogarth, D. G.—Researches in Crete .....	70
<i>Section I.—Physiology.</i>	
Schäfer, Prof. E. A.—The Ductless Glands .....	35
Sherrington, Prof. C. S.—Body Metabolism in Cancer	20
Waller, Dr. A. D.—Electrical Phenomena and Metabolism of <i>Arum spadices</i> .....	10
Hickson, Prof. S. J.—Table at the Zoological Station at Naples .....	25
Waller, Dr. A. D.—Reflex Muscular Rhythm .....	10
Waller, Prof. A. D.—Anæsthetics .....	25
Starling, Prof. E. H.—Tissue Metabolism .....	20
Sherrington, Prof. C. S.—Mental and Muscular Fatigue .....	40
<i>Section K.—Botany.</i>	
Scott, Dr. D. H.—Structure of Fossil Plants .....	5
Oliver, Prof. F. W.—Botanical Photographs .....	10
Darwin, Dr. F.—Experimental Study of Heredity .....	30
Blackman, Dr. F. F.—Symbiosis between Turbellarian Worms and Algae.....	10
Johnson, Prof. T.—Survey of Clare Island .....	65
<i>Section L.—Education.</i>	
Magnus, Sir P.—Studies suitable for Elementary Schools .....	5
Lodge, Sir Oliver—Curricula of Secondary Schools ...	5
<i>Corresponding Societies' Committee.</i>	
Whitaker, W.—For Preparation of Report .....	21
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SECTION B.

CHEMISTRY.

OPENING ADDRESS BY PROF. F. S. KIPPING, D.Sc., Ph.D., F.R.S., PRESIDENT OF THE SECTION.

ON taking the Chair of this Section my first duty is to express my personal thanks to the Council of the British Association for having chosen me to fill this position of honour.

At this meeting the Association is enjoying, not for the first, but for the fourth time, the generous and genial hospitality of the citizens of Dublin; it is my privilege, on behalf of all the members of this Section, to tender our cordial thanks to our hosts for giving us this opportunity of meeting again in the capital of Ireland.

During the past few months we have read in the daily journals—and we sincerely hope it may be true—that there are signs of the commencement of a great development of the resources of this island; as such a desirable event must be closely connected with, and, indeed, may even be dependent on, the vitality of the chemical industries of the country, the moment seems opportune for the consideration of a subject which has a direct bearing on both commerce and chemistry.

Although this Section is chiefly occupied with matters relating to pure science, the discussion of industrial questions is also regarded as one of its important functions; it does not attempt to distinguish pure from applied chemistry, and any problem which concerns either is deemed worthy of its attention.

From this point of view I propose to consider whether any steps can be taken to place the chemical industries of



the United Kingdom of Great Britain and Ireland in a more prominent position than that which they now occupy in the world of commerce.

The subject is not new; it has been dealt with by many, but principally by those more directly interested—prominent members of the Society of Chemical Industry, who are far better qualified to express opinions on commercial matters than am I. It is perhaps presumption on my part to attempt to add anything to what has been said by such leaders of industrial chemistry, but I propose to deal with the subject from a very different standpoint—namely, from that of the teacher in the class-room and laboratory. Even if I fail to make a single suggestion of immediate practical value, the question is one of such magnitude and so many-sided that I feel justified in bringing it under the notice of this Section. It is not merely a matter of money, of a few millions or of a few tens of millions sterling. There are few branches of industry to which chemistry, in one way or another, is not of supreme importance. Whether we look to the great shipbuilding interests, dependent on the progress of metallurgy; to our cotton and linen trades, where cellulose reigns supreme; to our dye-houses or to our breweries, or to any other industry, great or small, there do we find problems in chemistry awaiting solution, and the nation which solves them will not only progress in civilisation and contentment, but will also justly claim to have taken a leading part in the advancement of science.

It is not then in any grudging spirit of envy that we approach this question; recognising the splendid work of men of other countries, rejoicing in the services which they have rendered to the world at large, our only desire is not to lag behind in the general intellectual and industrial advance of nations.

It is unnecessary to trouble you with any detailed comparison of the position which we occupy to-day with that which we have taken in the past. The fiftieth anniversary of the epoch-making discovery of mauve was held only two years ago, and the proceedings are still fresh in our recollection; the pæans of congratulation addressed to the discoverer (now, alas! no longer with us) were marred by a plaintive note, a note of lamentation over our lost industry, the manufacture of dyes. The jubilee of the founder of the colour industry in this country was also the occasion for pronouncing its funeral oration. If this were the full extent of our loss we might bear it with equanimity; but it is not so much what has already gone as what is going and what may go that are matters of such deep concern. Those who doubt the seriousness of our condition may find statistical evidence, more than sufficient to convince them, in the technical journals and in the Board of Trade reports of recent years.

The facts there disclosed show that in the manufacture of "fine chemicals," including perfumes, alkaloids, and crude coal-tar products, as well as dyes, the decadence of our industry is far advanced; in the case of heavy chemicals our position, perhaps, is not quite so serious at the present moment, but the future is dark and threatening. Chemical industries are so intimately connected and dependent on one another that the fate of one may determine the fate of all; the by-product of one process is often the raw material of another. Who, then, can deny that the patience, perseverance, and high scientific skill, which have built up the colour industry abroad, if applied, as they have been and are being applied, to the manufacture of heavy chemicals, will not soon defy all competition from less progressive countries?

Such a possibility is full of national danger. It has been pointed out—and the prophecy cannot be regarded as unduly pessimistic—that from present indications a time will arrive when we shall be dependent on outside sources, not only for our food-supply, but also for our means of self-defence. When nitrates are exhausted, when nitric acid and ammonia are prepared from the components of the atmosphere, when all chemical industries have been so highly developed abroad that they have completely vanished from these Islands, and when their loss has reacted on all our other important industries, then, indeed, shall we feel the pinch of poverty; then, indeed, must we submit to national decay.

Is it possible to remedy the present unsatisfactory state of affairs, and to guard against an ominous future?

During the Perkin Jubilee celebrations Prof. Carl Duisberg answered this question, in so far as it concerns the coal-tar colour industry, by an uncompromising negative. In an able and interesting speech he pointed out that, although the Briton is in general a practical man, he is lacking in patience, in the power of waiting for success; he expects to be compensated in hard cash, and at once, for his work or for his capital outlay. The German, on the other hand, is primarily a theorist possessing endless patience, and works without any immediate prospect of pecuniary reward; he has now learnt to be practical as well, but not at the expense of his ideals. It is to this happy combination of qualities that Prof. Duisberg ascribes the success of his countrymen in the coal-tar colour industry—a success which he considers we are powerless to emulate, with which it would be futile for us to try and compete.

With this view that our chemical industries must submit to gradual extinction, even when it is held by so high an authority, we cannot and must not agree; if one nation can learn to be practical, we—the four nations of these islands—one or all, can learn to be plodding and patient, and to appreciate the importance of theory. We may be encouraged in our efforts to do so by the opinions of others, countrymen of Prof. Duisberg, eminent in pure or applied science. Prof. Ostwald, discussing this subject, said that he was sure the difficulties were considerable only in the beginning,<sup>1</sup> while Prof. Lunge, in an address to the Royal Institution,<sup>2</sup> made use of the following words: "Seeing that in pure science the people of Great Britain have never lagged behind any other nation, and that, on the contrary, the land of Newton and Faraday has been a beacon to all others at more than one epoch, there is absolutely no valid reason why she should now, or at any other time, be behind any other in the combination of science with practice."

Here, indeed, is encouragement, and from one who has had ample opportunity for studying the conditions which obtain in this country. Surely, therefore, we ought to have some confidence in ourselves and try our best to regain a strong and healthy position rather than fold our hands in a spirit of hopeless resignation.

The new Patent Act which came into force this year, and for which the country is so much indebted to the strenuous advocacy of Mr. Levinstein and Sir Joseph Lawrence, seems to many to have inaugurated a new era, and to have removed one of the principal causes of the decline of our chemical industries; if this be so, it is all the more important that the representatives of chemical science should be ready and willing to join hands with the manufacturers in order to assist in the process of regeneration.

The principal changes which have been introduced by the new law are, of course, familiar to all. The most important one, which came into operation on August 28 last, is that which requires that the article or process which is protected by the patent must be manufactured or carried on to an adequate extent in the United Kingdom after the expiration of four years from the date of the patent. If this condition is not fulfilled, any person may apply for the revocation of the patent.

Some of the results of this amendment, and some indications of the great industrial changes which it will bring about, are already obvious. Foreign firms or individuals who hold British patents and who have not sufficient capital to work them in this country, or who do not think they are worth working here, are attempting to sell their British patent rights. Others are building or buying works in Great Britain, and it has been estimated that in the immediate future a sum of at least 25,000,000*l.* of foreign capital will have been thus invested in order to comply with the new law.

We need not stop to consider the economic effects of this transfer of capital on the general trade of this country, but we may well pause a moment in order to try and forecast the consequences of these new conditions in so far as they concern our chemical industries.

The prospective establishment of branches of two of the largest German chemical works at Ellesmere Port and at Port Sunlight respectively are already matters of common

<sup>1</sup> *Journ. Soc. Chem. Ind.*, 1906, 1019.

<sup>2</sup> March 15, 1907.



knowledge, and it may be presumed that these firms will avail themselves to a large extent of British labour. If this be the case, and if they are successful—as they, no doubt, will be—the complaint that the inferior technical education of our artisans is responsible for our lack of success will thereby be proved to be groundless. Even if we admit that at the present time the British workman is an inferior operative in a chemical works, and only capable of undertaking the less-skilled labour, these firms will gradually raise a considerable number of trained men who will be ready to undertake more responsible duties under our own manufacturers when the good time comes; a school for chemical operatives will be created in our midst, and, as in the past, we shall reap the benefit of knowledge and experience brought to our shores. It also seems reasonable to expect that, as is the case abroad, these works will be equipped with laboratories and staffed by chemists, although possibly only so far as is necessary for routine work. Many of these chemists may settle permanently in our midst, become members of our Chemical Society and Society of Chemical Industry, and thus infuse us with their patience and perseverance. It is not beyond the bounds of possibility that these great firms may even employ British chemists in their works, if we can supply men sufficiently well trained to be of value. On the other hand, as experience seems to have shown that industrial chemistry cannot succeed with imported scientific labour, it is not very probable that many posts in the laboratory will be filled by our countrymen, who, in this connection, must be regarded as foreigners.

Now at the present time most chemical products can be manufactured more cheaply abroad than here, otherwise we should not have any reason to consider our position. Dr. Duisberg told us that even when an important firm in England had a licence to work all the British patents of two of the largest German colour works, merely paying for the privilege a small percentage of the net profits, it failed to take any advantage of the opportunity. If, then, in this free-trade kingdom production is cheaper than abroad, the foreign firms which have branches here will be in a position superior to that which they now occupy in their own countries. If, on the other hand, owing to inefficient labour, higher wages, freights, and other economic conditions, production is more costly, the superior efficiency and scientific organisation of these foreign firms will nevertheless enable them to command our home market with the goods made here, and to cut us out in the world market, as they do now, with those made abroad.

The conclusion which thus seems forced upon us is, that, although the new Patent Act will prove to be of great value in many respects, it will do little to foster British chemical trade and the development of British chemistry; it places us on an equality with other countries as regards patent rights, and thus remedies an outstanding grievance; but, unless we have something to patent, this equality will be valueless and our chemical industries will continue to decline, possibly more rapidly than heretofore.

Let us therefore pass in review the other causes which have been suggested as contributory to our failure; after eliminating those connected with freights and tariffs, and with the alleged supineness of the Government in assisting industry, matters which may be left to the manufacturers to deal with, there still remain several which are well within the purview of this Section.

These are: (1) the unsatisfactory condition of secondary education; (2) the nature of the training which is given to chemists in our universities and other institutions; (3) the insufficiency of the time and money devoted to research in the manufacturing industries; (4) the lack of cooperation between manufacturers and men of science.

There are some who believe that the first of these is the primary, if not the sole, cause of our weakness; that if our secondary education were placed on a sound basis all the other evils would disappear of their own accord; that a steady and broad stream of well-trained boys from the secondary schools would afford ample material from which good chemists could be fashioned in the universities and colleges; that these trained chemists would be greedily seized by the manufacturers, whose minds had been widened by improved educational methods; and once

installed in the works these chemists would have no difficulty in persuading their employers to spend time and money on research work in cooperation with the leaders of science.

Whether such desirable and far-reaching results would in fact follow if our system of secondary education were very much improved it is impossible to predicate; but there is no doubt that at the present time we are moving in an exactly opposite direction.

The shadow of the cypress rests upon our chemical trade, and manufacturers do not see their way to employ chemists; students are not attracted to chemistry as a profession because there are so few openings; without an ample and increasing supply of such students chemical industry must continue to decline, and as a necessary consequence the development of pure chemistry is cramped and hindered to a far greater extent than is generally realised.

In a Presidential Address to the Chemical Society last year Prof. Meldola discussed the position and prospects of chemical research in Great Britain, and in view of the importance of the subject and the able manner in which it had been treated the Council of the Society ordered the publication of five thousand copies of his Address for distribution among the members of various public bodies. We were told in this Address that many of our universities are distinct failures as centres of chemical research, and that the output of original work from our colleges, polytechnics, and similar institutions is emphatically not representative of the productive power of the teachers there employed. The causes of the failure of our universities were only lightly touched upon, and I propose to refer to them later; but in the case of our other institutions they were more fully discussed. May I venture to direct attention to one cause, which I believe is by far the most effective drag on research in the vast majority of such institutions not of university rank? It is simply the lack of those more advanced students who, while gaining valuable experience in the methods of research, would also render useful assistance to their teacher. The governing body of the institution may not realise the importance of research; the Principal, as, alas! is sometimes the case, may throw cold water on such work; the teacher may be overburdened with routine duties, and he may be most inadequately remunerated; if, however, the research spirit is strong within him, he would overcome all these difficulties were there any prospect whatsoever of success; but what chance has he when he must do everything himself, even to washing out his own test-tubes? Provide him with a few advanced students, and he would doubtless find time to undertake the necessary pioneer research work, which would then be extended and developed with their assistance.

It might be suggested that an efficient and enthusiastic man would soon attract a number of research students. This, no doubt, is true as regards the universities, but it must be remembered that a polytechnic or other institution which does not grant degrees can hardly expect to compete with a university as a centre for research; all those students who intend to undergo a so-called "complete" course of study—that is to say, all who are likely to become capable of undertaking research work—naturally proceed to one of the degree-giving universities. There are not enough students to go round, to satisfy the research requirements of the teachers, and the principal reason is—the limited demand for trained chemists on the part of the manufacturers.

Even of the small number of those who leave our teaching institutions fairly well trained in research, how many have a chance of passing into works and directly advancing applied science? A very small proportion indeed. Most of the better ones drift into other posts, become demonstrators, emigrate—anything rather than wait on with the prospect of accepting as 'works-chemist' a salary which, meagre though it be, may be stopped altogether if dividends are low.

With whom rests the responsibility for this state of affairs? Is it with the teachers, and, if so, is it because they are incapable of training chemists or because their system is at fault?

To answer this question it is necessary in the first place



to arrive at some conclusion as to the kind of training which is required for the future works-chemist. On consulting the opinions of the manufacturers it would seem that they attach great importance to what is called the "practical side"; they believe that, in addition to a knowledge of theoretical chemistry, the prospective works-chemist should also have some acquaintance with engineering, should understand the apparatus and machinery used in the particular manufacturing operations with which he is going to deal, and should have had practical experience in working the given process. It is from this point of view that we build and equip large technological chemistry departments, such as those in the Universities of Birmingham and Leeds and in the Manchester Municipal School of Technology, departments fitted up with complete apparatus and machinery for carrying out operations on a miniature manufacturing scale.

The arguments in favour of this view, that it is a hybrid chemist-engineer who is required in a chemical works, seem to me to be fundamentally unsound, and the kind of training suggested by them for the works-chemist can only result in the production of a sort of combined analytical machine and foreman. A two or three years' course of science, followed by one year's practical work in the dye-house, in paper-making, or in some other technological department, is quite inadequate if the student trained in this way is expected to do anything beyond routine analytical work and supervision.

We cannot possibly expect such a poorly trained Jack-of-all-trades to run a chemical works successfully in the face of competition directed by a large staff of scientific experts in chemistry and in engineering. It is no use spending immense sums of money on expensive machinery of the newest type in order that the works-chemist may be able to tell his future employer that the machinery used in his employer's works is completely out of date. In the course of time, moreover, unless expenditure is practically unlimited, the reverse conditions will obtain, and the technological department of the university or other institution will become more of the nature of a museum of antiquities. The great cost of the upkeep and of the working of such plant is also a very serious matter; is it possible to believe that the educational results of running, say, a large puddling furnace, such as is fitted up in Birmingham University, are in any way commensurate with their cost? The conditions in a chemical works cannot be successfully imitated in a university or polytechnic; attempts to do so can only lead to mistaken conclusions, and thus have the effect of rendering the works-chemist quite helpless when he passes from the elegant models of his educational apparatus to the workaday appliances of the manufactory.

Here, it seems to me, we touch the bed-rock of our trouble. The state of our chemical industries must be attributed to the erroneous views which have been and still are held as to the functions, and consequently as to the training, of a works-chemist. We have failed to realise that industrial chemistry must be based on a foundation of continuous and arduous research work. In the past we have sent out from our universities and other institutions students who no doubt were qualified to undertake routine analytical work, but the great majority of whom knew nothing of the methods of research. We are doing the same to-day. Just when a student has reached a stage at which his specialised scientific training should begin his course is finished, and whether he has been to a university or to a polytechnic matters little; he joins the band of those who subsist on but who do nothing to advance chemical industry. He enters a works; the manufacturer does not realise exactly what his chemist ought to do, but he expects some immediate results, and in consequence is generally disappointed; the lack of success of the chemist is put down to his ignorance of practical matters, and there is an outcry for technical education; science is most unjustly discredited, and any suggestion of spending money on research work is scouted as a mere waste.

The consequence is that if there is a scientific problem which intimately concerns all the members of some large industry what course do they adopt? Through their trade journal, and as an association representing a total capital of which I should not like to hazard a guess, they offer

a bronze or possibly a silver medal, or may even offer the extravagant sum of 20*l.*, to the happy person who will provide them with a solution. It is difficult to imagine the class of solvers to whom these princely rewards may appeal, more difficult still to believe that any useful result can be attained, and it is almost incredible that such methods should be adopted by any influential industrial organisation. This way of attempting to get research work "on the cheap" is certainly not unknown even in more enlightened countries, but that is hardly a sufficient justification for its employment.

Contrast these methods with those adopted by the Badische Anilin- und Soda-Fabrik and Meister, Lucius, and Brünig in their attempts to solve the problem of the commercial synthesis of indigo. Could there be a greater antithesis? If five thousand copies of Brunck's Paper on this subject<sup>1</sup> could be circulated among the manufacturers of this country—a task which might be fittingly undertaken by the Society of Chemical Industry—the study of the truly magnificent results attained by the systematic application of pure science, and of the indisputable evidence of their commercial value, might prove an object-lesson far more effective than argument for the accomplishment of a sorely needed reform.

Now if we are to meet successfully the very formidable scientific and commercial organisation opposed to us in chemical industry, we must perforce adopt the methods of our competitors; not only must we learn patience and perseverance, but we must also call to our aid the best brain-power available. We must recognise clearly that the scientific works-chemist, the only man who is likely to make discoveries of commercial value, must be thoroughly trained in the methods of research by those best qualified to do so, and we must not imagine that when he enters the works he should or could immediately become an engineer and a commercial expert; his place is in the research laboratory. The practical man—that is to say, the man who has a thorough and useful knowledge of some particular manufacturing process—must be trained under practical men in the works, and we must not imagine that a course of evening classes will convert him into an expert chemist. The ideal man who combines high scientific training and sound practical knowledge cannot be produced unless the period of his education is extended to half a life-time, and even then only through the cooperation of the chemistry teacher and the manufacturer.

Admitting the truth of these statements—and I do not think that they can be successfully controverted—we have now to consider what steps can be taken to provide these highly trained works-chemists, and to ensure for them a cordial reception on the part of the manufacturers.

The first fact which we have to bear in mind is that the great and rapid development of chemistry in recent times has lengthened the period which is required for the collegiate study of the subject. In order to acquire the necessary knowledge of facts and theory, and afterwards to devote even the minimum time to gaining experience in research methods, the future works-chemist must be prepared to continue at the university or other institution during at least five years. The course of study during the first three years might be on the lines now adopted by many of our universities for the B.Sc. pass examination, but to grant this degree in one or two subjects only, and then to call it an Honours degree, is in my opinion a serious mistake, as is also the admission of research work at this stage, both of which proceedings lead to far too early specialisation. The pass degree should be regarded merely as an indication of a sound general education in science, and the future works-chemist should then devote at least two years more to research and to special work in chemistry, on the results of which the Honours degree might be awarded. Every encouragement in the form of low fees, free admission, research scholarships, and so on, should be offered to such students, according to their merit and circumstances, in order that they may prolong their studies; the cost of these remissions or awards would not be very serious, and the money would be well spent. Teachers should then refuse to recommend, and manufacturers should refuse to employ, as a works-chemist, any student who had not passed through such a course satis-

<sup>1</sup> *Ber.*, 1900, i, lxxi.



factorily, unless it was understood that he was only expected to undertake routine analysis or work outside the research laboratory. By thus extending the period of training, and making research work compulsory as far as possible, a great deal would be gained; pure science would reap an immediate benefit from the investigations of the students—as has been the case abroad—and this stimulus would necessarily react on industrial chemistry; the manufacturers could be assured that they were being supplied with men of the right type; they would soon come to recognise that fact, and the demand for works-chemists would expand. In the laboratory of the works the manufacturer would then have the opportunity of gauging the capabilities and special leanings of every chemist on his staff. Those who were best fitted for directing operations in the works could be trained on the spot, as they could not possibly hope to be trained in any university or polytechnic; those who proved to be the best research chemists would, of course, remain in the laboratory working out scientific problems. Organisations of this kind could not fail to command success, and the opsonic curve of our chemical industries would soon begin to rise.

There is one institution, not a teaching body, which might greatly assist in this movement; I refer to the Institute of Chemistry of Great Britain and Ireland. This body desires and claims to represent chemistry, not only in these islands, but in all our dominions, and also to exercise some supervision or control over public appointments. It examines in chemistry and grants diplomas, and claims that its examinations are a test of practical ability rather than of theoretical knowledge. I have not a word to say against the character of these examinations, but to imagine that the Institute of Chemistry qualification is the hall-mark of a chemist is ridiculous. An average student can obtain the diploma after three, or at the very most four, years' work subsequent to matriculation, and more easily than the London B.Sc. (external). Here, again, it should be recognised that the present Institute of Chemistry qualification is only a step in the training of a chemist; the permission to present a thesis for the Associate examination should be withdrawn, and good research work should be insisted on in the case of all candidates for the Fellowship. It would then be possible to distinguish between those who are capable routine chemists and those who might be expected to advance pure and applied science. It is certainly a grave matter for an Institution entirely controlled by chemists to set such a bad example by ignoring the necessity of research work; if all our official chemical appointments and many of our posts in works are to be filled by men who have done no independent scientific work, the results will be most serious; the research habit and the research method are not easily acquired without assistance, and therefore it is all the more important to make use of this assistance while it is within reach, and before the budding chemist begins to believe that he has nothing more to learn.

As a necessary corollary to making research compulsory in the training of works-chemists, all our important teaching institutions must afford ample opportunities for such work, and measures must be adopted to guard against that failure of some of our universities as centres of research which was pointed out by Prof. Meldola.

Such failure, whatever may be the contributory causes, must be principally due to the absence of sufficient interest in research work on the part of the professor, and it certainly seems surprising, at first sight, that in these days many such professors are to be found; but it must be remembered that although by members of this Section research work is regarded as the highest and most important of all professorial duties, this is not always the view of those who make an appointment to a Chair.

In selecting a professor there are many other considerations which come into play: his ability as a teacher in the class-room and laboratory; his qualifications as a popular exponent of science; his power of organisation; his bearing towards his colleagues and his students—all these matters are of great and direct importance to a university, and it is not to be wondered at that a man highly qualified in these accessories may sometimes be chosen even though he may take no special interest in research work.

The results of such an appointment, however, cannot fail to be most prejudicial to the highest interests of the university and of the country; the chemistry department becomes a chemistry school, but not a school of chemistry.

Unfortunately, moreover, the results extend over a long period: this raises another question which certainly requires attention if we are to become more efficient.

It is far from my object to create any gratuitous insecurity of tenure in chairs of chemistry, but is it not desirable that in our teaching institutions the conditions of all appointments should include a superannuation clause? Not that a rigid age-limit should be introduced, but there should be a possibility of bringing about the retirement of those who for any reason can no longer adequately fulfil their duties. When, owing to the lapse of time, such retirement became necessary, the aged and honoured professor, pensioned by a grateful university, might still retain an intimate connection with its scientific life; as emeritus professor, with a research laboratory at his disposal, he might remain to advise and encourage his youthful successor even when the duties of teaching and the general supervision of a department had become too arduous.

It cannot be suggested that my remarks on this delicate topic are inspired by the impatience of youth or by freedom from personal consequences; the time when superannuation becomes desirable may arrive for one and all, and I have ventured to direct attention to the matter simply and solely because of its grave importance in connection with the subject of my Address. The country cannot afford to allow periods of inactivity or decadence in our seats of learning, and the interests of the individual must be subordinated to those of the nation.

Even if by adopting the above suggestions the training of our chemists is improved, and all our higher educational institutions become permanent and active centres of research, the manufacturers may still remain unresponsive, what can be done in other ways to bring about the active cooperation of pure and applied science?

The great proportion of the original work now done in this country, judging from the published records, is absolutely free from any utilitarian bias; the time, brain-power, and money devoted to this work are considerable, and the results from a scientific point of view eminently satisfactory. If even a fraction of the same skill and energy were brought to bear under proper conditions on problems of applied science, who can doubt but that the effect on our chemical industries would be one of vast importance? And yet it is the rarest possible occurrence to find any record of research work undertaken with a commercial object even in the natural home of such records, the Journal of the Society of Chemical Industry.

One reason for this may be that the discoveries made in the works-laboratories are not given to the world at large, but are quietly and lucratively applied in some secret manufacturing process. Another reason, unfortunately the more probable one, may be that nearly all the principal research workers are completely shut off from any industrial influences.

Now the worker in pure science, unaided by the advice of the manufacturer and business man, has little chance of solving any important technological problem, except as the result of accident; he has not the requisite acquaintance with commercial conditions, does not realise the enormous difference between operations on the laboratory and the manufacturing scales, or, if he does so, is unable to enter fully and with confidence into questions of fuel, labour, and so on which often determine the success or otherwise of a process. Further, much of the research work of direct commercial value concerns methods for reducing the cost of processes already in operation, and demands an intimate practical knowledge of these processes.

It is obvious, therefore, that, even if all the research capacity of the country were henceforth devoted to purely technical matters, any great improvement in our industries could hardly be anticipated without the active cooperation of the manufacturers.

Now it has been stated<sup>1</sup> that the authorities of the Manchester Municipal School of Technology intend to under-

<sup>1</sup> Levinstein, *Journ. Soc. Chem. Ind.*, 1903, 845.



take investigations for local manufacturers and merchants in connection with difficulties which may be met with in their works or business. This method of securing the interest and support of those engaged in applied chemistry may or may not be workable according to the conditions under which such cooperation is carried out. The staff and the laboratories of a university or polytechnic cannot be placed at the unrestrained and gratuitous disposal of any manufacturer who is in some trivial difficulty, nor can the choice of the subjects to be investigated be decided by the governing body. If however the arrangements, pecuniary and otherwise, are left entirely in the hands of those directly concerned, namely, the manufacturer and the responsible head of the chemistry department, the scheme should then prove exceedingly valuable, and should be adopted as widely as possible. It should be understood, and the fact might even be advertised by the governing body, that for purposes of research work in applied chemistry—but not of course for analytical work—the laboratory of the university, college, or polytechnic is, under certain conditions, at the service of the manufacturers; that although primarily and unswervingly devoted to work in pure science, such institutions recognise that, for their own interests, they must do all they can to assist chemical industries.

It might be thought that these conditions prevail at the present time, and that any manufacturer, if he so choose, may consult the university staff on any problem in which he is interested. Possibly this is true to a limited extent, but in most institutions the members of the staff are restrained from undertaking any outside work; in others, such work may only be done with the sanction of the authorities.

These conditions, of course, are only laid down because the governing body believes that they safeguard the interests of the institution, and if it were shown that their enforcement is really contrary to those interests they would soon be abrogated. Many or all such authorities readily permit the members of their staff to undertake outside examination work because they consider this course to be to the advantage of their institution; but how incomparably more important is the object of gaining the confidence and support of the manufacturers.

Pray do not let it be imagined that this is some subtle scheme for increasing the pecuniary rewards of the teachers. I greatly fear that to many of those who are now engaged in research work the suggestion that they should give some attention to applied chemistry would be very distasteful, simply because it would involve an immediate encroachment on the time, already far too limited, which they are able to give to the immediate scientific problem which is one of their principal joys in life. To those who might have fears of this kind I would point out that there would soon be some compensation; once the cooperation of the manufacturers is secured, the demand for research chemists would expand, and the laboratories would be filled with students whose help in pure science would be invaluable.

The possible objection that the teaching staff would devote too much time to applied work and neglect other duties is one which could be left for the governing body to deal with unsparingly. If the institution took some percentage of all extraneous remuneration, or any similar arrangement were made, the funds thus provided could be used for increasing the staff of assistants and demonstrators—a most desirable reform in itself.

One of the greatest advantages of a working arrangement such as that here indicated would be that, like the method already suggested, it would lead to the evolution of what is otherwise almost unattainable—namely, men thoroughly trained in both science and practice. The research students of the teaching institution, engaged on a given problem for a manufacturer, would of course be allowed to study its practical aspects in the works; on the other hand, works-chemists, with considerable practical experience, would be granted permission to proceed to the university laboratory, where they would study the problem with the assistance of the highest scientific knowledge, and acquire further training in the methods of research.

Combinations such as these could hardly fail to lead to valuable results, which would form the subject of

patents; the monopolies thus acquired would place the manufacturers in a favourable position, and the revival of our chemical industries would follow in due course. There is nothing Utopian in this scheme, and there are no great initial difficulties to be overcome; it may be set in operation by the manufacturer, and possibly also, as will be indicated later, by the worker in pure science. Reading between the lines certain records which have recently appeared in the science journals and the patent lists, it may even be inferred that such arrangements are already in force in one of our large industrial centres.

There are other ways in which it might be possible to obtain the active cooperation of the manufacturers. Any individual or firm interested in a problem of applied science might be invited to found a temporary research scholarship at the university or other institution for the definite object of the particular problem in question. The maximum period during which such a scholarship would be tenable might be fixed beforehand, so that the financial liability of the founder would be limited and proportionate to the importance of the object in view. The holder of the scholarship might be nominated by the university, or by the founder and the university jointly, and suitable conditions would be drawn up to ensure the interests of the founder; he would of course have the benefit of all the results of the work, and would secure the patent rights of any new invention, subject possibly to the payment of a small percentage of the profits to the university and to the holder of the scholarship. During the tenure of the scholarship, the holder, and also the founder, would have the advantage of the scientific knowledge of the university; the scholarship holder would also be allowed to gain practical experience in the works, and, if successful, there is little doubt but that he would have the option of working the process on the large scale and of obtaining permanent employment under satisfactory conditions. After a given period the scientific results of the work would be published through the usual channels in the ordinary way.

This idea of applied research scholarships had taken shape in my mind when I happened to come across a book recently published in the United States, called "The Chemistry of Commerce," in which I found that a similar proposal had been made by the author, R. K. Duncan, Professor of Industrial Chemistry at the University of Kansas. The scheme is there worked out in some detail, and a form of legal agreement to be signed by the university authorities and by the founder of the "Industrial Fellowship," is suggested.

Thinking it would be of interest to know how the plan had worked out in practice, I wrote to Prof. Duncan and received a reply a few weeks ago. He very courteously informed me that five industrial fellowships had already been established in his laboratories, that the agreements for two additional ones were being prepared, and that he might have obtained more, but wished to proceed conservatively; also that he had no reason to doubt the entire practicability of the scheme, and that experience had shown that the terms of the agreement could be made more favourable to the university than those which were first drawn up. One of the new conditions is that the industrial fellowship holder shall give two hours a week gratuitous instruction in the work of the chemistry department—an arrangement which has proved to be of great inspirational value. The fellowships are tenable during two years, and are of the value of 500 dollars or 1000 dollars per annum.

It is too soon to be able to form any opinion as to the commercial importance of the work carried out under this scheme, but it is obvious that the foundation of such scholarships for the study of general or special problems in applied chemistry is most desirable. One of their great advantages would be that they might be founded by those manufacturers who cannot afford permanently to engage a research chemist. Large and successful firms like the United Alkali Co., Brunner, Mond and Co., and many others which can employ a staff of chemists, are of course eminently capable of managing their own affairs without outside assistance or advice, and it is only for those which are less prosperous that the foregoing suggestions are made.

The great benefits which are conferred on pure science



by the open research scholarships at present available afford some indication of what might be done for industrial chemistry by the foundation of such scholarships in applied science. There are, no doubt, scattered over the country many men who possess originality and inventive talent, and who have practical experience in industrial operations, but who have not been sufficiently trained in science; if it were possible to attract this dormant talent by means of open scholarships it might be directed into proper channels instead of being allowed to run to waste.

It is easy to say how money might be spent advantageously, but very difficult to suggest how the funds for such open scholarships should be raised. An appeal to the manufacturers by this Association or by the Society of Chemical Industry might meet with some response, and it is also possible that public bodies might render assistance. If the Government of Bengal, under the spur of dire necessity, can subsidise research work on indigo, and if our county councils can offer scholarships for dairy work, and grants for experiments on turnip-growing, bee-keeping, and so on, our city and borough councils might award scholarships in applied chemistry for subjects of especial importance to the dominant trades of the district. By so doing they would be utilising to the best advantage the chemistry departments of our universities and polytechnics.

I noted a few moments ago that practically all the published research work of this country has no direct reference to any industrial problem; nevertheless the results of this work are often of such a character that they might be of considerable technological importance. New reactions are discovered; new or improved methods of preparing known compounds; new facts as to the conditions under which important general reactions occur; and, needless to add, a great many new compounds are prepared.

Now, abroad, all or nearly all such matters are protected by patents, generally taken out by some firm of manufacturers. To the uninitiated it seems absurd to think that there is money in the great majority of such patents, and yet it is obvious that the employment of this system must pay in the long run. Why should it not be adopted in this country—at any rate to a limited extent to start with?

If all those who are engaged in purely scientific research work would seriously consider the desirability of obtaining provisional protection for any discovery which they may make, and would then consult some manufacturer or industrial expert with whom the further development of the matter might be undertaken, there is reason to believe that in some cases at least the patent might prove to be a commercial success.

The examination of the therapeutic action of compounds discovered in our laboratories is also a possible means of assisting our chemical industries; the matter is not so trivial as it may seem; a monopoly in the manufacture of some valuable medicinal preparation would serve as a *point d'appui* from which more important operations could be undertaken.

Unfortunately the investigation of the physiological action of new preparations is a matter of some difficulty in this country, as it is to some extent connected with vivisection in the public mind; we may poison rats with impunity, and even create an organisation for their extermination, but we may not individually try the effect of a new compound on a rabbit.

In drawing this Address to a conclusion I cannot but feel that my suggestions may seem utterly inadequate to the attainment of those important results which are so greatly to be desired. If so, I can only plead that more drastic measures are hardly available, and that even in the most favourable circumstances improvement can take place only very slowly. Whatever differences of opinion may be held as to the details of any scheme for regaining our lost ground, the main lines seem to be clearly indicated. The workers in pure science must recognise that it is their duty to do all they can to promote the industrial welfare of their country; the manufacturers must concede the paramount importance of science and the impossibility of dispensing with its counsels. Guided by these principles and by a spirit of cordial cooperation, a sustained and strenuous effort on the part of the leaders of chemical industry and

of chemical science can hardly fail to accomplish the end in view.

In elaborating this Address I have enjoyed the advantage of the criticisms and suggestions of my friend and relative Prof. Perkin, F.R.S., to whom my sincere thanks are here expressed.

## SECTION C.

### GEOLOGY.

OPENING ADDRESS BY PROF. JOHN JOLY, M.A., D.Sc., F.R.S., PRESIDENT OF THE SECTION.

#### URANIUM AND GEOLOGY.

##### *Introduction.*

In our day but little time elapses between the discovery and its application. Our starting-point is as recent as the year 1903, when Paul Curie and Laborde showed experimentally that radium steadily maintains its temperature above its surroundings. As in the case of many other momentous discoveries, prediction and even calculation had preceded it. Rutherford and McClung, two years before the date of the experiment, had calculated the heat equivalent of the ionisation effected by uranium, radium, and thorium. Even at this date (1903) there was much to go upon, and ideas as to the cosmic influence of radio-activity were not slow in spreading.<sup>1</sup>

I am sure that but few among those whom I am addressing have seen a thermometer rising under the influence of a few centigrams of a radium salt; but for those who pay due respect to the principles of thermodynamics, the mere fact that at any moment the gold leaves of the electroscopes may be set in motion by a trace of radium, or, better still, the perpetual motion of Strutt's "radium clock," is all that is required as demonstration of the ceaseless outflow of energy attending the events proceeding within the atomic systems.

Although the term "ceaseless" is justified in comparison with our own span of existence, the radium clock will in point of fact run down, and the heat outflow gradually diminish. Next year there will be less energy forthcoming to drive the clock, and less heat given off by the radium by about the one three-thousandth part of what now are evolved. As geologists accustomed to deal with millions of years, we must conclude that these actions, so far from being ceaseless, are ephemeral indeed, and that if importance is to be ascribed to radium as a geological agent, we must seek to find if the radium now perishing off the earth is not made good by some more enduringly active substance.

That uranium is the primary source of supply cannot be regarded as a matter of inference only. The recent discovery of ionium by Boltwood serves to link uranium and radium, and explains why it was that those who sought for radium as the immediate offspring of uranium found the latter apparently unproductive, the actual relation of uranium to radium being that of grandparent. But even were we without this connected knowledge, the fact of the invariable occurrence in Nature of these elements, not only in association but in a quantitative relationship, can only be explained on a genetic connection between the two. This evidence, mainly due to the work of Boltwood, when examined in detail, becomes overwhelmingly convincing.

Thus it is to uranium that we look for the continuance of the supplies of radium. In it we find an all but eternal source. The fraction of this substance which decays each year, or, rather, is transformed to a lower atomic weight, is measured in tens of thousands of millionths; so that the uranium of the earth one hundred million years ago was hardly more than 1 per cent. greater in mass than it is to-day.

As radio-active investigations became more refined and extended, it was discovered that radium was widely diffused over the earth. The emanation of it was obtained from the atmosphere, from the soil, from caves. It was

<sup>1</sup> See letters appearing in NATURE of July 9 and September 24, 1903, from the late Mr. W. E. Wilson and Sir George Darwin referring to radium as a solar constituent, and one from the writer (October 1, 1903) on its influence as a terrestrial constituent.



extracted from well waters. Radium was found in brick-earths, and everywhere in rocks containing the least trace of demonstrable uranium, and Rutherford calculated that a quantity of radium so minute as  $4.6 \times 10^{-14}$  grams per gram of the earth's mass would compensate for all the heat now passing out through its surface as determined by the average temperature gradients. In 1906 the Hon. R. J. Strutt, to whom geology owes so much, not only here but in other lines of advance, was able to announce, from a systematic examination of rocks and minerals from various parts of the world, that the average quantity of radium per gram was many times in excess of what Rutherford estimated as adequate to account for terrestrial heat-loss. The only inference possible was that the surface radium was not an indication of what was distributed throughout the mass of the earth, and, as you all know, Strutt suggested a world deriving its internal temperature from a radium jacket some 45 miles in thickness, the interior being free from radium.<sup>1</sup>

My own experimental work, begun in 1904, was laid aside until after Mr. Strutt's paper had appeared, and a valued correspondence with its distinguished author was permitted to me. This address will be concerned with the application of my results to questions of geological dynamics.

Did time permit I would, indeed, like to dwell for a little on the practical aspect of measurements as yet so little used or understood; for the difficulties to be overcome are considerable, and the precautions to be taken many. The quantities dealt with are astoundingly minute, and to extract with completeness a total of a few billionths of a cubic millimetre of the radio-active gas—the emanation—from perhaps half a litre or more of a solution rich in dissolved substances cannot be regarded as an operation exempt from possibility of error; and errors of deficiency are accordingly frequently met with.

Special difficulties, too, arise when dealing with certain classes of rocks. For in some rocks the radium is not uniformly diffused, but is concentrated in radio-active substances. We are in these cases assailed with all the troubles which beset the assayer of gold who is at a loss to determine the average yield of a rock wherein the ore is sporadically distributed. In the case of radium determinations this difficulty may be so much the more intensified as the isolated quantities involved are the more minute and yet the more potent to affect the result of any one experiment. There is here a source of discrepancy in successive experiments upon those rocks in which, from metamorphic or other actions, a segregation of the uranium has taken place. With such rocks the divergences between successive results are often considerable, and only by multiplying the number of experiments can we hope to obtain fair indications of the average radio-activity. It is noteworthy that these variations do not, so far as my observations extend, present themselves when we deal with a recent marine sediment or with certain unaltered deposits wherein there has been no readjustment of the original fine state of subdivision, and even distribution, which attended the precipitation of the uranium in the process of sedimentation.

But the difficulties attending the estimation of radium in rocks and other materials leave still a large balance of certainty—so far as the word is allowable when applied to the ever-widening views of science—upon which to base our deductions. The emanation of radium is most characteristic in behaviour; knowledge of its peculiarities enables us to distinguish its presence in the electroscope not only from the emanation of other radio-active elements, but from any accidental leakage or inductive disturbance of the instrument. The method of measurement is purely comparative. The cardinal facts upon the strength of which we associate radium with geological dynamics, its development of heat and its association with uranium, are founded in the first case directly on observation, and, in the second, on evidence so strong as to be equally convincing. Recent work on the question of the influence of conditions of extreme pressures and temperatures on the radio-active properties of radium appear to show that, as would be anticipated, the effect is small, if indeed existent. As observed by Makower and Rutherford, the small diminution noticed under very

<sup>1</sup> Proc. R.S., lxxvii, p. 472, and lxxvii. p. 150.

extreme conditions in the  $\gamma$  radiation possibly admits of explanation on indirect effects. These observations appear to leave us a free hand as regards radio-thermal effects unless when we pursue speculations into the remoter depths of the earth, and even there while they remain as a reservation, they by no means forbid us to go on.

The precise quantity of heat to which radium gives rise, or, rather, which its presence entails, cannot be said to be known to within a small percentage, for the thermal equivalent of the radio-active energy of uranium, actinium, and ionium, and of those members of the radium family which are slow in changing, has not been measured directly. Prof. Rutherford has supplied me, however, with the calculated amount of the aggregate heat energy liberated per second by all these bodies. In the applications to which I shall presently have to refer I take his estimate of  $5.6 \times 10^{-2}$  calories per second as the constant of heat-production attending the presence of one gram of elemental radium.

To these words of introduction I have to add the remark, perhaps obvious, that the full and ultimate analysis of the many geological questions arising out of the presence of radium in the earth's surface materials will require to be founded upon a broader basis than is afforded by even a few hundred experiments. The whole sequence of sediments has to be systematically examined; the various classes of igneous materials, more especially the successive ejecta of volcanoes, fully investigated. The conditions of entry of uranium into the oceanic deposits have to be studied, and observations on sea-water and deep-sea sediments multiplied. All this work is for the future; as yet but little has been accomplished.

#### *The Radium in the Rocks and in the Ocean.*

The fact first established by Strutt that the radium distributed through the rock materials of the earth's surface greatly exceeds any permissible estimate of its internal radio-activity has not as yet received any explanation. It might indeed be truly said that the concentration of the heaviest element known to us (uranium) at the surface of the earth is just what we should not have expected. Yet a simple enough explanation may be at hand in the heat-producing capacity of that substance. If it was originally scattered through the earth-stuff, not in a uniform distribution, but to some extent concentrated fortuitously in a manner depending on the origin of terrestrial ingredients, then these radio-active nuclei heating and expanding beyond the capacity of surrounding materials would rise to the surface of a world in which convective actions were still possible and, very conceivably, even after such conditions had ceased to be general; and in this way the surface materials would become richer than the interior. For instance, the extruded mass of the Deccan basalt would fill a sphere 36 miles in radius. Imagine such a sphere located originally somewhere deep beneath the surface of the earth surrounded by materials of like density. The ultimate excess of temperature, due to its uranium, attained at the central parts would amount to about  $1000^{\circ}$  C., or such lesser temperature as convective effects within the mass would permit. This might take some thirty million years to come about, but before so great an excess of temperature was reached the force of buoyancy developed in virtue of its thermal expansion must inevitably bring the entire mass to the surface. This reasoning would, at any rate, apply to material situated at a considerable distance inwards, and may possibly be connected with vulcanicity and other crustal disturbances observed at the surface.<sup>1</sup> The other view, that the addition of uranium to the earth was mainly an event subsequent to its formation in bulk, so that radio-active substances were added from without and, possibly, from a solar or cosmic source, has not the same *a priori* probability in its favour.<sup>2</sup>

I have in this part of my address briefly to place before you an account of my experiments on the amounts of radium distributed in surface materials. Here, indeed, direct knowledge is attainable; but this knowledge takes us but a very few miles inwards towards the centre of the earth.

<sup>1</sup> See Appendix A.

<sup>2</sup> NATURE, lxxv., p. 294.



*The Igneous Rocks.*—The basalt of the Deccan, to which I have referred, known to cover some 200,000 square miles to a depth of from 4000 to 6000 feet or more, appears to be radio-active throughout. A fine series of tunnel and surface specimens sent to me by the Director of the Indian Geological Survey has enabled me to examine the radio-activity at various points. It is remarkable that the mean result does not depart much from that afforded by a long series of experiments on North of Ireland basalt and on the basalt of Greenland.

Again, the granites and syenites—and those of Mourne, Aberdeen, Leinster, Plauen, Finsteraarhorn have been examined—while variable, yet approximate to the same mean result.

In the Simplon and St. Gothard tunnels igneous rocks have been penetrated at considerable depth beneath the surface. The greatest true depth is attained, I think, in the central St. Gothard massif. It is remarkable, and may be significant, that in these rocks I have reached the lowest radio-activities I have met—down to almost one-billionth of a gram of radium per gram; although the general mean of the St. Gothard igneous rocks, owing to the high radio-activity of the Finsteraar granite at the north end of the tunnel, is not exceptionally low. Radio-active minerals seem common in the Simplon rocks, involving considerable variations in successive experiments. Some of the highest results are omitted in the mean given below, but as it is difficult to know what to allow for purely sporadic radium the mean is not very certain. In the case of a specially high result I asked Prof. Emil Werner to determine the uranium: my result was confirmed. My list of mean results on igneous rocks up to the present is the following:—

Basalts (14) ... ..	5·0 <sup>1</sup>	Lewisian Gneiss (3) ...	5·7
Granites (6) ... ..	4·1	Simplon (32) ... ..	7·6
Syenites (1) ... ..	6·8	St. Gothard (32) ...	5·1

The general mean is 6·1.

From the igneous rocks have originated the sediments after a toll of dissolved substances has been paid to the ocean. It does not of course follow necessarily that the percentage of radium, or more correctly of uranium, in the sedimentary rocks should be less than in the igneous. The residual materials might keep the original percentage of the parent rock, or even improve upon it. There are reasons for believing, however, that there would be a diminution.

Those sedimentary rocks which have been derived from materials formerly in solution offer a different problem. In their case there is little or none of the original materials carried into the secondary rock, and the radio-activity will depend mainly upon how far uranium is precipitated or abstracted with the rock-making substances. In other words, upon how far the waters of the ocean will restore to the rocks what it has borrowed from them.

This brings me to consider the condition of the ocean as preparatory to quoting experiments on the sediments.

*The Ocean and its Sediments.*—The waters of the ocean, covering five-sevenths of the earth's surface to a mean depth of 3·8 kilometres, represent the most abundant surface material open to our investigation. As the mean of a very large number of experiments upon twenty-two different samples of sea-water from various widely separated parts of the ocean, I obtain a mean of  $0·016 \times 10^{-12}$  gram per cubic centimetre. There is considerable variability. Taking the mass of the ocean as  $1·458 \times 10^{18}$  tonnes, there must be about  $20 \times 10^9$  grams (20,000 tons) of radium in its waters.

The experiments which I have been able to make on deep-sea deposits, thanks mainly to the kind cooperation of Sir John Murray, apply to ten different materials of typical character.

The results are so consistent as to lead me to believe that although so few in number they cannot be far wrong in their general teaching.

<sup>1</sup> This number is to be multiplied by  $10^{-12}$ , and represents billionths of a gram of radium per gram of material investigated. Throughout the rest of my address this understanding holds, unless where a different meaning is specified. The numbers in parentheses signify the number of different specimens investigated.

The means are:—

	Radium	Extension: Millions of square miles
Globigerina Ooze ... ..	7·2 ... ..	49·5
Radiolarian " ... ..	36·7 ... ..	2·5
Red Clay ... ..	33·3 ... ..	51·5

Diatom Oozes have not yet been examined.

It is apparent from these results that the more slowly collecting sediments are those of highest radio-activity, as if the organic materials raining downwards from the surface of the ocean carried everywhere to the depths uranium and radium abstracted from the waters, but in those regions where the conditions were inimical to the preservation of the associated calcareous tests there was the less dilution of the radio-active substances accumulating beneath. The next table shows that radio-activity and the percentage of calcareous matter in these deposits stand in an inverse relation:—

	Calcium carbonate per cent.	Radium
Globigerina Ooze, <i>Challenger</i> 338 ...	92·24 ...	6·7
" " " " 290 ...	64·34 ...	7·4
Red Clay ... ..	5 ...	15·4
" " " " 276 ...	28·28 ...	52·6
Radiolarian Ooze " 272 ...	10·19 ...	22·8
" " " " 274 ...	3·89 ...	50·3

The percentages of calcium carbonate are from the Report of the *Challenger* Expedition. The Red Clay in the table, which reads as an apparent exception, is probably a case of recent change in the character of the deposit, for the evidence of manganese nodules and sharks' teeth brought up with this clay is conclusive as to the slow rate of its collection. Readers of Sir John Murray's and Prof. Renard's report will remember many cases where recent change in the character of a deposit is to be inferred.

A point of much importance in connection with our views on oceanic radio-activity is that of the presence in the waters and in the deposits of the parent radio-active substance, uranium. The evidence that the full equivalent amount of uranium is present is, I believe, conclusive.

In the first place, to so vast a reservoir as the ocean the rivers cannot be supposed to supply the radium sufficiently fast to make good the decay. In a very few thousand years, in the absence of uranium, the rivers must necessarily renew almost the entire amount of radium present. I have made examination of the water of one great river only—the Nile. The quantity of radium detected was  $0·0042 \times 10^{-12}$  per cubic centimetre. That is less than the oceanic amount. In short, it is evident that the uranium must accumulate year by year in the oceanic reservoir, like other substances brought in by the rivers, and that the present state of the waters is the result of such actions prolonged over geological time.

While this reasoning is conclusive as regards the waters of the ocean, it does not assure us that the sediments accumulating in their depths are throughout as radio-active as their surface parts would indicate. There might be a precipitation of radium unattended by uranium, in which case their deeper parts would not be radio-active.

Against this possibility there is the evidence of such true deep-sea deposits as were formed in past times and to-day still preserve their radio-activity. For instance, the chalk, which, considering that it was undoubtedly a very rapidly formed deposit, exhibits a radio-activity quite comparable with that of the Globigerina Oozes, deposits which it most nearly resembles. In this deposit, clearly, the uranium must have collected along with the calcareous materials. We can with security argue that the similar oozes collected to-day must likewise contain uranium. In the case of the Red Clays we have the direct determination of the uranium which Prof. Emil Werner was so good as to make at my request. Considering the difficulties attending its separation, the result must be taken as supporting the view that here, too, the radium is renewed from the uranium. Regarding the efforts of other observers to detect uranium in such deposits, it is noteworthy that without the guidance of the radium, enabling specially rich materials to be selected for analysis, the



success of the investigation must have been doubtful. The material used was a Red Clay with the relatively large quantity of 54.4 billionths of a gram per gram. In a few grams of this Werner obtained up to seven-twelfths of the total theoretic amount, and of course the separation of the uranium is not likely to have been complete.

It might be thought a hopeless task to offer any estimate of the total bulk of the sub-oceanic deposits, and from this to arrive at some idea of the quantity of radium therein contained. Nevertheless, such an estimate is not only possible, but is based on deductions which possess considerable security. As a major limit I believe the estimate of the total mass of deposit is unassailable, and such deductions as might be applied will still leave it an approximation to the truth.

The elements of the problem are simple enough; we know that the sedimentary rocks have been derived from the igneous, some 30 per cent. of the latter entering into solution in the process of conversion. Some of the soluble constituents, owing to their great solubility, have remained in solution since they entered the ocean.<sup>1</sup> These are the salts of sodium. An estimate of the amount of these salts in the ocean gives us a clue to the total amount of rock substance which has contributed to oceanic salts and oceanic deposits since the inception of the oceans. Some years ago I deduced on this basis that the igneous rocks which are parent to the sodium in the sea must have amounted to about  $91 \times 10^{16}$  tons.<sup>2</sup> This figure in no way involves the rate of supply by the rivers, or our estimate of geological time. It only involves the quantity of sodium now in the ocean—a fairly well-known factor—and the loss of this element, which occurs when average igneous rocks are degraded into sedimentary rocks—a factor also fairly well known. Mr. F. W. Clark, to whom geological science is indebted for so much exact investigation, has recently repeated this calculation, using data deduced anew by himself, and arrives at the result that the bulk of the parent igneous rock was  $84.3 \times 10^6$  cubic miles.<sup>3</sup> On a specific gravity of 2.6 my estimate in tons gives nearly the same result:  $84 \times 10^6$  cubic miles.

Now about one-third part of this parent rock goes into solution when breaking up into a detrital sediment. The limestones upon the land are part of what was once so brought into solution. Having made deduction of these former marine deposits (and I here avail myself of Van Hise's and Clark's estimates of the total amount of the sedimentaries and the fraction of these which are calcareous),<sup>4</sup> and, allowing for the quantity remaining in solution in the ocean, the result leaves us with the approximation of twenty million cubic miles of matter once in solution, and now for the greater part existing as precipitated or abstracted deposits at the bottom of the ocean. We are to distribute this quantity over its floor. If the rate of collection had been uniform in every part of the ocean throughout geological time, a depth of about one-seventh of a mile (240 metres) of deposit would cover the ocean bed.

While, I believe, we can place considerable reliance on this approximation, we are less sure when we attempt an estimate of its mean radio-activity. If we assume for it an average radio-activity similar to that of Globigerina Ooze, we find that the quantity of radium involved must be considerably more than a million tons. Apart from the value which such estimates possess as presenting us with a perspective view of the great phenomena we are dealing with, it will now be seen that it supports the finding of the experiments on sedimentary rocks, and leads us to anticipate a real difference in the radio-activity of the two classes of material.

*The Sedimentary Rocks.*—The radium content of those of detrital character is indicated in the following sandstones, slates, and shales:—

Shales, sandstones, grits (10) ... ..	4.4
Slates (Cambrian, Devonian) ... ..	4.7
Mud from Amazon ... ..	3.2

Some of the above are from deep borings in Carbon-

<sup>1</sup> Trans. Royal Dublin Soc., vol. vii., ser. ii., p. 23 *et seq.*

<sup>2</sup> *Ibid.*, p. 46.

<sup>3</sup> "The Data of Geochemistry," by F. W. Clark, p. 29.

<sup>4</sup> *Ibid.*, p. 31.

iferous rocks (the Balfour and Burnlip bores),<sup>1</sup> and from their nature, where not actually of fresh-water origin, can owe little to oceanic radio-activity. Many of the following belong to the class of precipitates, and therefore owe their uranium wholly or in part to oceanic source:—

Marsupites chalk ... ..	4.2
Green sandstone ... ..	4.9
Green sand (dredged) ... ..	4.5
Limestones and dolomites [Trenton, Carboniferous, Zechstein, Lias, Solenhofen (7)] ... ..	4.1
Keuper gypsum ... ..	6.9
Coral rock, Funafuti bore (4) <sup>2</sup> ... ..	1.7
Trias-Jura sediments, Simplon: 17 rocks of various characters ... ..	6.9
Mesozoic sediments, St. Gothard: 19 rocks of various characters ... ..	4.2

The general mean on sixty-two rocks is 4.7.

Making some allowance for uncertainties in dealing with the Simplon rocks, I think the experiments may be taken as pointing to the result:—

Igneous rocks from 5 to 6.

Sedimentary rocks from 4 to 5.

If our estimate of oceanic radium be applied to the account of the sedimentary rocks in a manner which will be understood from what I have already endeavoured to convey, there will be found to exist a fair degree of harmony between the great quantities which we have found to be in the sediments of the ocean and the impoverishment of the sediments which the experiments appear to indicate.

In all these results fresh and unweathered material has been used. The sand of the Arabian desert gave me but 0.4. Similarly low results have been found by others for soils and such materials. These are not to be included when we seek the radio-activity of the rocks.

As regards generally my experiments on the radium-content of the rocks, I cannot say with confidence that there is anything to indicate a definite falling off in radio-activity in the more deeply seated materials I have dealt with. The central St. Gothard and certain parts of the Deccan have given results in favour of such a decrease. On the other hand, as will be seen later, the granite at the north end of the St. Gothard and the primitive gneiss of the Simplon show no diminution. According to the view I have put forward above as to the origin of the surface richness in radium it is, I think, to be expected that, while the richest materials would probably rise most nearly to the surface, there might be considerable variability in the radio-activity of the deeper parts of the upper crust.

*Uranium and the Internal Heat of the Earth.*

While forced to deny of the earth's interior any such richness in radium as prevails near the surface, the inference that uranium exists yet in small quantities far down in the materials of the globe is highly probable. This view is supported by the presence of radium in meteoric substances and by its very probable presence in the sun—that greatest of meteorites. True, the radio-thermal theory cannot be supposed to account for any great part of solar heat unless we are prepared to believe that a very large percentage of uranium can be present in the sun, and yet yield but feeble spectroscopic evidence of its existence. Taken all together, the case stands thus as regards the earth. We are assured of radium as a widely distributed surface material, and to such depths as we can penetrate. By inference from the presence of radium in meteoric substances and its very probable presence in the sun, from which the whole of terrestrial stuff probably originated, as well as by the inherent likelihood that every element at the surface is in some measure distributed throughout the entire mass, we arrive at the conclusion that radium is indeed a universal terrestrial constituent.

The dependent question then confronts us—Are we living on a world heated throughout by radio-thermal actions? This question—one of the most interesting which has

<sup>1</sup> For these rocks, and for much other valuable material, I have to thank Mr. D. Tate, of the Scottish Geological Survey.

<sup>2</sup> For these I have to thank the Trustees of the British Museum and Mr. A. S. Woodward, F.R.S.



originated in the discovery that internal atomic changes may prove a source of heat—can only be answered (if it can be answered at all) by the facts of geological science.

I will not stop to discuss the evidence for and against a highly heated interior of the earth. I assume this heated interior as the obvious and natural interpretation of a large class of geological phenomena, and pass on to consider certain limitations to our knowledge which have to be recognised before we are in a position to enter on the somewhat treacherous ground of hypotheses.

In the first place, we appear debarred from assuming that the surface and central interior of the earth are in thermal connection, for it seems certain that, since the remote period when (probable) convective effects became arrested by reason of increasing viscosity, the thermal relations of the surface and interior have become dependent solely on conductivity. From this it follows if the state of matter in the interior is such as Lord Kelvin assumed—that is, that the conductivity and specific heat may be inferred from the qualities of the surface materials—we have remained in thermal isolation from the great bulk of the interior for hundreds of millions of years, and perhaps even for more than a thousand millions of years. Assuming a diffusivity similar to that of surface rocks, and starting with a temperature of  $7000^{\circ}$  F., Kelvin found that after 1000 million years of cooling there would be no sensible change at a depth from the surface greater than 568 miles. In short, even if this great period—far beyond our estimates of geological time—has elapsed since the *consistenter status*, the cooling surface has as yet borrowed heat from only half the bulk of the earth.

It is possible, on the other hand, that the conductivity increases inwards, as Prof. Perry has contended; and if the central parts are more largely metallic, this increase may be considerable. But we find ourselves here in the regions of the unknown.

With this limitation to our knowledge, the province of geothermal speculation is a somewhat disheartening one. Thus if with Rutherford, who first gave us a quantitative estimate of the kind, we say that such and such a quantity of radium per gram of the earth's mass would serve to account for the  $2.6 \times 10^{20}$  calories which, according to the surface gradients, the earth is losing per annum, we cannot be taken as advancing a theory of radio-active heating, but only a significant quantitative estimate. For, in fact, the heat emitted by radium in the interior may never have reached the surface since the convective conditions came to an end.

And here, depending upon the physical limitations to our knowledge of the earth's interior, a possibility has to be faced. That uranium is entirely absent from the interior is, as I have said, in the highest degree unlikely. If it is present, then the central parts of the earth are rising in temperature. This view, that the central interior is rising in temperature, is difficult to dispose of, although we can adduce the evidence of certain surface-phenomena to show that the rise in temperature during geological time must be small or its effects in some manner kept under control. In a word, whether we assume that the whole heat-loss of the earth is now being made good by radio-active heating or not, we find, on any probable value of the conductivity, a central core almost protected from loss by the immense mass of heated material interposed between it and the surface, and within this core very probably a continuous source of heat. It is hard to set aside any of the premisses of this argument.<sup>1</sup>

We naturally ask, Whither does the conclusion lead us? We can take comfort in a possible innocuous outcome. The uranium itself, however slowly its energy is given up, is not everlasting. The decay of the parent substance is continually reducing the amount of heat which each year may be added to the earth's central materials. And the result may be that the accumulated heat will ultimately pass out at the surface by conductivity, during remote future times, and no physical disturbance result.

The second limitation to our hypotheses arises from this transformation and gradual disappearance of the uranium. And this limitation seems as destructive of definite geo-

<sup>1</sup> Prof. H. A. Wilson has made a suggestive estimate of the thermal effects of radium enclosed in the central parts of the earth (*NATURE*, February 20, 1908).

thermal theories as the first. To understand its significance requires a little consideration. The fraction of uranium decaying each year is vanishingly small, about the ten thousand-millionth part; but if the temperature of the earth is maintained by uranium, and consequently its decay involves the fall in temperature of the whole earth, the quantity of heat escaping at the surface attendant on the minute decrement would be enormous. An analogy may help to make this clear. Consider the familiar case of a boiler maintained at a particular temperature by a furnace within. Let the combustion diminish and the furnace temperature fall a little. The whole mass of the boiler and its contents follow the downward movement of temperature, heat of capacity escaping at the surface. An observer, only noting the outflow of radiated heat and unable to observe the minute drop of temperature, would probably ascribe to the continued action of the furnace heat which, although derived from it in the past, should no longer be regarded as indicating the heating value of the combustion. Magnify the boiler to terrestrial dimensions: the minutest fall in temperature of the entire mass involves immense quantities of heat passing out at the surface, which no longer indicate the sustaining radio-thermal actions within.

It is easy to see the nature of the difficulties in which we thus become involved. In fact, the heat escaping from the earth is not a measure of the radium in the earth, but necessarily includes, and for a great part may possibly be referred to, the falling temperature, which the decay of the uranium involves. If we take  $\lambda$  (the fraction of uranium transforming each year) as approximately  $10^{-10}$  and assume for the general mass of the earth a temperature of  $1500^{\circ}$ , a specific heat of 0.2, and, taking  $6 \times 10^{27}$  as its mass in grams, we have, on multiplying these values together, a loss in calories per annum of  $1.8 \times 10^{20}$ . This by hypothesis escapes at the surface. But the surface loss, as based on earth-gradients of temperature, is but  $2.6 \times 10^{20}$  calories. We are left with  $0.8 \times 10^{20}$  calories as a measure of the radium present. On this allowance our theories, in whatever form, must be shaped. Nor does it appear as if relief from this restriction can be obtained in any other way than by denying to the interior parts of the earth the requisite high thermal conductivity. Taking refuge in this, we are, however, at once confronted with the possibility of internal stores of radium of which we know nothing, save that they cannot, probably, be very great in amount. In short, I believe it will be admitted on full examination of this question that, while we very probably are isolated thermally from a considerable part of the earth's interior, the decay of the uranium must introduce a large subtractive correction upon our estimates of the limiting amounts of radium which might be present in the earth.

But, finally, is there in all these difficulties sufficient to lead us to reject the view that the present loss of earth-heat may be nearly or quite supplied by radium, and the future cooling of the earth controlled mainly by decay of the uranium? I do not think there are any good grounds for rejecting this view. Observe, it is the condition towards which every planetary body and every solar body containing stores of uranium must tend; and apparently must attain when the rate of loss of initial stores of heat, diminishing as the body grows colder, finally arrives at equilibrium with the radio-thermal supplies. This final state appears inevitable in every case unless the radio-active materials are so subordinate that they entirely perish before the original store of heat is exhausted.

Now, judging from the surface richness in radium of the earth and the present loss of terrestrial heat, it does not seem reasonable to assign a subordinate influence to radio-thermal actions; and it appears not improbable that the earth has attained, or nearly attained, this final stage of cooling.

How, then, may we suppose the existing thermal state maintained? A uniformly radio-active surface layer possessing a basal temperature in accordance with the requirements of geology is, I believe, not realisable on any probable estimate of the allowable radium, or on any concentration of it which my own experiments on igneous rocks would justify.

But we may take refuge in a less definite statement,



and assume a distribution by means of which the existing thermal state of the crust may be maintained. A specially rich surface layer we must recognise, but this need be no more than a very few miles deep; after which the balance of the radium may be supposed distributed to any depth with which we are thermally connected. Below that our knowledge is indefinite. The heat outflow at the surface is in part from the surface radium, in part due to the cooling arising from the diminishing amount of uranium, in part from the deep-seated radium. In this manner the isogeotherms are kept in their places, and a state is maintained which is in equilibrium with the thermal factors involved, but which cannot be considered steady, using the word in a strictly accurate sense, in view of the decay of the uranium.

While the existing thermal state may, I think, thus be maintained by radio-active heating and radio-active decay, we find ourselves in considerable difficulties if we extend this view into the past and assume that the same could be said of any previous stage of the earth's history. If the heat emitted by the earth, when the surface was at melting temperature, was in a state of equilibrium with the radio-active supplies, then, at that date, there must have been many thousands of times the present amount of uranium on the earth, and the period of the *consistentior status* must be put back by thousands of millions of years. Apart from hopeless contradiction with every geological indication as to the age of the earth, difficulties in solar physics arise. For the sun must be supposed of equal duration, and we are required to assume impossible amounts of uranium to maintain his heat all that great lapse of time; and again this uranium would perish at just the same rate as that upon the earth, so that at the present time the solar mass must be, for by far the greater part, composed of inert materials of high atomic weight: the products of the transformations of the uranium family. The difficulty is best appreciated when we consider that even to maintain his present rate of heat-loss by radium supplies, some 60 per cent. of his mass must be composed of uranium. But there are other troubles to face if we adopt this view. The earth, or rather those parts of it which are sufficiently near the surface to lose heat at the requisite rate, would have cooled but one per cent. in  $10^8$  years. Shrinkage of the outer parts and crustal thickness will be proportionately small, and we must put back our epochs of mountain building to suit so slow a rate of cooling and shrinkage and refer the earlier events of the kind to a past of inconceivable remoteness. Otherwise we must abandon the only tenable theory of mountain formation with which we are acquainted. On such a time-scale the ocean would be supersaturated under the influence of the prolonged denudation like the waters of certain salt lakes, and the sediments would have accumulated a hundredfold in thickness.

Nor do the facts as we know them require from us such sacrifices. We are not asked to raise these difficulties on supposititious quantities of uranium for the existence of which there is no evidence. Radium has occasioned no questioning of the older view that the cooling of the earth from a *consistentior status* has been mainly controlled by radiation. But, on the contrary, this new revelation of science has come to smooth over what difficulties attended the reconciliation of physical and geological evidence on the Kelvin hypothesis. It shows us how the advent of the present thermal state might be delayed and geological time lengthened, so that Kelvin's forty or fifty million years might be reconciled with the hundred million years which some of us hold to be the reading of the records of denudation.

On this more pacific view of the mission of radium to geology, what has been the history of the earth? In the earlier days of the earth's cooling the radiation loss was far in excess of the radio-thermal heating. From this state by a continual convergence, the rate of radiation loss diminishing while the radio-thermal output remained comparatively constant, the existing distribution of temperature near the surface has been attained when the radio-thermal supply may nearly or quite balance the loss by radiation. The question of the possibility of final and perfect equilibrium between the two seems to involve the interior conductivity and in this way to evade analysis.

It will be asked if the facts of mountain building and earth-shrinkage are rendered less reconcilable by this interference of uranium in the earth's physical history. I believe the answer will be in the negative. True, the greatest development of crustal wrinkling must have occurred in earlier times. This must be so, in some degree, on any hypothesis. The total shrinkage is, however, not the less because delayed by radio-thermal actions, and it is not hard to point to factors which will attend the more recent uprising of mountain chains tending to make them excel in magnitude those arising from the stresses in an earlier and thinner crust.

#### *Underground Temperature.*

It would be a matter of the highest interest if we could definitely connect the rise of temperature which is observed in deep borings and tunnels with the radio-activity of the rocks. We are confronted, however, by the difficulty that our deepest borings and tunnels are still too near the surface to enable us to pronounce with certainty on the influence of the radium met with in the rocks. This will be understood when it is remembered that a merely local increase of radio-activity must have but little effect upon the temperature unless the increase be of a very high order indeed. A clear understanding of this point shows us at once how improbable it is that volcanic temperatures can be brought within a very few miles of the surface by local radio-activity of the rocks. To account on such principles for an elevation of temperature of, say,  $1200^\circ$  at a depth of three or four miles from the surface, a richness in radium must be assumed far transcending anything yet met with in considerable rock masses; and as volcanic materials appear to show nothing of such exceptional richness in radium we can hardly suppose local radio-activity of the upper crust responsible for volcanic phenomena.

When we come to apply calculation to results on the radio-activity of the materials penetrated by tunnels and borings, we at once find that we require to know the extension downwards of the rocks we are dealing with before we can be sure that radium will account for the thermal phenomena observed. At any level between the surface and the base of a layer of radio-active materials—suppose the level considered is that of a tunnel—the temperature depends, so far as it is due to local radium, on the total depth of the rock-mass having the observed radio-activity. This is evident. It will be found that for ordinary values of the radium content it is requisite to suppose the rocks extending downwards some few kilometres in order to account for a few degrees in temperature at the level under observation. There is, of course, every probability of such a downward extension. Thus in the case of the Simplon massif the downward continuance of the gneissic rocks to some few kilometres evokes no difficulties. The same may be said of the granite of the Finsteraarhorn massif and the gneisses of the St. Gothard massif, materials both of which are penetrated by the St. Gothard tunnel, and which appear to possess a considerable difference in radio-activity. In dealing with this subject, comparison of the results obtained at one locality with those obtained at another is the safest procedure. We must accordingly wait for an increased number of results before much can be inferred. I will now lay the cases of the two great tunnels as briefly as possible before you.

And first as to the temperature effects observed in the two cases.

The Simplon tunnel for a length of some seven or eight kilometres lies at a mean distance of about 1700 metres from the surface. At the northerly end of this stretch the rock temperature attains  $55^\circ$ , and at the southern extremity has fallen to about  $35^\circ$ . The temperature of  $55^\circ$  is the highest encountered. The maximum predicted by Stappf, basing his estimates on his experience of the St. Gothard tunnel, was  $47^\circ$ . Other authorities in every case predicted considerably lower temperatures. Stockalper, who also had experience of the St. Gothard, predicted  $36^\circ$  at a depth of 2050 metres from the surface, and Heim  $38^\circ$  to  $39^\circ$ .<sup>1</sup>

<sup>1</sup> See the account given by Schardt, *Verhandl. Schweizerischen Naturf. Gesellsch.*, 1904, lxxvii., "Jahresversammlung," p. 204 et seq.



When the unexpectedly high temperatures were met with, various reasons were assigned. Mr. Fox has suggested volcanic heat. Others point to the arrangement of the schistosity and the dryness of the rocks, where the highest temperatures were read. The latter is evidently to be regarded more as explanation of the lower temperatures at the south end of the tunnel, where the water circulation was considerable, than of the high temperatures of the northern end. The schistosity may have some influence in bringing the isotherms nearer to the surface; however, not only are the rocks intensely compact in every direction, but what schistosity there is by no means inclines in the best directions for retention of heat. From the sections the schistosity appears generally to point upwards at a steep angle with the tunnel axis.<sup>1</sup>

Where there is such variability in the temperatures, irrespective of the depth of overlying rock, there is difficulty in assigning any significant mean gradient. The highest readings are obviously those least affected by the remarkable water-circulation of the Italian side. The higher temperatures afford such gradients as would be met in borings made on the level—about 31 metres per degree.

The temperatures read in the St. Gothard rocks were of a most remarkable character. For the central parts of the tunnel the gradients come out as 46.6 metres per degree. Stapff, who made these observations and conducted the geological investigations, took particular pains to ascertain the true surface temperatures of the rock above the tunnel; and from these ascertained temperatures, the temperatures in the tunnel rock and the overlying height of mountain, he calculated the gradients.

But this low gradient is by no means the mean gradient. At the north end, where the tunnel passes through the granite of the Finsteraarhorn massif, there is a rise in the temperature of the rock sufficient to steepen the gradient to 20.9 metres per degree. Stapff regarded this local rise of temperature as unaccountable save on the view that the granite retained part of the original heat. This matter I will presently return to.

Now, it is a fact that the radium-content of the Simplon rocks, after some allowance for what I have referred to as sporadic radium, stands higher than is afforded by the rocks in the central section of the St. Gothard, where the gradient is low. For the Simplon the general mean is (on my experiments) 7.1 billionths of a gram per gram. This mean is well distributed as follows:—

Jurassic and Triassic altered sediments	... ..	6.4
Crystalline schists, partly Jurassic and Triassic, partly Archaean	... ..	7.3
Monte Leone gneiss and primitive gneiss	... ..	6.3
Schistose gneiss (a fold from beneath)...	... ..	6.5
Antigorio gneiss	... ..	6.8

The divisional arrangement is Prof. Schardt's. Forty-nine typical rocks are used in obtaining these results, and the experiments have been in many cases repeated on duplicate specimens. Including some very exceptional results, the mean would rise to  $9.1 \times 10^{-12}$  grams per gram.

Of the St. Gothard rocks I have examined fifty-one specimens selected to be, as far as attainable, representative.<sup>2</sup>

Of these, twenty-one are from the central region, and their mean radium content is just 3.3. The portion of the tunnel from which these rocks come is closely coincident with Stapff's thermal subdivision of regions of low temperature.<sup>3</sup> This portion of the mountain offers the most definite conditions for comparison with the Simplon results. The region south of this is affected by water circulation; the regions to the north are affected by the high temperature of the granite.

We see, then, that the most definite data at our disposal in comparing the conditions as regards temperature and radio-thermal actions in the two tunnels appear

<sup>1</sup> Schardt, *loc. cit.*

<sup>2</sup> I would like to express here my acknowledgments to the Trustees of the British Museum for granting me permission to use chips of the rocks in their possession; and especially to Mr. Prior for his valuable assistance in selecting the specimens.

<sup>3</sup> Trans. North of England Mining and Mec. Engineers, xxxiii., p. 25.

to show that the steeper gradient is associated with the greater radium-content.

It is possible to arrive at an estimate of the downward extension of the two rock masses (assumed to maintain to the same depth their observed radio-activity), which would account for the difference in gradient. In making this estimate, we do not assume that the entire heat-flow indicated by the gradients is due to radium, but that the difference in radium-content is responsible for the difference of heat-flow. If some of the heat is conducted from an interior source (of whatever origin), we assume that this is alike in both cases. We also assume the conductivities alike.

Calculating on this basis, the depth required to establish on the radium measurements the observed difference in gradients of the Central St. Gothard and of the Simplon, we find the depth to be about 7 kilometres on the low mean of the Simplon rocks, and 5 kilometres on the high mean. There is, as I have already said, nothing improbable in such a downward extension of primitive rocks having the radio-activities observed; but as a different distribution of radium may, of course, obtain below our point of observation, the result can only claim to be suggestive.

Turning specially to the St. Gothard, we find that a temperature problem of much interest arises from the facts recorded. The north end of the tunnel for a distance of 2 kilometres traverses the granite of the Finsteraarhorn massif. It then enters the infolded syncline of the Usern-mulde and traverses altered sediments of Trias-Jura age for a distance of about 2 kilometres. After this it enters the crushed and metamorphosed rocks of the St. Gothard massif, and remains in these rocks for  $7\frac{1}{2}$  kilometres. The last section is run through the Tessinmulde for 3 kilometres. These rocks are highly altered Mesozoic sediments.

I have already quoted Stapff's observations as to the variations of gradient in the northern, central, and southern parts of the tunnel. He writes: "They (the isotherms) show irregularities on the south side, which clearly depend on cold springs, they bend down rapidly, and then run smoothly inclined beneath the water-filled section of the mountain. Other local irregularities can be explained by the decomposition of the rock; but there is no obvious explanation of the rapid increase in the granite rocks at the northern end of the tunnel (2000 metres), and it is probably to be attributed to the influence of different thermal qualities of the rock on the coefficient of increase. For the rest these 2000 metres of granite belong to the massif of the Finsteraarhorn, and, geologically speaking, they do not share in the composition of the St. Gothard. Perhaps these two massifs belong to different geological periods (as supposed for geological reasons long ago). What wonder, then, if one of them be cooler than the other." (*Loc. cit.*, p. 30.)

Commenting on the explanation here offered by Stapff, Prestwich<sup>1</sup> states his preference for the view that the excess of temperature in the granite is due to mechanical actions to which the granite was exposed during the upheaval of this region of the Alps.

The accompanying diagram shows the distribution of temperature as given by Stapff, and the distribution of radium as found from typical specimens of the rocks. There is a correspondence between the two which is obvious, and when it is remembered that the increase in radio-activity shown at the south end would have been, according to Stapff, masked by water circulation, the correspondence becomes the more striking. The small radium values in the central parts of the tunnel are remarkable. The rocks of the Central St. Gothard massif are apparently exceptionally poor in radium.

At the north end the excess of radium is almost confined to the granite, the rock to which Stapff ascribed the exceptional temperatures. The radium of the Usern-mulde is probably not very important, seeing that these sediments cannot extend far downwards. The principal local source of heat appears located more especially beneath the synclinal fold, for Stapff's table (*loc. cit.*, p. 31) of the gradients beneath the plain of Andermatt shows a rising gradient to a point about 2500 metres

<sup>1</sup> Proc. R.S., xli., p. 44.



from the north entrance of the tunnel. It is observable that the radio-activity of the granite increases as it approaches the Usernmulde and attains its maximum (14.1) where it dips beneath the syncline.

The means of radium-content in the several geological sections into which the course of the tunnel is divisible are as follows :—

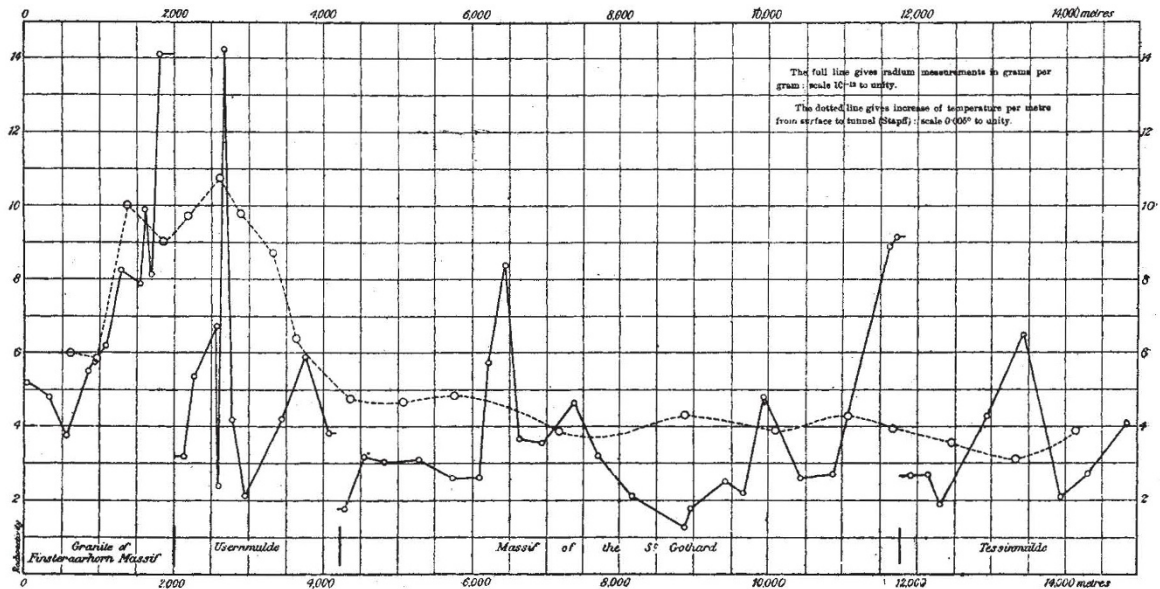
Granite of Finsteraarhorn ... ..	7.7
Usernmulde ... ..	4.9
St. Gothard massif ... ..	3.9
Tessinmulde ... ..	3.4

The central section, however, if considered without reference to geological demarcations, would, as already observed, come out as barely 3.3. And this is the value of the radio-activity most nearly applicable to Stapff's thermal subdivision of the region of low temperature.

If we accept the higher readings obtained in the granite as indicative of the radio-active state of this rock beneath the Usernmulde, a satisfactory explanation of the difference of heat-flow from the central and northern parts of the tunnel is obtained. Using the difference of gradient as basis of calculation, as before, we find that a downward extension of about six thousand metres would, if

folding up of the great beds of sediment, and even their over-thrusting for many miles. So that the mountain ranges of the world are not constituted from materials rising from below, save in so far as these may form a sustaining core, but of the slowly accumulating deposits of the ages preceding the upheaval.

The thickness of collected sediments involved in these great events is enormous, and although uncertainty often attends the estimation of the aggregate depths of sedimentation, yet when we consider that unconformities between the deposits of succeeding eras represent the removal of vast masses of sediment to fresh areas of deposition, and often in such a way as to lead to an underestimate of the thickness of deposit, the observations of the geologist may well indicate the minor and not the major limit. Witness the mighty layers of the Huronian, Animikean, and Keweenawan ages where deposits measured in miles of thickness are succeeded by unrecorded intervals of time, in which we know with certainty that the tireless forces of denudation laboured to undo their former work. Each era represents a slow and measured pulse in the earth's crust, as if the overloading and sinking of the surface materials induced the very conditions required for their re-elevation. Such events,



the outflow took place in an approximately vertical direction, account for the facts observed by Stapff. This depth is in agreement with the result as to the downward extension of the St. Gothard rocks as derived from the comparison with the Simplon rocks.

We are by no means in a position to found dogmatic conclusions on such results; they can only be regarded as encouragement to pursue the matter further. The coincidence must be remarkable which thus similarly localises radium and temperature in roughly proportional amounts, and permits us, without undue assumptions, to explain such remarkable differences of gradient. There is much work to be done in this direction, for well-known cases exist where exceptional gradients in deep borings have been encountered—exceptional both as regards excess and deficiency.

*Radio-active Deposits and the Instability of the Crust.*

At the meeting of the British Association held last year at Leicester, I read a note on the thermal effects which might be expected to arise at the base of a sedimentary accumulation of great thickness due to the contained radium.

The history of mountain building has repeated itself many times: ages of sedimentation, with attendant sinking of the crust in the area of deposition, then upheaval,

even in times when the crust was thinner and more readily disturbed than it is now, must have taken vast periods of time. The unconformity may represent as long a period as that of accumulation. In these Proterozoic areas of America, as elsewhere on the globe and throughout the whole of geological history, there has been a succession in time of foldings of the crust always so located as to uplift the areas of sedimentation, these upheavals being sundered by long intervals during which the site of sedimentation was transferred and preparation made for another era of disturbance. However long deferred there seems to be only the one and inevitable ending, inducing a rhythmic and monotonous repetition surely indicative of some cause of instability attending the events of deposition.

The facts have been impressively stated by Dana: "A mountain range of the common type, like that to which the Appalachians belong, is made out of the sedimentary formations of a long preceding era; beds that were laid down conformably, and in succession, until they had reached the needed thickness; beds spreading over a region tens of thousands of square miles in area. The region over which sedimentary formations were in progress in order to make, finally, the Appalachian range, reached from New York to Alabama, and had a breadth of 100 to 200 miles, and the pile of horizontal beds along the



middle was 40,000 feet in depth. The pile for the Wahsatch Mountains was 60,000 feet thick, according to King. The beds for the Appalachians were not laid down in a deep ocean, but in shallow waters, where a gradual subsidence was in progress; and they at last, when ready for the genesis, lay in a trough 40,000 feet deep, filling the trough to the brim. It thus appears that epochs of mountain making have occurred only after long intervals of quiet in the history of a continent.<sup>1</sup>

The generally observed fact that the deposition of sediments in some manner involves their ultimate upheaval has at various times led to explanations being offered. I think I am safe in saying that although the primary factor, the compressive stress in a crust which has ceased to fit the shrinking world within it, has probably been correctly inferred, no satisfactory explanation of the connection between sedimentation and upheaval has been advanced. The mere shifting upwards of the isogeotherms into the deposits, advanced as a source of local loss of rigidity by Babbage and Herschel, need not involve any such loss so long as the original distance of the isogeotherms from the surface is preserved.

We see in every case that only after great thicknesses of sediments have accumulated is the upheaval brought about. This is a feature which must enter as an essential condition into whatever explanation we propose to offer.

Following up the idea that the sought-for instability is referable to radio-thermal actions, we will now endeavour to form some approximate estimate of the rise of temperature which will be brought about at the base of such great sedimentary accumulations as have gone towards mountain building, due to the radium distributed throughout the materials.

The temperature at the base of a feebly radio-active layer, such as an accumulation of sediments, is defined in part by radio-active energy, in part by its position relative to the normal isogeotherms, whether these latter are in turn due to or influenced by radio-thermal supplies or not. It is convenient, and I think allowable, to consider these two effects separately, and deal with them as if they were independent, the resultant state being obtained by their summation.

In dealing with the rise of temperature at the base of a radio-active layer we arrive at an expression which involves the square of the depth. This is a very important feature in the investigation, and leads to the result that, for a given amount of radium, diffuse distribution through a great depth of deposit gives rise to a higher basal temperature than a more concentrated distribution in a shallower layer.

But this will not give us the whole effect of such a deposit. Another and an important factor has to be taken into account. We have seen that the immediate surface rocks are of such richness in radium as to preclude the idea that a similar richness can extend many miles inward.

Now, it is upon this surface layer that the sediments are piled, and as they grow in thickness this original layer is depressed deeper and deeper, yielding under the load until at length it is buried to the full depth of the overlying deposit. This slow and measured process is attended by remarkable thermal effects. The law of the increase of temperature with the square of the depth comes in, and we have to consider the temperature effect not merely at the base of the deposited layer, but that due to the depression and covering over of the radium-rich materials upon which the sediments were laid down.

The table which follows embodies an approximate statement of the thermal results of various depths of deposit supposed to collect under conditions of crustal temperature such as prevail in this present epoch of geological history:—

Thickness of sedimentary deposit	Resulting rise of isogeotherms	Weakening of earth's crust as defined by the rise of the geotherm at 40 kilometres
Kilometres	Kilometres	Kilometres
6	7.4	40 to 32.6
8	10.2	40 to 29.8
10	13.3	40 to 26.7
12	16.7	40 to 23.3
14	20.4	40 to 19.6

I have deferred to the conclusion of this address an account of the steps followed in obtaining the above results. It is clearly impossible, within the limited time allotted to me, to make these quite clear. It must suffice here merely to explain the significance of the figures.

The first column gives the depth of sedimentary deposit supposed to be laid down on the normal radio-active upper crust of a certain assumed thickness and radio-activity. From the rise of temperature which occurs at the base of this crust (due to the radio-activity, not only of the crust, but of the sediments) the results of the second column are deduced, the gradient or slope of temperature prevailing beneath being derived from the existing surface gradients corrected for the effects of the radio-thermal layer. The third column is intended to exhibit the effect of this shift of the geotherms in reducing the strength of the crust. I assume that at a temperature of 800° the deep-seated materials lose rigidity under long-continued stress. The estimated depth of this geotherm is, on the assumptions, about 40 kilometres. The upward shift of this geotherm shows the loss of strength. Thus in the case of a sedimentary accumulation of 10 kilometres the geotherm defining the base of the rigid crust shifts upwards by 13 kilometres, so that there is a loss of effective section to the amount of 30 per cent.<sup>1</sup>

As regards the claims which such figures have upon our consideration, my assumptions as to thickness and radio-activity of the specially rich surface layer are, doubtless, capable of considerable amendment. It will be found, however, that the assumed factors may be supposed to vary considerably, and yet the final results prove such as, I believe, cannot be ignored. Indeed, those who are in the way of making such calculations, and who enter into the question, will find that my assumptions are not specially favourable, but are, in fact, made on quite independent grounds. Again, a certain class of effects has been entirely left out of account, effects which will go towards enhancing, and in some cases greatly enhancing, the radio-thermal activity. I refer to the thickening of the crust arising from tangential pressure, and, at a later stage, the piling up and overthrusting of mountain building materials. In such cases the temperature of the deeper parts of the thickened mass must still further rise under the influence of the contained radium. These effects only take place, indeed, after yielding has commenced, but they add to the element of instability which the presence of the accumulated radio-active deposits occasions, and doubtless increase thermal metamorphic actions in the deeper sediments, and result in the refusion of rocks in the upper part of the crust.<sup>2</sup>

The effect of accumulated sediment is thus necessarily a reduction in the thickness of that part of the upper crust which is capable of resisting a compressive stress. Over the area of sedimentation, and more especially along the deepest line of synclinal depression, the crust of the globe for a period assumes the properties belonging to an earlier age, yielding up some of the rigidity which was the slow inheritance of secular cooling. Along this area of weakness—from its mode of formation generally much elongated in form—the stressed crust for many hundreds, perhaps thousands, of miles finds relief, and flexure takes place in the only possible direction; that is, on the whole upwards. In this way the prolonged anticline bearing upwards on its crest the whole mass of deposits is formed, and so are born the mountain ranges in all their diversity of form and structure.

We have in these effects an intervention of radium in the dynamics of the earth's crust, which must have influenced the entire history of our globe, and which, I believe, affords a key to the instability of the crust. For after the events of mountain building are accomplished, stability is not attained, but in presence of the forces of denudation the whole sequence of events has to commence over again. Every fresh accession of snow to the firn, every passing cloud contributing its small addition to the

<sup>1</sup> See Appendix B.

<sup>2</sup> Prof. C. Schmidt (Basel) has recently given reasons for the view that the Mesozoic schists of the Simplon at the period of their folding were probably from 15,000 to 20,000 metres beneath the surface ("Ec. Geol. Helvetica," vol. ix., No. 4, p. 590). As another instance consider the compression of the Laramide range (Dawson, Bull. Geol. Soc. Am., xii., p. 87).



torrent, assists to spread out once more on the floor of the ocean the heat-producing substance. With this rhythmic succession of events appear bound up those positive or negative movements of the strand which cover and uncover the continents, and have swayed the entire course of evolution of terrestrial life.

*Oceanic Deposits.*—The displacements of the crust which we have been considering are now known to be by no means confined to the oceanic margins. The evidence seems conclusive that long-continued movements have been in progress over certain areas of the sea floor, attended with the formation of those numerous volcanic cones upon which the coral island finds foundation. Here there are plainly revealed signs of instability and yielding of the crust (although, perhaps, of minor intensity) such as are associated with the greater movements which terminate in mountain building. I think it will be found, when the facts are considered, that we have here phenomena continuous with those already dealt with, and although the conditional element of a sufficient sedimentary accumulation must remain speculative, the evidence we possess is in favour of its existence.

One of the most interesting outstanding problems of deep-sea physiography is that of the rates of accumulation of the several sorts of deposit. In the case of the more rapidly collecting sediments there seems no serious reason why the matter should not be dealt with observationally. I hope it may be accomplished in our time. For my present purpose I should like to know what may or may not be assumed in discussing the accumulation of radio-active sediments on the ocean floor.

As regards the rate of collection of the non-calcareous deposits, the nearest approach to an estimate is, I think, to be obtained from the exposed oceanic deposits of Barbados. In the well-known paper of Jukes Brown and Harrison<sup>1</sup> on the geology of that island, it is shown that the siliceous radiolarian earths and red clays aggregate to a thickness of about 300 feet. These materials are true oceanic deposits, devoid of terrigenous substances. They collected very probably during Pliocene and, perhaps, part of Pleistocene times. Now, there is evidence to lead us to date the beginning of the Pliocene as anything from one million to three million years ago. The mean of these estimates gives a rate of collection of 5 millimetres in a century. This sounds a very slow rate of growth, but it is too fast to be assumed for such deposits generally. More recent observations might, indeed, lead us to lengthen the period assigned to the deposition of these oceanic beds; for if, following Prof. Spencer,<sup>2</sup> we ascribe their deposition to Eocene times, a less definite time-interval is indicated; but the rate could hardly have been less than 3 millimetres in a century. The site of the deposit was probably favourable to rapid growth.

We have already found a maximum limit to the average thickness of true oceanic sediments; and such as would obtain over the ocean floor if the rate of collection was everywhere the same and had so continued during the past. If there is one thing certain, however, it is that the rates of accumulation vary enormously. The 1200 or 1500 feet of chalk in the British Cretaceous, collected in one relatively brief period of submergence, would alone establish this. Huxley inferred that the chalk collected at the rate of 1 inch in a year. Sollas showed that the rate was more probably 1 inch in forty years. Sir John Murray has advanced evidence that in parts of the Atlantic the cables become covered with Globigerina ooze at the rate of about 10 inches in a century. Finally, then, we must take it that the fair allowance of one-seventh of a mile may be withheld in some areas and many times exceeded in others.

Now it is remarkable that all the conditions for rapid deposition seem to prevail over those volcanic areas of the Pacific from which ascend to the surface the coral islands—abundant pelagic life and comparatively shallow depths. Indeed, I may remind you that the very favourable nature of the conditions enter into the well-known theory of coral island formation put forward by Murray.

The islands arise from depths of between 1000 and 2000

<sup>1</sup> O. I. G. S., xlviil., p. 270.

<sup>2</sup> *Ibid.*, lviii., p. 354 *et seq.*

fathoms. These areas are covered with Globigerina ooze having a radio-activity of about 7 or 8. The deeper-lying deposits around—red clay and radiolarian ooze—show radio-activities up to and more than 50. From these no volcanic islands spring.

These facts, however, so far from being opposed to the view that the radio-activity and crustal disturbance are connected, are in its favour. For while those rich areas testify to the supply of radio-active materials, the slow rate of growth prevailing deprives those deposits of that characteristic *depth* which, if I may put it so, is of more consequence than a high radio-activity. For the rise in temperature at the base of a deposit, as already pointed out, is proportional to the square of the thickness; in reality the dilution of the supplies of uranium which reach the calcareous oozes flooring the disturbed areas is a necessary condition for any effective radio-thermal actions.

It might appear futile to consider the matter any closer where so little is known. But in order to give an idea of the quantities involved I may state that, if my calculations are correct, a rate of deposit comparable with that of the chalk prevailing for ten million years would, on assumptions similar to those already explained when discussing the subject of mountain building, occasion a rise of the deeper isotherms by from 20 to 30 per cent. of their probable normal depth.

In making these deductions as to the influence of radium in sedimentary deposits, I have so far left out of consideration the question of the time which must elapse in order that the final temperature-rise in the sediments must be attained. The question we have to answer is: Will the rate of rise of temperature due to radium keep pace with the rate of deposition, or must a certain period elapse after the sedimentation is completed to any particular depth, before the basal temperature proper to the depth is attained?

The answer appears to be, on an approximate method of solution, that for rates of deposition such as we believe to prevail in terrigenous deposits—even so great as 1 foot in a century, and up to depths of accumulation of 10 kilometres and even more—the heating waits on the sedimentation. Or, in other words, there is thermal equilibrium at every stage of growth of the deposit; and the basal temperature due to radio-active heating may at any instant be computed by the conductivity equation. For accumulations of still greater magnitude the final and maximum temperature appears to lag somewhat behind the rate of deposition.

From this we may infer that the great events of geological history have primarily waited upon the rates of denudation and sedimentation. The sites of the terrigenous deposits and the marginal oceanic precipitates have many times been convulsed during geological time because the rates of accumulation thereon have been rapid. The comparative tranquillity of the ocean floor far removed from the land may be referred to the absence of the inciting cause of disturbance. If, however, favourable conditions prevail for such a period that the local accumulations attain the sufficient depth, here, too, the stability must break down and the permanency be interrupted.

Upheaval of the ocean floor, owing to the laws of deep-sea sedimentation, should be attended with effects accelerative of deposition—a fact which may not be without influence. But although ultimately sharing the instability of the continental margins, the cycle of change is tuned to a slower periodicity. From the operation of these causes, possibly, have come and gone those continents which many believe to have once replaced the wastes of the oceans, and which with all their wealth of life and scenic beauty have disappeared so completely that they scarce have left a wreck behind. But those forgotten worlds may be again restored. The rolled-up crust of the earth is still rich in energy borrowed from earlier times, and the slow but mighty influences of denudation and deposition are for ever at work. And so, perchance, in some remote age the vanished Gondwana Land, the lost Atlantis, may once again arise, the seeds of resurrection even now being sown upon their graves from the endless harvests of pelagic life.



## APPENDIX A.

*Convective Movement of Uranium to the Earth's Surface.*

—The estimate of temperature given assumes (1) that the mass of igneous material is spherical, and (2) that its surface is kept at constant temperature, heat escaping freely. The first assumption is in favour of increasing the estimate of temperature, and probably would not generally be true, especially of a mass moving upwards. The second assumption tends to give a lower estimate of temperature, and is certainly misleading, as the surrounding materials are non-conducting, and must favour the accumulation of radio-active heat.

On assumptions (1) and (2) and on Barus' results for the thermal expansion of diabase between 1100° and 1500°,<sup>1</sup> and results of my own on basalt,<sup>2</sup> which are in approximate agreement, and assuming the mean excess of temperature to be 500° and the surrounding material to be at a fluid temperature, the force of buoyancy comes out at more than 60 dynes per cubic centimetre of the spherical mass. This is an under-estimate.

If we may assume that the Deccan Trap is indeed an instance of such an over-heated mass escaping at the surface, and that similar radio-active masses rising up from beneath at various times in the past may have affected the crust, we have at our disposal a local source of energy of plutonic origin which may account for much.

## APPENDIX B.

*Sedimentation and Rise of Geotherms.*—The depth of the upper radio-active layer is, of course, unknown. We possess, however, the means of arriving at some idea of what it must be. The quantitative thermal conditions impose a major limit to its average thickness, and the indications of injected rocks suggest a minor limit.

It will be found that if  $2.6 \times 10^{20}$  calories is the heat output of the whole earth per annum, and if we assign only one-fifth of this amount to cooling due to decay of the uranium, then, on the assumption that the earth is no longer losing any part of its original store of heat, we have about  $2 \times 10^{20}$  representing radium heating. From this the allowance of terrestrial radium per square centimetre inwards is  $2.3 \times 10^{-5}$  grams. This would give a major limit. But it is almost certain that some of this radium is located in more deeply seated parts of the earth. If we take  $10^{-5}$  as contained in the normal radio-active surface layer, and assume (what according to my experiments should not be far from the truth) that the average radio-activity is 3, we arrive at a thickness of 12 kilometres.

Some such mean value is necessitated by the evidence we derive from the radio-activity of igneous rocks. These rocks must in many cases be derived from considerable depths. Such outflows as the Deccan may indicate local sub-crustal conditions; so also may the eruptions of certain volcanic areas. But those extrusions which have attended mountain building, more especially its closing phases, appear to indicate general conditions, and involve the existence of such radio-active materials at considerable depths. If we assume a thickness for the radio-active part of the crust much less than the 12 kilometres, difficulties are met with on this line of reasoning.<sup>3</sup>

Proceeding now to the derivation of the results given in the table, p. 464. The equation  $k\theta = qhx(D - x/2)$  (where  $\theta$  is the temperature at the depth  $x$ ,  $D$  being the total depth of the radio-active layer,  $q$  the radium per c.c. in grams,  $h$  the heat output of one gram of radium per second,  $k$  the thermal conductivity) is easily derived by considering the conditions of thermal flow in the layer, supposed to lose heat only at the surface.<sup>4</sup>

The aggregate depths of radio-active material in the several cases of sedimentary deposit assumed in my address amount to 18, 20, 22, 24, and 26 kilometres. I assume the mean radio-activity to be 3.5, and the average conductivity to be  $4 \times 10^{-3}$ . From this the basal temperatures are found, as due to radio-thermal actions. These temperatures are to be augmented by the temperatures

proper to the several depths, which depend upon the conducted interior heat. To estimate these we require to apportion the observed average surface gradient (taken as 32 metres per degree) between radio-active effects in the upper layer and the flow of heat from within. The radio-thermal gradient comes out at about 75 metres; the inner gradient is accordingly 56 metres. Hence the total temperature at the base of each radio-active mass is obtained. But the geotherms proper to the several depths, 18, 20, &c., kilometres, under conditions prevailing elsewhere in the crust, are easily found from the value of  $\theta$  for the normal layer (82° C.), and adding the temperature due to interior heat. From the difference of the temperatures we, finally, find the rise of the geotherms.

As conveyed in my address, I have found on several different values of the thickness and radio-active properties of the surface layer, results in every case showing large values for the rise of the geotherms. The data assumed above are by no means the most favourable.

## NOTES.

It is with deep regret that we learn of the sudden death of Prof. Alexis Hansky, whose work in solar physics at the Pulkowa Observatory has attracted so much attention. According to a letter from M. Tikhoff, which appears in the September number of the *Bulletin de la Société astronomique de France*, M. Hansky was drowned whilst bathing in the Black Sea at Simeise, in the Crimea, on August 11 (July 29 O.S.). The deceased astronomer commenced his practical work in solar physics by observing the total eclipse of 1896 at Novaya Zemlya, and at the time of his tragic death was engaged in the installation of a new observatory in the Crimea which had been given to him, and which he had handed over to the Pulkowa Observatory. By his death at the early age of thirty-six years, the study of solar physics has suffered a loss which it will be exceedingly difficult to repair.

THE President of the Local Government Board has arranged for the making of the two following researches:—a chemical and bacteriological investigation, by Mr. C. G. Moor and Prof. R. T. Hewlett, as to the influence of softening and of other chemical processes on the purity of water supplies from the chalk as shown in actual experience and under experimental conditions, and an investigation by Prof. Sidney Martin, F.R.S., into the powers of production of disease possessed by certain streptococci and by the poisonous substances produced by them, in continuance of previous investigations by him on the same subject. These investigations complete the allocation of the scientific grant for the year 1908-9.

PROF. ROBERT KOCH has been chosen to represent the German Government at the forthcoming International Tuberculosis Congress at Washington.

THE annual conference of the Sanitary Inspectors' Association opened on Tuesday last under the presidency of Sir James Crichton-Browne, F.R.S.

THE third International Philosophy Congress has been in session at Heidelberg during a portion of the past week. The next meeting will take place in 1912, at Bologna.

THE arrangements for the fourth International Fisheries Congress, which, as has already been announced, is to be held at Washington from September 22-26, are now complete. An attractive itinerary has been arranged for the week following the sessions of the congress, and

<sup>1</sup> Phil. Mag., xxxv., p. 173. <sup>2</sup> Trans. R.D.S., vi., p. 208.  
<sup>3</sup> See p. 464, ante, and foot-note as bearing on the possible displacement of the geotherms.

<sup>4</sup> See Strutt, Proc. R. S., lxxvii., p. 482.