The Age of the Earth.[1]

By PROF. J. JOLY, F.R.S.

"THE Age of the Earth" is a somewhat ambiguous phrase. From the geological point of view it is generally understood to mean the age of the ocean : in other words the age of the earth since the beginning of those geological surface changes which are due to denudation. But another meaning may be ascribed to the term. We may assume the beginning to date from the cooling of a highly heated surface to the point of solidification. In this case we include in the age those long periods of Archæan time during which the activity of water played a subordinate part and volcanic commotion prevailed among the semi-fluid, rocky constituents of the globe. Yet a third interpretation refers the birth time to a still more remote and indefinite epoch when the world became differentiated as a planet by activities, of the nature of which we are ignorant. Astronomical deductions and speculations regarding the Age are mainly concerned with the last period.

What I have to say will be restricted, almost entirely, to the first interpretation of the term. I mean by the age of the earth the period which has elapsed since its surface became the scene of world-wide denudative forces and the foundations of organic evolution were laid.

In virtue of these denudative forces we find ourselves possessed of certain methods of estimating the Age which are valid upon the assumption that denudation proceeds in our time at a rate not greatly differing from its mean rate over geological time.

The bases of this assumption are as follows :—

(*a*) That the chief factor in denudative activity being the rain supply falling on the land, solar heat and atmospheric circulation are primary causes. The life on the globe since very early times and the narrow temperature limits conditioning protoplasmic existence and activity show that great extremes of solar radiation cannot have affected denudation for long periods in the past. Mere climatal extremes do not sensibly affect solvent denudation. Atmospheric circulation, being largely conditioned by the earth's rotation and the distribution of solar heat, cannot have varied to any effective extent.

(*b*) That a considerable percentage of the existing land area being rainless, changes in continental area cannot greatly affect the amount of denudation : the belt undergoing denudation being merely displaced outwards or inwards. The evidence derived from palæography and from the extent of sedimentary deposits in all ages shows that the present land area is not greatly different from the past mean area.

c) That the minor factors affecting solvent and detrital denudation being very many and of very different character are unlikely to combine at any time, and for any long period, in one direction, so as to create a considerable departure from the mean.

Time will not permit a discussion of these statements.

I shall refer but briefly to the methods by which the statistics of solvent and detrital denudation are used to afford the age of the ocean.

(1) The chemistry of the ocean and of the rocks is the key to our position. As the result of a comparative study of the primary or igneous rocks and the secondary or sedimentary rocks we find that, say, n grams of sodium are shed into the ocean for each tonne of igneous rock converted into sedimentary rock, and in the ocean we find N grams of sodium. The total denudation over geological time has, therefore, been N/n expressed in tonnes of denuded igneous rock. Our study also tells us the average total loss attending the conversion of the primary rock into sediment, and so we get the total of the secondary rocks in tonnes. We now go to the principal rivers of the world and availing ourselves of estimates which have been made of the amounts of sediment—*i.e.* of secondary rock material—which they transport from the land in a year, we calculate the number of years which it would take to lay down in the ocean the great mass of sediment generated in the past ages. After certain allowances this comes out as about 100 million years.

(2) Again the total of oceanic sodium may give us the Age in another and more direct way. We know that by far the greater part of this sodium was carried into it by the rivers during geological time. We turn to the analyses of river water and estimate the total annual supply of this element to the ocean. Dividing the latter into the former and making certain allowances we find an Age which is about 100 million years.

(3) A third and more difficult method is independent of our knowledge of chemical denudation. We estimate the maximum thickness of the integral sedimentary deposits, and knowing the burthen of sediment conveyed per annum by the rivers, we estimate the maximum thickness of deposit annually derived from the same ; we divide the latter into the former and find an age which, again, is about 100 million years.

Of these methods, that which involves the sodium modulus only is the most direct. Of course the reason for selecting this particular element as a modulus is because of its great solubility, on account of which it alone among the dissolved oceanic constituents has been preserved from organic abstraction or chemical precipitation. This method has been examined by many critics. Notably by Sollas, who, in a presidential address to the Geological Society in 1909, subjects it to searching examination. He concludes that a period of 175 millions of years may be reached upon certain assumptions, and that this must be very nearly the maximum allowable. My own examination of this method has led me to believe that it is *possible* that 150 millions of years may be indicated by it, and that 200 millions of years would not be reconcilable with our present knowledge of the factors involved. This would, as I have already stated, apply only to the duration of sedimentation. It cannot be compared with data which apply to an age dating back into the Archæan.

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[1] Discourse delivered at the Royal Institution on Friday, February 24.

There was, indeed, some scanty sedimentation in Archæan times. We cannot form any estimate of its effects either upon our numerator or upon our denominator save that we seem entitled to conclude that they were small. " The Archæan was essentially a period of world-wide vulcanism, and in the relative proportions of rocks of igneous and sedimentary origin represents a departure from the uniformity of conditions of later geological time." I quote from the monograph of Van Hise and Leith.

Before passing on to the results based upon radioactivity I must refer to one point in particular which has been urged against accepting present-day rates of denudation as a basis of time measurement. It is said we live in a period of abnormal continental elevation which, it is asserted, involves excessive solvent denudation. A little attention to the nature and conditions of solvent denudation should have sufficed to forestall the argument. But a ready method of dealing with it is available. The continent of North America has a mean elevation of 700 metres : it is being denuded at the rate of 79 tonnes per square mile per annum : for South America the corresponding figures are 650 metres and 50 tonnes. Now Europe has a much lower mean elevation—300 metres. Its rate of denudation is, however, 100 tons per square mile per annum. The rate of solvent denudation is, in fact, by measurement found to be *less* for the more elevated land, as, theoretically, it should be. The argument then, if it has any basis, would indicate that the age as found from solvent denudation is excessive.

Prior to the advent of those methods for investigating the earth's age, which are based on radio-active changes in the elements, no serious objections to the results reached by the geological methods were raised, so far as I know. There were some, indeed, who regarded the age as excessive. Thus Becker arrived at a lesser figure by taking into account the progressive impoverishment of the surface materials during geological time. The validity of the correction is, however, open to doubt. Others considered that the organic changes recorded in the rocks required a longer period. Sollas gave, as I think, a clear answer to this objection in his " Age of the Earth." Both Lyell and Geikie, and Poulton, had in past years upheld the doctrine of Uniformity. But the advent of the radio-active method, as founded on the uranium family of elements, seemed to point to a vastly greater Age ; leading, in fact, to the extraordinary conclusion that the present rate of solvent denudation is not less than four times and may be eight (or even more) times in excess of the average rate obtaining during the past.

The earliest suggestion of the possibility of using the stored-up products of radio-active change came from Rutherford. He, and later Strutt (now Lord Rayleigh), applied the accumulation of helium to the evaluation of geological time. Strutt laid out a geological chronology, the first of its kind, but considered he was dealing with minor limits. Boltwood used the residual product of uranium—lead—and for Archæan (?) materials reached as much as 1640 million years. As I have already said, the denudative method cannot be regarded as extending to those remote times. But such results as 430×10^6 years

for Silurian or Ordovician deposits, and 1200×10^6 years for Post-Jatulian are quite out of harmony with the denudative method. To-day the matter stands thus :—A number of results are available based upon the use of carefully selected material, and when the material is thus selected the ratio of lead to uranium—the " lead ratio " as it is termed—increases as we go downwards and diminishes as we go upwards in the strata, preserving a fair degree of agreement even for widely separated localities.

Those who would rest content with this result, however, can do so only by ignoring the very interesting and suggestive fact that when we base the results on the lead ratio of selected thorium minerals, we arrive at ages which are in substantial agreement with the results reached by the denudative method. On the face of it this agreement gives strong support to the conclusions reached by methods absolutely different in nature.

For long it was known that thorium minerals—such as thorite—gave persistently lower ages than uranium minerals. It became the custom with some to treat these ages as untrustworthy. But we know now that this attitude is not justified, but rather that the onus of explaining away the impressive agreement between the indications of thorium lead and denudative statistics rests with those who would reject the Age supported by both.

Soddy's determination of the atomic weight of the thorium lead isotope, in 1917, afforded material for an age determination on a very large scale, and from the nature of the research, one of special value. The material was a thorite from Ceylon ; from rocks immediately overlying the Charnockite series. The latter is extremely ancient—Lewisian or Lower Archæan. Upon reading in NATURE Prof. Soddy's account of his determination of the atomic weight of the lead derived from these rocks, I estimated that the quantity of lead extracted from the thorite gave an age of 130 millions of years for the time since this mineral had been generated ; and on communicating with Prof. Soddy I found that he had reached a somewhat similar conclusion.

At this time, however, there was the possibility that thorium lead was not altogether stable. Suspicion fell more especially on thallium as the final product. Two experimental results, however, laid this doubt to rest : experiments upon a thorianite made in my laboratory by J. R. Cotter failed to detect even spectroscopic traces of this element, and there was insufficient thallium found in the thorite dealt with by Prof. Soddy. In a subsequent letter to NATURE Prof. Soddy states that a research carried out at the Radium Institute of Vienna supported the view that the lead isotopes derived from thorium were both stable. I shall refer presently to yet additional evidence that the transformations of the thorium family cease with lead.

Writing to NATURE in support of the hypothesis then under discussion—*i.e.* that thorium lead was unstable—A. Holmes cited a result on a selected specimen of uraninite, showing that the rocks in which Soddy's thorite occurred were, according to the uranium-lead ratio, 512 millions of years old. Previous uranium-lead ratios had assigned a much greater age

to them. Here, then, the results join issue : the uranium result is just four times as great as the thorium. We notice, too, that on the uranium-scale of time this thorite must be older than Silurian or Ordovician, which have been determined by uranium lead as 430 millions of years ago. Probably its age dates back to Cambrian or even to pre-Cambrian time. From what we have already inferred we cannot regard 130 millions of years for early Palæozoic time as irreconcilable with the maxima which denudative methods afford. More recently, lead derived from a Norwegian thorite of Langesundfiord—also of lower Palæozoic age—seems to reveal an age of 150 millions of years. In this case, also, there is the added security of a determination of the atomic weight of the lead.

We cannot discredit these results on the score of radio-active instability of the lead. Why, then, set them aside in favour of results reached on uranium lead, which are in hopeless contradiction with the indications of the record of the surface activities of the globe ? It is, indeed, not too much to say that the whole position is now reversed and that to-day suspicion attaches to the uranium-lead ratio. And, as we shall see, there is much unknown about the earlier radio-active sequence in the uranium series ; while the discovery of isotopes opens the way to possibilities unthought of in the earlier days of radio-active science.

I shall, however, now turn to the evidence of the pleochroic halo on this matter.

The halo affords a means of investigating certain facts respecting the break-up of the radio-active elements in the remote past. For the dimensions of the halo—minute though they be—can be determined with considerable accuracy, and these dimensions are conditioned by the added effects of the several α-rays emitted by the transmuting elements. Bragg and Kleeman observed and measured just such integral ionisation effects in air. In the rocks the ionisation curves, owing to the great stopping power of minerals, are on a scale 2000 times as small. They are very faithful hieroglyphics, however, and carry back our knowledge over an appalling vista of time.

One single α-ray produces a well-known curve of ionisation determined by Geiger. The range of the ray does not affect the general nature of the curve. If we imagine uranium or thorium as parent elements contained in a minute crystal—of zircon, for instance—we must picture the various α-rays affecting the surrounding substance—mica, we may suppose—in such a way as to build up concentric spherical shells more or less overlapping and corresponding to the radial distances at which the ionisation of the several rays is at a maximum. As seen in section upon cleaved flakes of the mica, we find concentric coloured rings representing the ionisation due to the rays.

In order to arrive at the theoretical location of these rings we must add up the several ionisation effects as observed in air. This involves assigning a Geiger curve to each ray according to its range and adding up the ordinates.

Let us consider first the case of the thorium halo. Fig. 1 is a curve arrived at in the manner I have just described. Its ordinates are proportional to the

integral ionisation effects of those radio-active elements in the thorium series which emit α-rays. And above it I have marked, calculated into the range in air, the positions of the coloured rings which in biotite we observe encircling a minute mineral particle containing thorium and all the successive products of its transmutation. This, of course, necessitates magnifying the halo enormously—rather more than 2000 diameters. You perceive that the halo very faithfully conforms to the features of the air-curve. It may be of interest to mention that the finding of the third ring led to the discovery of the prominence on the curve which accounts for it. This part of the curve had originally been plotted from an insufficient number of
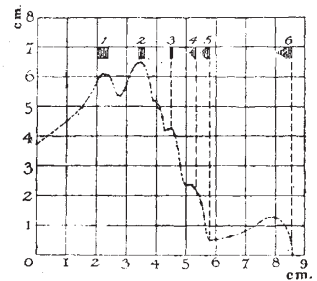

FIG. 1.

ordinates. This close agreement really reveals a very important fact. The air-curve depends for its dimensions on the ranges of the several α-rays as we measure them to-day in the laboratory. The halo-measurements refer to radio-active effects which began their record in this mica in Carboniferous times— possibly long before. The halo reveals no sign of change in the several ranges concerned. As you are aware, the rate of break up, the transformation constant of the element, is related to the range. We are, therefore, in the case of the thorium family, entitled to read in these minute and ancient records a guarantee that the accumulation of the final product—the thorium isotopes of lead—was in the remote past effected at just such a rate as we have inferred from the splendid researches of our day. The thorium halo gives us this guarantee. It also tells us that it is improbable that the resulting lead is unstable. For if it were we must find room for rays additional to those we have used in deriving the ionisation curve. True, a coincidence of range might enable a ray to lie concealed in the halo ; but the fit of the halo is so absolutely faithful to every feature of the curve that this seems improbable.

It is also possible to observe the successive stages of development in thorium haloes. The first rings to appear are those corresponding to the two conspicuous crests of the curve, Fig. 1. If the central nucleus is small or feeble, nothing more may be developed.

We now turn to the uranium curve. The eight contributory ionisation curves are placed according to the range of each ray, and Fig. 2 shows the curve produced by adding up the ordinates. Above it are laid out the several rings observed in the uranium halo.

Looking at these rings, we notice that the outer features of the halo seem in fair agreement with the present-day ranges. But the innermost ring has a larger radius than would be expected from the curve. Much care has been expended in verifying this point. In the Devonian mica of County Carlow these haloes are found in every stage of development according to the size or activity of the nucleus. The uranium halo

begins as a single delicate ring surrounding the minute central nucleus. It can be measured from a stage bordering on invisibility to a stage when its central area is beginning to darken up and the first shadowy signs of the outermost ring of all—that due solely to radium C—appear. A large number of readings on these embryonic haloes, made recently by various observ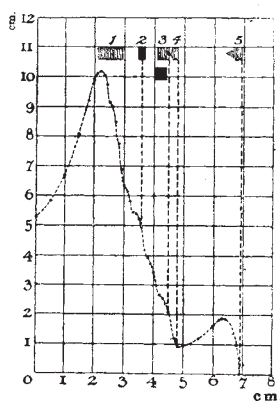ers, confirm the mean value of is radius as cited in a paper communicated to the Royal Society in 1916. The discrepancy with the theoretic curve is small; 10 or 12 per cent. of the external radius. The allowance for, and measurement of, the nucleus is sufficiently difficult to introduce some uncertainty.

FIG. 2.

This misfit may be of considerable significance. I have already reminded you that the range of the α-ray emitted by a transforming element is related to its rate of break-up. The range is longer for the shorter lived elements. Now here the first ring of the uranium halo in mica shows a longer range than we would expect from the air-curve as observed to-day. The agreement between the two in other cases appears to show that this is not due to any unknown effect influencing the retardation in mica. The location of the first uranium ring is mainly referable to those short-range α-rays arising from the initial transformations of the uranium series. We infer that one or more of these rays must have had a longer range in past times and, of course, that the corresponding transformation periods must have been shorter. A specially influential ray is that slowest of all the rays—that which is emitted in the break-up of uranium 1. The discrepancy might be due to this ray possessing a greater range in early geological times. But, whatever the cause, the nature of the misfit suggests evidently that formerly the rate of transformation of uranium to lead was faster than it is to-day.

It is with some reserve that I refer here to measurements made lately on haloes of comparatively recent and of very remote geological ages. I say " with reserve," for not only are the results of a nature calling for very adequate confirmation, but the measurements present considerable difficulty. The point at issue may be stated in a few words :—Is the abnormality observed in the dimensions of the uranium halo dependent in amount upon the antiquity of the rock in which the halo is developed ?

I had sought occasionally for uranium haloes in rocks younger than the Leinster granite—which is of early Devonian age. The granite of Mourne, which is of Eocene or early Tertiary age, for long refused to reveal any haloes suitable for measurement. However, recently, I was so fortunate as to find a few of these early halo rings which I was able to measure. Further search has revealed a few more ; but htey are excessively scarce and rather difficult to detect. The nuclei of these haloes are only rarely zircon—they seem to be apatite—possibly allanite—and their average size is greater than the zircon nuclei of the Carlow mica. Both the mineral nature of the Mourne nuclei and their dimensions involve, therefore, a bigger subtractive correction on the observed radius than is required in the case of the Carlow haloes. But in addition to this, there appears to be a small difference in the external radius of the Eocene halo and that of the Devonian halo. According to a large number of readings by several observers, some of whom were not acquainted with the question at issue, the external radius of the Eocene halo-ring—no allowance being made for the nuclear radius—is 0·0135 mm. The same observers obtained for the Devonian halo 0·0146 mm.—without allowance for the nucleus. The nuclear correction, as I have said, would have increased the discrepancy, but the correction is a difficult one. There is no reason to believe that *more* than 1 per cent. of this difference can be ascribed to the chemical composition or density of the micas, both of which have been investigated.

Still more recently I have found these primary ring-haloes in the micas of Arendal and Yitterby, which are said to be of Archæan age, and which are certainly extremely ancient. These haloes appear to possess a radial dimension of 0·0160 mms., or a little less. Here, again, the nature of the mica does not appear to be responsible. According to these measurements it would appear that the radius of the Eocene halo-ring must be increased by over 7 per cent. to attain the size of the Devonian halo-ring, and that this is, in radial dimension, about 10 per cent. smaller than the Archean. It would seem as if we might determine a geological chronology on the dimensions of these halo-rings !

The foregoing results, if confirmed, would give strong support to the view that some factor, variable over geological time, had affected the ranges and periods of certain elements concerned in building up the uranium halo. However, too much stress must not be placed on these measurements till they are confirmed by haloes in yet other micas. Pending further investigations, I return to the fact that the uranium halo of Devonian age does not conform to the ionisation curve of the uranium family as determined on present-day measurements. Serious discrepancy seems confined to the shorter ranges, more especially with that primary range which is most influential in determining the rate of production of uranium lead.

We do not appear to be in a position to deny the possibility that uranium 1 may have slowed down in its rate of decay over geological time. Such laboratory observations as can be extended to the case of short-lived elements would not, probably, shed any light on the matter. It is a possibility long ago suggested by Rutherford. But if this is the explanation we must admit that in the case of thorium any corresponding effect must have been much smaller. On the whole the former influence of one or more isotopes of uranium —which possibly may almost have disappeared— seems the more probable explanation. Hypothetical isotopes of uranium have been invoked by highly competent authorities to meet the difficulties affecting the ionisation accounts of the uranium family of elements. Boltwood suggests as " not impossible "

that what we now call uranium consists of three radio-elements ; a parent element and two isotopic products all emitting α-rays (*Phil. Mag.*, July 1920). In 1917, A. Riccard put forward the view that the parent of actinium is a third isotope of uranium not belonging to the uranium family and having an atomic weight of 240. This view is regarded favourably by Soddy and Cranston. It clears up the difficulty respecting the atomic weight of uranium and fits in with the atomic weights of radium and of uranium lead. Soddy and Cranston remark that in order to explain, in this case, the constant ratio of actinium to uranium observed in minerals we must suppose the period of uranium 1 and of the hypothetical isotope to be the same. This difficulty, however, is removed if we may assume that the ratio varied over geological time.

A somewhat similar theory to Riccard's may be invoked to explain the abnormality of the Devonian uranium halo. We have these facts to go on :—The age indicated by uranium for Lower or Pre-Palæozoic rocks is about 4 times too great as compared with the age indicated by thorium. We assume, therefore, that three-fourths of the lead as measured in uranium minerals is derived from a certain isotope. This isotope, not having been detected in our time by its primary α-radiation, we must suppose to be now sensibly exhausted. We, therefore, have a known mass of this isotope transforming to lead in a known time—130×10^6 years. Assuming that only 1 per cent. of it is left we get its transformation constant (3.5×10^{-8}), and by Geiger and Nutall's relation we find the corresponding range as 2·6 cms. at 0° C. ; or about 2·75 cms. at 15° C. To-day the α-radiation of the hypothetical body would be only $\frac{1}{1000}$ of that due to uranium 1, but during the period since the Devonian there will be about 3 α-rays from the short-lived isotope to 1 from the long-lived. The integral curve of ionisation as modified by these hypothetical results would be in agreement with the Devonian halo. We have to assume that the ranges of the rays emitted by the successive disintegrating products of the supposed isotope were such as to leave the outer features of the halo sensibly undisturbed. This seems not improbable.

The salient facts which appear in the study of radio-active haloes are :—firstly, that the agreement of our laboratory measurements of to-day with the features of the Palæozoic thorium halo is such as to support the view that the periods of the several elements concerned in its genesis have remained unchanged over 130 millions of years. This fact, taken along with the stability of thorium lead, seems to render its reading of geological time authentic in a high degree. Its indications are confirmed by the consistent testimony of the denudative processes which have progressed on the earth's surface. Secondly, it appears that the uranium halo is not in conformity with the period we ascribe to-day to uranium ; a disagreement which is emphasised by the failure of uranium-time to conform with the united testimony of thorium-time and denu-dative-time ; as well as by much that remains un-explained respecting the earlier changes in the uranium family of elements.

The complete tale is not yet told, but I think the balance of probability is in favour of an age between

150 and 200 millions of years for the earliest advent of geological conditions upon the globe.

Astronomical investigation on the subject of the age of the earth deals, generally, with that greater age which must be ascribed to the earth as a planet. For this age vast periods have been claimed. But it is possible to reconcile superior ages for the earth as a planet with comparatively brief geological time. And—to my mind—in doing so we proceed upon what is no more than a necessary deduction based on our knowledge of the radio-activity of terrestrial materials. I would go further—still, as I believe, logically,—and ascribe to radio-active energy an influence on planetary and stellar evolution much greater than has hitherto been admitted.

The only planet we can investigate at all closely is, of course, our earth. And what do we find ? In its surface materials there are sufficient of the radio-active elements, as Lord Rayleigh first showed, to account for the observed average temperature gradient if the surface conditions extend a little way, about 19 kilometres, inwards. It is, for many reasons, in the highest degree improbable that such a definitely defined radio-active layer exists. Nor is it probable that the earth's interior is free from radio-active sub-stances. We find both uranium and thorium in meteorites containing a large percentage of iron and nickel, and, although they have not as yet been found in meteoric iron, we know from the mean density of the earth that its interior cannot be composed of pure iron. It is probable that a considerable pro-portion (some 40 per cent.) of siliceous materials are intermingled : and when such exists in meteorites invariably we find the radio-active elements. By what conceivable activity was all the uranium and thorium separated out and brought to the surface ?

The view that radio-active elements exist in the earth's interior is sometimes met by a formal denial that the earth can be getting hotter within. Upon what evidence is this denial based ? If the central core of the earth for a radial distance of 2000 kilometres—say—had risen in temperature by 1000° C. over geological time—and upon a low assumption of the interior radio-activity it might reach this temperature in 150 million years—would we be aware of the fact ? Would the day be appreciably lengthened ? Would there be any effect at all if the outer parts were cooling due to loss of primal heat ? We have further to consider that only over the short period of historical time would any observations be available. The denial is quite baseless so far as my estimates go.

Well, then, if our earth is heating up within. is there not an impending termination to our geological age ? Kelvin showed how complete is the thermal isolation of the earth's interior, and it is certain that interior heat is not now escaping. The rise of temperature within must go on till the present epoch succumbs to the accumulated energy. Then must ensue a period of vulcanicity which will end life upon the globe, and probably reverse the chemical work stored up by ages of denudative and organic activity. The whole sequence of events — rapid cooling by radiation, restoration of the oceans and, possibly, re-birth of life and of its evolutionary history—would begin all over again. On this view the Age we have been studying

may be one of many and will inevitably attain its three score and ten ; terminating in labour and sorrow. But there must come a rejuvenation, and the re-juvenation, possibly, may one day be pondered by other Minds than ours. Remember that after some ten thousand millions of years there still survives 50 per cent. of the heat-generating elements, and the effect of their diminution is only to lengthen out the recurring geological ages. Our planetary companions may be in various stages of such cyclical changes.

Recovery of Hughes's Original Microphones and Other Instruments of Historic Interest.

By A. A. CAMPBELL SWINTON, F.R.S.

FROM the perusal of David Hughes's note-books recently bequeathed to the British Museum by Mrs. Hughes, and sent to me for examination by the Keeper of the Manuscripts (see NATURE, March 9, 1922, pp. 315-316), it became obvious to me that Hughes must at one time have possessed numerous original instru-ments, mostly constructed with his own hands. Having been informed 'that the note-books had been rescued from " an incredible accumulation of useless lumber," it occurred to me to try to locate this " lumber " if still existent, and to see whether it comprised any of the instruments. To make a long story short, a room in a furniture depository not far from the Tottenham Court Road was found to be filled with Hughes's personal effects, which had been stored there since his death in 1900, when Mrs. Hughes returned to America. Having interested Col. H. G. Lyons, F.R.S., Director of the Science Museum, in the matter, the effects were carefully examined, when not only were there discovered eight more note-books—one containing an illustrated account of Hughes's inven-tion of the microphone—but also numerous instruments.

These comprise a number of microphones, of which several are different-sized instruments of the well-known pivoted-lever type. Others consist of pointed carbon pencils, loosely held at their ends between fixed carbon sockets, the whole being mounted on sounding-boards, which in one case takes the form of an in-verted Japanese ash-tray. Several more consist of carbon pencils suspended pendulum-wise by paper strips, so as to bridge other carbon pencils mounted on vertical sounding-boards, while others, again, consist of glass tubes containing either carbon blocks held together by a light spiral spring, or carbon granules. Finally, among the microphones, there is the one consisting of three French nails that has served to illustrate many a text-book.

In addition, there is an induction balance, probably the first one that Hughes made, together with the actual instruments with which he practised wireless telegraphy in 1879. They are all readily identified from the illustrated descriptions in the note-books, and include the clockwork with which currents from a single-cell battery, connected to one of the coils of the induction balance, were interrupted so as to trans-mit wireless signals. There are also two Bell tele-phones, evidently made by Hughes himself, together with two more which he says were made for him by Sax, which he used for wireless reception in con-nection with a water-tight pocket battery, and a special microphone that seems to have acted as a self-decohering coherer. This latter is contained in a glass bottle, the loose contact being made between a steel needle and a wire loop, which latter Hughes says he made more sensitive by coating it with soot from the flame of a spirit lamp.

With these simple pieces of home-made apparatus Hughes not only prepared the way for the modern telephone transmitter, but also transmitted and received wireless signals over distances up to 300 yards no less than 43 years ago.

All these instruments have been made over to the Science Museum, South Kensington, by Mrs. Hughes's trustees, and are now on view in Room No. 10.

Obituary.

WE learn from the *British Medical Journal* that Dr. Harris Graham died at Beirut, Syria, on February 27. Dr. Graham, who was in his sixtieth year, was of Canadian birth and was educated at Toronto and Michigan Universities. Going to Turkey as a mission-ary he served four years at Aintab Medical College. On its closure, he was called to Beirut and joined the American University there in 1889. During various leaves of absence he worked in Berlin and Vienna, and advanced evidence that a Culex mosquito is the carrier of dengue fever. He had an extensive practice and spoke all the principal languages of the Levant. Dr. Graham will be much missed, for he was an energetic and inspiring teacher and a physician of great acumen.

THE *Chemiker Zeitung* of March 25 announces the death, at the age of eighty-four years, of Prof. A. Naumann. Prof. Naumann was one of the first workers in the field of what is now called physical chemistry. His researches on dissociation, thermo-chemistry, and mass action, and his text-book had great influence on the science ; his name is prominent in all the earlier work in this field.

THE death is announced, at the age of fifty-one years, of Dr. George Vincent Wendell, who had occupied a chair of physics at Columbia University since 1910. From 1892 to 1907 he held various posts at the Massa-chusetts Institute of Technology, and from 1907 to 1910 he was professor of physics and head of the department at the Stevens Institute of Technology, New Jersey.

WE deeply regret to record the death on April 9, at seventy-seven years of age, of Sir Patrick Manson, G.C.M.G., F.R.S., whose pioneer work on tropical diseases opened up fields of research of profound significance both to science and civilisation.