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THE AGE OF THE EARTH

BY

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INTRODUCTORY REVIEW

The birthday of the world known to us is commonly regarded as coinciding with the epoch of Leibnitz's *consistentior status*.¹ This great philosopher believed that the earth solidified from a state of fusion by stages similar to those he had observed in the refrigeration of large masses of metal: cavities analogous to blow-holes developed and burst; thin partial crusts formed and broke; in some cases such crusts were extensive enough and sufficiently cool to receive bodies of water, but were not strong enough to support the accumulated liquid and presently gave way, so that fresh convulsions ensued. At length, through the progress of refrigeration, the globe emerged into "a state of greater consistency." Then chaos was at an end and permanent oceans became possible.

It is interesting to note that Leibnitz seems to have had in mind not merely dry fusion, but fusion with the co-operation of water, or what would now be called aqueoigneous fusion.

Three methods have been devised for ascertaining the age of the ocean: one from the accumulation of stratified rocks; another from chemical denudation; and the third from the progress of refrigeration. Closely allied to these is Sir George Darwin's discussion of the lapse of time since the earth and the moon parted company.²

¹ Protogaea, 1749. A sketch of this posthumous work appeared in the *Acta Eruditorum*, Jan., 1693.

² *Phil. Trans.*, London, vol. 170, 1879, p. 511; and vol. 171, 1880, p. 882.

A faultless mode of arriving at the ocean's age is perhaps unattainable, though better ways than are now available may possibly be elaborated hereafter. All that can be hoped for at present is that diverse methods may lead to estimates of the same order of magnitude; and such a result is more important just now than ever before. By physical reasoning geologists have been compelled to adopt a moderate estimate of geological time. Of late, however, physicists engaged in studies of radioactivity have reached the conclusion that certain uranium minerals are of an enormous age, which is estimated by simple though tremendous extrapolation. If geologists can now give convincing reasons for adhering to ages within a hundred million years, or even within two hundred, they may partly repay the heavy debt due by them to Kelvin and his intellectual heirs.

So long ago as 1860 John Phillips,¹ the originator of the mechanical theory of slaty cleavage, estimated that the time required for the deposition of the stratified rocks lay between 38 and 96 million years. So far as my reading goes, this was the only reasonable estimate prior to Kelvin's truly epoch-making paper of 1862.² During the last 20 years much energy has been expended upon the study of the maximum thickness of strata and the time-rate of their deposition. In 1890 de Lapparent thus estimated a period of from 67 to 90 millions of years.³ The most careful investigation of this kind yet made is due to Mr. Charles D. Walcott who, in 1893, carried out minute and cautious studies of the rate of deposition and of the amount of accumulation, both mechanical and chemical.⁴ His estimate for the lapse of time since the base of the Cambrian is 27,640,000 years and he allows for the Algonkian a period of 17,500,000 years. As a matter of course this last figure rests on extremely imperfect data and should probably be increased. Mr. Walcott considered 70 millions the maximum age. In 1899 Sir Archibald Geikie stated that, so far as he was able to form an opinion, 100 million years would suffice for the formation of the stratified rocks,⁵ and in 1900 Mr. W. J. Sollas, assuming a constant rate of deposition, reached an age of 26½ million years.⁶ In 1909 Mr. Sollas increased this estimate to nearly 34 million years,⁷ but in view of erosions and the uncertainties involved he inclines to believe, as do many others, that the pre-Cambrian sediments took as much time for their deposition as those from the base of the Cambrian to the top of the column. Making also an allowance for gaps, he thinks the stratigraphical column, if complete, might indicate 80 million years. He draws especial

¹ "Life on the Earth," etc., 1860, p. 119.

² Trans. Roy. Soc. Edinburgh and Thomson and Tait, Nat. Phil., Pt. II, p. 468.

³ Bull. Soc. Géol. de France, vol. 18, 1890, p. 351.

⁴ Journal of Geol., vol. 1, 1893, p. 675.

⁵ Brit. Assoc. Adv. Sci., 1899, p. 727.

⁶ Brit. Assoc. Adv. Sci., 1900, p. 711.

⁷ Mr. Sollas assumes a constant rate of deposition of one foot per century and thinks most geologists would incline to a higher rate. Quart. Jour. Geol. Soc. London, vol. 65, 1900, p. CXII.

attention to the discrepancy between stratigraphical estimates and those from sodium accumulation in the ocean. If Mr. Walcott's estimate for time since the base of the Cambrian be doubled, in accordance with the opinions stated above, it gives 55 million years.

The introduction into modern geology of estimates founded on chemical denudation is due to T. Mellard Reade,¹ but Mr. J. Joly was the first to show that sodium is the only element which can be trusted to afford good estimates of the lapse of time involved in denudation. Adopting the hypothesis that the sodium content of the ocean is derived at a constant rate from that of the rocks, Mr. Joly, in 1899, arrived at an age of between 80 and 90 million years, and this he increased by 10 million in 1900.² In 1909 Mr. Sollas made a very searching inquiry into this subject, availing himself of much material published since Mr. Joly's papers, and, on the same hypothesis of uniformity, placed the age of the ocean between 80 million and 150 million years.³

Very curiously, the great astronomer Edmund Halley nearly 200 years ago devised a method of determining the age of the ocean from the amount of salt which it contains.⁴ He recognized that the means he proposed for determining the annual increment of salt were impracticable and, writing long before Lavoisier was born, could not have guessed that analysis of river waters would become a simple matter. It is not strange that Halley's paper was completely forgotten, but it is now of such interest and is so inaccessible to a majority of geologists that an extract from it will not be regarded as out of place here.

A short Account of the Cause of the Saltness of the Ocean, and of the several Lakes that emit no Rivers; with a Proposal, by help thereof, to discover the Age of the World.

There have been many attempts made and proposals offered, to ascertain from the appearances of nature, what may have been the antiquity of this globe of earth; on which, by the evidence of sacred writ, mankind has dwelt about 6000 years; or according to the Septuagint above 7000. . . . This inquiry seeming to me well to deserve consideration, and worthy the thoughts of the Royal Society, I shall take leave to propose an expedient for determining the age of the world by a medium, as I take it, wholly new, and which in my opinion seems to promise success, though the event can not be judged of till after a long period of time; submitting the same to their better judgment. What suggested this notion was an observation I had made, that all the lakes in the world, properly so called, are found to be salt, some more some less than the ocean sea, which in the present case may also be esteemed a lake; since by that term I mean such standing waters as perpetually receive rivers running into them, and have no exit or evacuation. . . .

¹ Proc. Liverpool Geol. Soc., vol. 3, 1876, p. 211.

² Trans. Roy. Soc. Dublin, vol. 7, 1899, p. 23; and Brit. Assoc. Adv. Sci., 1900, p. 369.

³ Quart. Jour. Geol. Soc. London, vol. 65, 1909, p. CXII.

⁴ Phil. Trans., vol. 29, 1715, p. 296. See also Science, vol. 31, 1910, p. 459.

Now I conceive that as all these lakes do receive rivers and have no exit or discharge, so it will be necessary that their waters rise and cover the land, until such time as their surfaces are sufficiently extended, so as to exhale in vapor that water that is poured in by the rivers; and consequently that lakes must be bigger or lesser, according to the quantity of the fresh they receive. But the vapors thus exhaled are perfectly fresh; so that the saline particles that are brought in by the rivers remain behind, while the fresh evaporates; and hence it is evident that the salt in the lakes will be continually augmented, and the water grow salter and salter.

Now if this be the true reason of the saltness of these lakes, it is not improbable but that the ocean itself is become salt from the same cause, and we are thereby furnished with an argument for estimating the duration of all things, from an observation of the increment of saltness in their waters. For if it be observed what quantity of salt is at present contained in a certain weight of the water of the Caspian Sea, for example, taken at a certain place, in the driest weather; and after some centuries of years the same weight of water, taken in the same place and under the same circumstances, be found to contain a sensibly greater quantity of salt than at the time of the first experiment, we may by the rule of proportion, take an estimate of the whole time wherein the water would acquire the degree of saltness we at present find in it.

And this argument would be the more conclusive, if by a like experiment a similar increase in the saltness of the ocean should be observed: for that, after the same manner as aforesaid, receives innumerable rivers, all which deposit their saline particles therein; and are again supplied, as I have elsewhere shewn, by the vapors of the ocean, which rise therefrom in atoms of pure water, without the least admixture of salt. But the rivers in their long passage over the earth do imbibe some of the saline particles thereof, though in so small a quantity as not to be perceived, unless in these their depositories after a long tract of time. And if upon repeating the experiment, after another equal number of ages, it shall be found that the saltness is further increased with the same increment as before, then what is now proposed as hypothetical would appear little less than demonstrative. But since this argument can be of no use to ourselves it requiring very great intervals of time to come to our conclusion, it were to be wished that the ancient Greek and Latin authors had delivered down to us the degree of the saltness of the sea, as it was about 2000 years ago; for then it can not be doubted but that the difference between what is now found and what then was, would become very sensible. I recommend it therefore to the Society, as opportunity shall offer, to procure the experiments to be made of the present degree of saltness of the ocean, and of as many of these lakes as can be come at, that they may stand upon record for the benefit of future ages.

If it be objected that the water of the ocean, and perhaps of some of these lakes, might at the first beginning of things, in some measure contain salt, so as to disturb the proportionality of the increase of saltness in them, I will not dispute it: but shall observe that such a supposition would by so much contract the age of the world, within the date to be derived from the foregoing argument, which is chiefly intended to refute the ancient notion, some have of late entertained, of the eternity of all things; though perhaps by it the world may be found much older than many have hitherto imagined.

Ever since the publication of Mr. Joly's papers Mr. F. W. Clarke and I have been deeply interested in his method and in chemical denudation. A

portion of the large mass of data accumulated during the past decade has been determined at our suggestion, though most of the hydrological work of the U. S. Geological Survey, including the very numerous analyses executed under the charge of Mr. R. B. Dole, has formed part of a systematic effort to develop the mineral resources of the Commonwealth. In pursuance of our common aim Mr. Clarke has now completed a review of all available data, not only for the United States but for the world, and his paper is published by the Smithsonian Institution under the title, "A Preliminary Study of Chemical Denudation."¹ His results form the basis of a discussion of the age of the ocean which will be presented below.

Kelvin in 1862 first discussed the age of the earth considered as a cooling body. His results were for some years received with sorrow and indignation by most geologists and especially by evolutionists, who then desired unlimited time in which to effect the development of species. More mature study has convinced the scientific world that there is no necessary discrepancy between Kelvin's 20 to 400 million years, with a probable 98 million, and the conclusions of geologists or paleontologists. In 1893 Clarence King, with the aid of Mr. Carl Barus, introduced the important criterion of tidal stability and reached the conclusion that 24 million years best represented the conditions.² This result was accepted by Kelvin in 1897³ and he then placed the limits at 20 and 40 million years. The earths considered had uniform initial temperatures. In 1908 I showed that such a distribution of temperature necessarily involved a prolonged period of tidal instability and discussed a globe the initial temperature of the outer shell of which increased in simple proportion to the distance from the surface.⁴

Only Sir George H. Darwin has discussed the age of the earth from a purely astronomical point of view. From his theory of the earth-moon system he derived an estimate of more than 56 million years which for a long time stood rather alone between groups of higher and lower figures.

In the succeeding pages will be found my discussion of Mr. Clarke's data and also a revised edition of my paper on the age of the earth as determined from refrigeration. The two arguments accord remarkably well.

¹ Smithsonian Miscellaneous Collections, vol. 56, No. 5, 1910.

² Amer. Jour. Sci., vol. 45, 1893, p. 1.

³ Trans. Victoria Inst., vol. 31, 1899, p. 11.

⁴ Science, vol. 27, 1908, p. 227.

AGE FROM SODIUM ACCUMULATION

THE DATA

For comparison with the results of radioactive studies age determinations from chemical denudation have an advantage over those from refrigeration, because decomposition of the rocks is only remotely affected by thermal action and radioactivity. Mr. Joly's method of finding the age of the ocean consists substantially in dividing the total sodium content of the ocean by the amount of sodium annually poured into it by the rivers. The result is corrected for wind-borne sodium and marine denudation. I cannot agree that this quotient is the required age, but before discussing that matter it is essential to ascertain as nearly as possible the numerator and the denominator of Mr. Joly's ratio.

Mr. Clarke's new discussion has already been referred to and he has been kind enough to furnish me with the following data. The total sodium content of the ocean is $14,130 \times 10^{12}$ metric tons. The annual uncorrected sodium increment escaping from the rivers is 175×10^6 tons. The river-borne unchloridized sodium is 62.57×10^6 tons. The amount of average igneous rock needed to supply the total sodium of the ocean is 48.225×10^6 cubic miles, while the amount of rock necessary to supply both this and the sodium retained as silicates (mostly feldspars) in the sedimentaries would occupy 77.570×10^6 cubic miles. This would make a shell 2,050 feet thick enveloping the globe. These figures I shall accept.

THE SODIUM INCREMENT.

To the order of accuracy required for the purposes of this paper there is little difficulty in finding a correction for wind-borne sodium. The normal distribution of chlorine throughout New England and New York has been studied in considerable detail and the results are stated in Mr. Daniel D. Jackson's paper on this subject.¹ Nearly all of the normal chlorine of this region is due to wind-borne sea-salt and I find by examination of typical cross-sections of the chlorine charts that the total chlorine is fairly represented by supposing the coastal value of six parts per million to be constant for 20 miles inland and there to vanish.² The waters examined for normal chlorine were by preference surface waters, and since the region has a rainfall of 45 inches with no excessive evaporation, it seemed probable that if the rain from appropriate rain gauges had been bottled periodically, the collected precipitation would show substantially

¹ Jackson, D. D., The normal distribution of chlorine in the natural waters of New York and New England: Water-Supply Paper U. S. Geol. Survey, No. 144, 1905.

² The means employed was to plot a typical cross-section, adapt to the observed points a logarithmic curve, and take its area from the shore to a distance theoretically infinite. The section selected as representative passed through Rye, N. H., and bore N. 65 W. The method is capable of much elaboration.

the normal chlorine. Inquiry of Mrs. Ellen S. Richards as to the chlorine of the rainfall in Massachusetts confirmed this view. The assumed relation leads at once to the amount of wind-borne sodium along a length of coast of about 500 miles; and supposing the total length of the coast of the United States (without counting indentations) to be 4,500 miles, the order of magnitude of the correction required is 6 per cent. Mr. Clarke from different data and by a different course of reasoning estimates this correction at 7 per cent.

Applying a 6 per cent. correction reduces the annual river-borne sodium of the world an eighty-six millionth part of the oceanic sodium, or to 164.5×10^6 tons, of which more than half is combined with chlorine.

The origin of oceanic chlorine has long been regarded as mysterious. Average rock contains less than 2 per cent. of the chlorine needed to convert the accompanying sodium into chloride, and this has led to hypotheses of a primitive atmosphere heavily charged with chlorine or to an original solid surface containing great quantities of ferrous chloride. It may still be needful to seek such explanations, yet they seem to me highly unsatisfactory. From the base of the Cambrian onward some animal forms, such as *Lingula*, have persisted practically without change, and there is every paleontological indication that Paleozoic sea-water was a medium substantially like that of to-day. Had the sea been overcharged with chlorine in any form, consequences of this fact would almost certainly have manifested themselves in the composition of sediments and the decomposition of rocks, but nothing known to me indicates any unfamiliar chemical conditions. In the absence of such proofs it seems unreasonable to assume a Cambrian ocean essentially different from that of to-day. Now the ordinary geological estimate of the age of Cambrian strata is something like 30 million years; and during that period some 30 million times 63 million tons of unchloridized sodium, or perhaps considerably more than this, has been poured into the sea. All of this sodium has found chlorine to saturate it, once and a half its own weight of chlorine. Where can all this have come from?

We all know definitely that volcanoes and solfataras emit free chlorine, chlorhydric acid, ammonium chloride, calcium chloride and sodium chloride. Hot springs and certain cold springs also emit some of these, especially calcium chloride. So abundant is chlorhydric acid in the emanations of Vesuvius that according to Mr. Suess¹ vegetation has been blasted over large areas by acid rain, and according to Mr. Stoklasa² the greater part of the "pine-tree" clouds above the crater of Vesuvius during eruptions consists of chlorides, mainly ammonium chloride. Other volcanoes send out similar emanations and since there are hundreds of

¹ Ueber heisse Quellen: Verhandl. Gesell. deutsch. Naturf. und Aerzte, 1902, p. 113.

² Ber. Deutsch. chem. Gesell., vol. 39, 1906, p. 3530.

volcanoes and thousands of solfataras the total output must be very large. Cold chloride springs are not uncommon and are widely distributed at points far distant from volcanic centers.

Thus the question arises whether it is absurd to suppose that the chlorine product of volcanoes and solfataras is sufficient to saturate the unchloridized river sodium. It may be fanciful to subject such an hypothesis to computation, but this seems to me the only way to judge whether or not an absurdity is involved. Suppose then that all the chlorine were emitted as chlorhydric acid. The unchloridized river sodium being taken at 62.57×10^6 metric tons each year would require very nearly 100×10^6 tons chlorhydric acid. If this amount of gas were to issue from a single vent, at a temperature of 200° and a corresponding pressure of 6.62 kilograms per square centimeter into atmosphere at 0° , the opening would emit 29×10^6 metric tons per square meter per annum.¹ Thus the area of the vent required would be $100/29$ or less than $3\frac{1}{2}$ square meters, so that (neglecting friction) 1000 vents each of $6\frac{1}{2}$ cm. diameter working continuously would emit it all.² Considering the many violent eruptions of volcanoes and the large number of solfataras, a chlorhydric acid production of 100 million tons does not seem at all impossible. If the entire amount were uniformly distributed over the globe and reached the surface in a rainfall of one meter, this precipitation would contain less than a fifth of a millionth part of chlorine. Such an amount of chlorine would be recorded by hydrologists as "a trace."

Since the only known sources of chlorine are volcanoes and juvenile waters and these sources of supply are not manifestly insufficient, I consider it fair to conclude that the juvenile chlorine is and has been since the Cambrian about equivalent to the sodium set free from the rocks. Doubtless there have been variations in the relations between these two elements which were considerable from the point of view of the hydrologist, but they were, apparently, not great enough to unfit the ocean for the abode of a salt water fauna,³ or to characterize its geochemical behavior.

What the conditions were which prevailed before the Cambrian is naturally more uncertain, but I do not believe that a primitive acid ocean should be invoked until probable explanations involving juvenile chlorine have been exhausted. If the earth is an aggregate of meteorites, many of them siderolites, charged, as Mr. Clarke has suggested, with lawrencite, it seems to me natural to suppose that most of this chloride, whose density is nearly 3, would be deeply buried beneath the lighter

¹ Cf. Ch. Briot, *Théorie Méc. de la Chaleur*, 2d ed., 1883, Section 113.

² The present pig-iron production is about 60 million long tons and the weight of air which must be blown into the blast furnaces producing this amount of pig is about 320 million long tons.

³ Possibly fluctuations in the composition of sea-water may have affected the rate of evolution of organisms or limited their survival.

siliceous rocks. Its gradual decomposition might yield chlorine, and no other equally probable origin for the chlorine of volcanoes or deep springs is known.

Assuming that the supply of juvenile chlorine not combined with sodium is sufficient to maintain the present ratio of chlorine to bases in the ocean, it is possible to account for a part of the river-borne sodium chloride. Of the volcanic chlorine substantially $3/4$ must fall into the sea and if this were exactly equivalent to the unchloridized river sodium, the volcanic chlorine precipitated on the continental areas would suffice to chloridize one-third as much sodium as escapes chloridation until it reaches the ocean. Furthermore, in the present ocean there is about one and a seventh times as much chlorine as is needed to combine with oceanic sodium. On the hypothesis suggested this should also be the case on land. Thus if the unchloridized river sodium is 62.57×10^6 tons, the volcanic chlorine should be equivalent to the following amount of sodium:

$$\frac{8}{7} \times \frac{4}{3} \times 62.57 \times 10^6 = 95 \times 10^6 \text{ tons}$$

The chlorine equivalent of this sodium equally distributed over the earth's surface and dissolved in a rainfall of R meters would give a normal chlorine content

$$C = \frac{0.2864}{10^6 R}$$

so that if the rainfall were one meter, the normal chlorine content at inland stations would be less than $3/10$ of one part per million. It is to be desired that analyses of the rainfall should be made at a well distributed set of stations for many purposes, one of them being to ascertain what is the normal chlorine of the rainfall. This, however, may vary from year to year.

The total river-borne sodium less 6 per cent. for cyclic sodium is 164.5×10^6 tons. About 2 per cent. of this amount is covered by the chlorine of the rocks and, according to the hypothesis of volcanic chlorine set forth above, 95×10^6 tons more is thus accounted for. The sum of the two items amounts to over 98×10^6 tons, but leaves no less than 66×10^6 tons of river-borne sodium, or 40 per cent. of the total, which is chloridized but the chlorine of which has not yet been traced.

There is no doubt that a part of this sodium chloride is juvenile and that another portion is what may be called *secular*. Sediments at their first emergence from the ocean are saturated with salt water and any porous rocks, particularly sandstones, if temporarily submerged are recharged with salt water. Well compacted sands contain approximately 20 per cent. of interstitial space, and it is easy to compute that the whole mass of sandstones must originally have contained highly important

quantities of sodium chloride. On the other hand, a large part of the strata have been exposed to the leaching of vadose waters for so many million years that it is difficult to imagine how they can still retain any secular salt. The subject of juvenile sodium chloride is perhaps even more obscure. That many volcanic springs and many springs originating far below the stratified rocks bring ready-made sodium chloride to the surface is certain. Even where there are now no springs of this description, there may have been such at comparatively recent geological dates.

That sedimentary rocks do actually carry large quantities of disseminated salt seems to me fully established by the recent work of the hydrologists, in particular that of Mr. Dole. They have left no doubt that waters flowing through stratified rocks carry far more chlorine than those from massive drainage basins, and it is beyond question that this excess of chlorine is combined with alkalis. I have only very lately learned to appreciate the facts, and have previously erred in supposing the disseminated salt of the sedimentaries unimportant.

There seems to me the best reason to suppose that the origin of disseminated salt in strata is open to investigation. Careful chemical work on sufficiently numerous specimens should show whether the compounds associated with the salt are those to be expected from sea-water or those characteristic of deep springs, and at least a rough estimate of the proportion of secular sea-salt should be attainable.

At present I see but one way in which to form any opinion as to the division of the questionable 66×10^6 tons of sodium into secular and juvenile portions. Mr. Clarke has shown that a shell of average igneous rock enveloping the globe and 2050 feet thick would yield all the sodium of the ocean and the unchloridized sodium of the sedimentaries, and this result is confirmed by computation from the magnesium contents. If the continental area has always been as large as now, Clarke's shell would make a continental layer four times as thick, or 8200 feet. Thus if there were no juvenile sodium the total denudation of massive rocks, since the waters under the heaven were first gathered together unto one place and dry land appeared, would slightly exceed a mile and a half. Considering the vast well-demonstrated erosions, this figure is by no means startling. If the whole 66×10^6 tons were juvenile, the layer would reduce to 60 per cent. of 8200 feet, or to 9/10 of a mile, a depth considerably smaller than that of the deepest part of the Grand Cañon of the Colorado. As a field geologist I should unhesitatingly reject any train of reasoning however plausible which led to so low an estimate as this of the total denudation. Some juvenile sodium there certainly is, but it seems to me that this must form but a small part of the 66×10^6 tons.

To be on the safe side I shall estimate the age of the ocean on two

hypotheses each of which seems extreme, first that all of the questionable amount of sodium is secular and second that two-thirds of it are juvenile.

Before proceeding to these estimates, however, it is necessary to consider other uncertainties. At the present day the continent of North America stands above its average level, and this state of things appears to prevail in the other continents as well. The effect is to increase the potential of the streams and to hasten erosion. Again, the last glacial period is recent; and the ice has laid bare great areas of feldspathic rocks, which are thus more open to weathering than they were during the Tertiary. For this reason the yield of sodium per unit area must be above the mean for drainage basins including glaciated areas. Corrections for these conditions would tend to increase estimates of the earth's age. Finally, marine erosion needs consideration. The abrasive effect of the waves and tides on rock fragments and pebbles between high and low water marks is intense and world-wide, but the average width of beach is very small, much smaller than it would be were the continents to subside even a couple of hundred feet, and smaller than the mean width for the ocean's history. The sodium extracted by the waves is not represented in the figures for the rivers and a correction for marine erosion would tend to decrease estimates of the earth's age, or to offset the corrections for glaciation and high level. It is to be hoped that means may be found of evaluating the algebraic sum of these corrections, but I know of none; I cannot even guess its sign and must assume it to be zero.

Supposing none of the river-borne sodium to be juvenile the annual sodium increment is 98×10^6 tons and the ratio of the total oceanic sodium to this increment is 144.2×10^6 . This number of years would be the corresponding age of the ocean if Mr. Joly's method of estimation were adopted. Strictly speaking, the age should be somewhat greater, because the whole of the secular sodium still remaining in the rocks ought to be credited to the total oceanic sodium; but it is difficult to imagine that this could have any substantial influence on so huge a total as 14×10^{15} tons. On the second hypothesis, that 44×10^6 tons of sodium are juvenile, the increment would be 142×10^6 tons and the ratio 99.5×10^6 provided that no allowance is made for juvenile additions to the oceanic sodium by submarine springs. Any such additions would be equivalent to primitive salinity of the ocean and imply a further reduction in age.

RESULTS OF ASYMPTOTIC ACCUMULATION

To find the age of the ocean from the total oceanic sodium and its annual increment, it is manifestly indispensable to know or to assume a definite time relation between them. Mr. Joly's hypothesis is that the total sodium of the ocean is the annual increment of sodium multiplied by the age of the ocean in years; but another relation not less simple

seems to me much more probable and better fitted to give a first approximation to the truth.¹

Oceanic sodium is at least chiefly derived from lime-soda feldspars which as essential constituents are practically confined to Archean and later igneous rocks. The original surface of the earth must have consisted of such rocks to the exclusion of all others, while at the present day the greater part of the land area is covered with sedimentaries. Now the rate of decomposition of rocks is chiefly dependent on exposure. Even in areas of ancient feldspathic massives decomposition does not seem to penetrate to great depths. Thus in the southern Appalachians great areas of gneiss and allied rocks are now covered with a blanket of saprolite (rotten rock in place) which is in many localities 50 feet in thickness, but at all the points where I have observed it, less than 100 feet thick. Immediately below the saprolite blanket there is incipient decomposition and the feldspars are milky, but not many yards lower down the feldspars are characteristically translucent and the rock bluish in tint. A layer of decomposition products 100 feet thick seems to arrest decay. Corresponding statements are true of tertiary volcanics, excepting where the decomposition is solfataric. On the other hand mesozoic and paleozoic massive rocks deeply buried under sediments are in many cases found to be very free from decomposition. In short, buried massives decompose at a rate which is scarcely sensible.

It is quite imaginable that in the far distant future all the massive rocks might be thoroughly decomposed down to sea level or a trifle below. The continents would then be exclusively detrital. Under such conditions there could be no further important additions to the sodium content of the ocean, for there would then be no leaching; while mere diffusion to any considerable distance is too inordinately slow to produce any noteworthy result even in millions of years.

Thus in the distant past there must have been a time when a far greater mass of massive rock was decomposed each year than now decays in the same period; and a limit to this process can also be foreseen. The total area of exposed massives has surely diminished and will continue to diminish. Climate and temperature may perhaps have been in the past much what they are to-day; the rate of chemical denudation per unit area may not have changed considerably, but the most rigid uniformitarian would not maintain that the total area of exposed massive rocks has been constant. The inference seems unavoidable that sodium accumulation is an asymptotic process which progressed more rapidly (though possibly not with greater intensity) in the distant past and will come substantially to an end when a certain very finite layer of surface material has been ex-

¹ Vide *Science*, vol. 31, 1910, p. 509.

hausted. It seems worth while to attempt some estimates based on this conception of the saltiness of the ocean.

There is a great deal of evidence for the elder Dana's generalization as to the permanence of continental areas, and it is accepted by most physical geologists. Assuming its truth it should be possible to arrive at a mean value for the exposed land surface throughout geological time, and this would be a constant of the same order of magnitude as the present continental area.

The simplest law compatible with the conditions set forth is that, at any given time, the decrease per unit of time in the area of the sodium-producing exposures has been simply proportional to the temporary area of the exposures. This is equivalent to the hypothesis that the area of the feldspathic rocks can be represented approximately by the descending exponential; for if y is the exposed area at a given time, t , and c a constant, the decrease is represented by

$$dy/dt = -y/c, \text{ whence } y = Ae^{-t/c}$$

Here A is the extent of the exposure when $t = 0$, or when erosion is supposed to begin. On the strength of Dana's law A may be taken as the mean land area of the globe.

Suppose the total sodium content of the ocean at time t to be N , and let m be the annual yield of sodium per unit area, so that my is the increment of N in one year from time t . Then, m being constant,

$$N = \int_0^t my dt = Amc (1 - y/A)$$

or

$$c = \frac{N/my}{A/y - 1}$$

while the formula for the age of the ocean is

$$t = c \log_e \frac{A}{y}$$

This hypothesis takes no account of a primitive saline ocean, though that condition could be included by merely adding another constant. When t becomes infinite, y reduces to zero and, therefore, the limiting value of N is Amc .

The selection of the exponential to represent the phenomena under discussion is neither a random one nor dictated by mere convenience. This function is well known to play a leading part in the theory of those natural operations which may be classed generically as processes of absorption or gradual extinction,¹ just as it also expresses the gradual accumulation of money at compound interest. The descending exponential ex-

¹ Cf. Cournot, *Théorie des fonctions et du calcul infinitésimal*, vol. 1, chap. 2.

presses the decay of velocity of a body moving in a resisting medium; or the gradual extinction of light in an absorbing medium; the loss of electricity or of heat by radiation from a conductor; the decay of radioactivity, and an endless number of similar phenomena. Whenever the loss of an entity during any instant is simply proportional to the amount of the entity then present, the process must be expressed by the exponential.

It does not follow that the decomposition of feldspathic rocks is accurately represented by a single exponential term. Were the whole history of these rocks known it might very probably appear that the process is complex and should be expressed in some such manner as

$$y = Ae^{-t/c} + A_1e^{-t/c_1} + \dots$$

one term expressing, for example, the decay of albite; another that of anorthite, etc.

There is no close analogy between the asymptotic diminution of the area of those feldspathic rocks which are subject to erosion and the process of refrigeration of a globe or that of the diffusion of solutions, although these also are asymptotic. In the diffusion of solutions the molecular flow across a plane during an instant of time is proportional to the difference of concentration of the two thin layers divided by the plane. If one of them is devoid of solute, the process commences at immense speed; but it soon becomes very slow because the layers tend to acquire the same concentration.¹ Beneath areas in which there is no erosion, such as the ocean floor or lake bottoms, decomposition by diffusion is probably in progress; but the amount of sodium thus contributed to the rivers or the sea must be negligible in comparison with that yielded by denudation.²

At present it appears useless to speculate on sodium accumulation as represented by a series of exponential terms, nor can I think it needful. Precisely as a cubic mile of granite may be regarded as isotropic because the mass is so large compared with that of the single æolotropic grains of which it is composed, so local inequalities of chemical denudation disappear in the average of innumerable diversities. There must be some descending exponential which will fairly represent the process and the problem reduces to making a judicious selection of constants.

The earlier portion of this paper is devoted to a discussion of the value of N/my , or the ratio expressing the earth's age on Mr. Joly's hypothesis, and limiting values have been found for it. To compute the age on the exponential hypothesis it only remains to find a value of y/A for the present time. From Mr. Charles Schuchert's valuable memoir on paleo-

¹ For examples of the extreme slowness of diffusion see Amer. Jour. Sci., vol. 3, 1897, p. 27. For definition of the process, see Everett's c. g. s. system of units, 1902, p. 151.

² Although the hyperbola is an asymptotic curve, its equation would not express the course of the extraction of sodium from the outer shell of massive rocks, for that would imply that this shell would in time yield an infinite quantity of the metal.

geography¹ it appears that the mean area of the continent of North America has been about 8/10 of its present area. While the paleogeography of no other continent seems to have been so satisfactorily treated, it is generally acknowledged that the continents have grown more stable and the ocean deeper. So far as existing continents are concerned, therefore, it is fair to conclude that the mean land exposure has been somewhat less than the actual land area. Many paleontologists, however, think themselves driven by zoölogical reasoning to believe in the former existence of extensive continents bridging the great oceanic basins of the present day. This seems to me mechanically improbable and some great authorities such as Sir Archibald Geikie² refuse to concede the point. If the existence of such sunken continents of unknown extent were to be assumed, there would be no such thing as approximating to a mean land area. All I can do, therefore, is to point out that the effect of such bodies of land would be greatly to reduce the appropriate estimate of the earth's age by the method under discussion.

Assuming the permanence of continental areas, the uncertainty of the mean area is partially offset by marine denudation; for when shallow seas invaded the continents, breakers and the tidal wave must have triturated far more rock than is ground up on the shores of present continents.

A careful study of the areas of exposure of the principal geological formations was made by the late distinguished physical geographer, Lieutenant-General Alexis von Tillo. This includes the Archean and the younger eruptives, the results being expressed in hundredths of the total surveyed area. The following is an extract from von Tillo's table:³

<i>Continent.</i>	<i>Archean.</i>	<i>Eruptives.</i>	<i>Total.</i>
Europe	20.6	1.3	21.9
Asia	17.7	4.7	22.4
Africa	18.4	2.2	20.6
Oceanica	20.0	4.8	24.8
North America	27.2	5.5	32.7
South America	18.7	4.6	23.3
Mean	20.3	4.0	24.3

The most recent geological map of North America (compiled by Mr. Bailey Willis) shows that the relative area of exposed feldspathic rocks on this continent is not so large as was supposed when von Tillo wrote, and, though I have made no minute measurements, this exposure as now mapped seems not to exceed 25 per cent. With this emendation, von Tillo's table shows a truly remarkable uniformity throughout the world, all the figures lying between a fourth and fifth of the total area, the average

¹ Bull. Geol. Soc. Am., vol. 20, 1910, p. 427.

² Text-book of Geology, 4th ed., 1903, p. 1365.

³ Comptes Rendus, Paris, vol. 114, 1892, pp. 246, 967.

being 23 per cent. He too was impressed by the smallness of the variation in the relative areas of Archean exposures.

If the present continental area were the mean land area von Tillo's data would give at once $y/A = 0.23$, but since the land now stands at a high level the true value of A is smaller than the actual area and the quantity 0.23 must be modified accordingly.

The greater part of the area of massive rocks lies at a considerable distance from the sea, so that a partial submergence would diminish the continental area much more in proportion than the exposure of feldspathic rocks. In order to include, if possible, the true mean value of the land area, I shall take y/A first at three-tenths and again at one-fourth; the latter exceeding the 0.23 derived from von Tillo's data by a proportion relatively trifling while, closely enough, $0.3 = 0.23/0.8$, the denominator representing the mean taken from Mr. Schuchert's memoir.

With these data the results are as follows:

y/A	0.30	0.30	0.25	0.25
$N 10^{-6}/my$	144.2	99.5	144.2	99.5
$c/10^6$	61.80	42.64	48.07	33.17
Amc	$10N/7$	$10N/7$	$4N/3$	$4N/3$
$t/10^6$	74.4	51.3	66.6	46.0

Here the second line gives the ages on Mr. Joly's hypothesis in millions of years, and the last that computed from the logarithmic relation. The fourth line gives the ultimate amount of oceanic sodium calculable from the equations, and inferentially the thickness which Clarke's shell may reach, $10/7$ or $4/3$ of its present value. With the larger fraction this shell, now measuring 2050 feet, would ultimately attain a thickness of 2930 feet, or if it were all piled onto our continents, 2.2 miles. The difference between this estimate of the ultimate size of Clarke's shell and its present dimensions represents a volume $2\frac{1}{2}$ times as great as that of the present continents above sea level. Such, then, on this rather extreme assumption, would be the volume of massive rock, still fresh, but doomed to eventual decomposition. Thus the hypothesis leaves an ample margin for future igneous effusions and epeirogenic uplifts without putting an excessive strain upon the imagination.

Of the ages computed, the greatest, 74 million years, is the most valuable. It is indisputable that the process of rock decomposition is an asymptotic one and at least roughly represented by the simple exponential, while every effort has been made to select for the first computation the highest reasonable values for N/my and y/A . All the estimates would be greatly reduced if a primitive salinity of the sea or the former existence of land areas in the great oceanic basins were assumed. Those who do not feel obliged to accept either of these hypotheses may perhaps agree in my

opinion that the age indicated is between 70 and 50 million, probably closer to the upper limit than to the lower one.

The extreme values of Mr. Joly's ratio, N/my , which I am led to adopt, do not differ greatly from those preferred by Mr. Sollas, viz., 80 million and 150 million, while the ages computed from the logarithmic expression fall in very well with Mr. Sollas's results from stratigraphic reasoning. Again, if Mr. Walcott's estimate is amended by making the age of the earth twice as great as the lapse of time since the beginning of the Cambrian, the resulting 55 million years accord with the result here reached. Stratigraphy and sodium denudation as here discussed thus give results which are at any rate of the same order of magnitude.

AGE FROM REFRIGERATION

HISTORICAL NOTE

Kelvin's famous and epoch-making paper on the secular cooling of the earth was published in 1862.¹ His problem was to find the time which would elapse before a globe completely solid from center to surface and having throughout a certain uniform initial temperature would cool so far as to reduce the surface gradient of temperature to any given value. He assumed an initial temperature of 3900° C., a diffusivity of 0.01178 in c. g. s. units and a final surface gradient of 1° C. in 27.76 m. or 1° F. in 50.6 feet. These data discussed by one of Fourier's theorems give for the age of the earth 98×10^6 years. Kelvin, however, expressly directed attention to the fact that the effect of temperature in modifying diffusivities is almost unknown, and that the original distribution of temperature is uncertain. He also referred to the great differences in the surface gradient of temperature, which varies with the locality, as he stated, from 1° F. in 15 feet to 1° F. in 110 feet. He, therefore, allowed very wide limits in his estimate and placed the age between 20 million and 400 million years.

In 1893 Clarence King made a very important contribution to the subject² by introducing the criterion of tidal stability. Mr. Barus determined for him the melting point of diabase in terms of depth. If in any hypothetical earth consisting solely of diabase the temperature in any couche were to exceed the melting point of diabase, then tidal instability would set in, the crust would break down and chaos would reign for the time being. In a real earth the same result would follow provided the couche were in a region where diabase or equally fusible rocks are to be expected. Excluding such cases, King found that the age of the earth could not exceed 24 million years when Kelvin's values for diffusivity and

¹ Trans. R. S. Edinburgh, reprinted in Thomson and Tait, "Natural Philosophy," Pt. II, p. 468.

² Am. Jour. Sci., vol. 45, 1893, p. 1.

surface gradient are assumed. He also found that the corresponding initial temperature of such a globe would be 1950° C.

Kelvin's last paper on a cooling earth¹ was read in 1897, and he there stated that after having worked out the problem of conduction of heat outwards from the earth by an elaborate method, he was not led to differ much from Clarence King's estimate. This he adopted as the most probable age and reduced his limits to between 20×10^6 and 40×10^6 years.

While King's earth is tidally stable, I confess that his solution of the problem seems to me to be fatally defective. He himself gives a temperature curve for the same earth at an age of 15 million years, and this earth shows a couche at a temperature above the melting point of diabase, this layer extending from a depth of 34 miles below the surface to 66 miles. According to Laplace's law of densities these two levels correspond respectively to densities of 2.85 and 2.93, and it seems certain that the material must consist chiefly of basaltic rocks. Thus the 15-million-year earth would be unstable and this instability would only just disappear at 24 million years. I am obliged to conclude that if an earth could cool in this way—if the crust could be prevented from breaking—the 24-million-year earth would only just have reached the "consistentior status" or the epoch of solidity.

The real earth, however, has been in a condition of tidal stability at least since the beginning of the Cambrian: for the strata are full of ripple marks, sands and pebbles rearranged by tidal currents, beach footprints and similar evidence of tides. Now oceanic tides would not exist upon a tidally unstable earth and, therefore, the consistentior status occurred long ago. It was the remoteness of this epoch that Kelvin attempted to calculate.

King gives data for only one earth which is satisfactory from this point of view. It had an initial temperature of 1230° C. and reached a surface gradient of 1° F. in 50.6 feet in 10 million years. It was solid almost from the beginning. But apart from the excessive brevity of the age, it seems to me that this earth must likewise be rejected. The temperature was insufficient to melt even diabase a few miles below the surface, much less andesites and rhyolites, while there is a mass of well-known evidence that the earth has been fluid at least to depths of many miles from its growing surface. This is shown by the general dependence of gravity on latitude, the nearly spheroidal shape of the earth, the oblateness of the interior layers of equal density and the conclusion reached by Kelvin,² Roche³ and Wiechert⁴ that a nucleus of constant high density (approx-

¹ Trans. Victoria Institute, vol. 31, 1899, p. 11.

² "Natural Philosophy," Pt. II, p. 420. This article also appeared in the first edition of the "Natural Philosophy," 1867.

³ Mém. Acad. Montpellier, 1882.

⁴ Göttingen Nachrichten, 1897, p. 221.

mately the density of iron) surrounded by a shell of much smaller density (near 3) will satisfy the observations on precession, ratio of surface density to mean density and the ellipticity of sea level.

Considering the materials of which the earth is composed and the high pressures which must have existed at some distance from the surface at any stage of the earth's growth, it seems clear that very high temperatures must have prevailed within its mass, while for the reasons stated above, tidal instability at any epoch since the ocean came into existence is inadmissible. Hence the hypothesis of a constant initial temperature will not satisfy the conditions.

The question thus arises whether the initial temperature may be supposed to have been graduated in such a manner as to satisfy known conditions. I believe that this question may be answered affirmatively. Kelvin himself contemplated a very different distribution of temperature from the uniformity assumed in his equations. The earth, he said, "did in all probability become solid at its melting temperature all through or all through the outer layer"; "convective equilibrium of temperature must have been approximately fulfilled until solidification commenced"; and "the temperature of solidification will, at great depths, because of the great pressure there, be higher than at the surface if the fluid contracts in becoming solid."

MODIFIED PROBLEM.

If the initial temperature at the consistentior status increased with distance from the surface, it was probably according to some complex law intimately related to that of convective equilibrium, but the thickness of the shell which has been sensibly affected by cooling is very small. At a distance of 80 miles below the surface the temperature is probably now very near 99 per cent. of what it was at the consistentior status. Hence if a layer double this thickness is considered, the conditions which prevailed in the remainder of the earth are of no consequence. The inner part, with a radius of say 3840 miles, may have been originally at the temperature of ice or of the electric arc; it may conduct heat as well as silver or as ill as magnesia; in any case the influence on the outer surface would be insensible even after scores of millions of years. Now, though the temperature at the consistentior status did vary with distance from the surface according to a highly complex law, it is altogether probable that for so short a distance as 2 per cent. of the radius this law may be adequately represented by a straight line. This would be comparable with the superficial portion of Mr. Barus's curve, which is nearly rectilinear. Mr. Barus's experiments indicated that the relation between pressure and the melting point of diabase is a linear one, so that, had he confined his conclusions to a shell whose thickness was small relatively to

the radius, his melting point curve would have reduced to a straight line. Such a restriction in the circumstances is not objectionable and corresponds to the limitation of the problem of refrigeration as solved by Kelvin, whose formulas, strictly speaking, represent the cooling of a solid of indefinite extent bounded by a plane surface and not the cooling of a sphere. It can be and has been shown that the error introduced by neglecting the earth's curvature is insignificant.

It will be sufficient, therefore, to assume that the initial temperature of the earth increased in simple proportion to the distance from the surface, and this simplification renders it easy to modify the Fourier equation employed by Kelvin to satisfy this condition. In his solution an infinite homogeneous solid is supposed divided by a plane on one side of which, at the initial instant, the temperature has one uniform value, while on the other side it has another uniform value. The object of the second mass is to replace outer space and preserve a constant temperature in the dividing plane. This device may seem at first sight too artificial, but Kelvin proved that after a comparatively brief period the temperature of the surface of the globe would be affected to an entirely negligible extent by internal heat. Radiation and convection, or briefly "emissivity," accomplish substantially the same end as the hypothetical conducting solid, in that they dispose of the heat as fast as it reaches the surface excepting for a period of possibly a few thousand years after the surface of the globe solidified.¹

Fourier's partial differential equation for the linear conduction of heat is

$$\frac{dv}{dt} = \kappa \frac{d^2v}{dx^2}$$

where v is temperature and x distance from a plane, while κ is the diffusivity which is assumed to be constant and known. Any value whatever of v which will satisfy this equation is a solution of some problem in conduction. In general the problem of finding a value of v which satisfies given boundary conditions is difficult, but in the particular case under discussion the appropriate form of v is easily arrived at. The equations

$$\frac{dv}{dx} = \frac{V}{\sqrt{\pi\kappa t}} e^{-x^2/4\kappa t} + c \quad (1)$$

and

$$v - v_0 = V \frac{2}{\sqrt{\pi}} \int_0^{x/2\sqrt{\kappa t}} e^{-z^2} dz + cx \quad (2)$$

¹ So far as the earth is concerned, the evaporation of water is the most important factor in the removal of heat from the surface to the upper regions of the atmosphere, or in what may be regarded as the emissivity of the globe. According to Sir John Murray's figures (Geochemistry, p. 53), the average annual rainfall less the run-off is about 70 centimeters, and substantially all of this is evaporated. The evaporation of 1 cubic centimeter absorbs about 591 gram calories and thus the total evaporation removes from the earth's surface some 13 times as much heat as the earth emits, the large residue being of course derived from the sun.

satisfy the partial differential equation when c , v_0 and V are constants, as may easily be tested by differentiation. If $c = 0$ they reduce to the forms used by Kelvin. Here v_0 represents the constant temperature at the surface of the cooling mass and V the initial temperature of the cooling mass at an infinitesimal distance from the surface. As will be proved in the next paragraph, c is the constant initial temperature gradient.

From the equations themselves it can be at once determined what boundary conditions are implied. When $t=0$, the upper limit of the integral in (2) becomes infinite, and the value of the integral itself is then $\sqrt{\pi}/2$; consequently when $t = 0$ and x is a positive quantity, the initial distribution of temperature in the real solid is represented by

$$v - v_0 = V + cx$$

while in the imaginary solid replacing outer space at the same instant

$$v - v_0 = -(V + cx)$$

Hence equations (1) and (2) fulfil the conditions demanded by the modified problem under discussion, and represent the cooling of a body in which the initial temperature increased from the surface value, V , in simple proportion to the depth, x .

The superficial temperature gradient at any time is found by making $x = 0$ in (1) and is expressed by

$$\left(\frac{dv}{dx}\right)_0 = \frac{V}{\sqrt{\pi\kappa t}} + c \quad (3)$$

In Kelvin's solution this quantity as well as κ and V is regarded as known, while c is made equal to zero, so that (3) gives the required age. In 1862 it seemed both unobjectionable and inevitable to rely on the surface gradient determined by observation in determining the age of the earth; but it is now known that this gradient is affected by radioactivity, and, therefore, that it cannot be trusted. It is the special purpose of this paper to dispense with the employment of the surface gradient. This will be accomplished by taking advantage of Mr. John F. Hayford's level of isostatic compensation, which lies far below the level at which radioactivity can affect the supply of heat.

If appropriate values of the constants can be found, equations (1) and (2) can be computed for any desired age, and this computation is an easy task because the value of the definite integral in (2) has been tabulated by various mathematicians, the most complete table being by Mr. James Burgess and printed in 1900.¹

Kelvin employed a diffusivity, κ , of 400, using the British foot and the year as units. In c. g. s. units this would be 0.01178. This value was obtained from experiments on the trap rock of Calton Hill, the sand of an

¹ Trans. R. S. Edinburgh, vol. 39, 1900, p. 257.

experimental garden and the sandstone of Craighleith quarry, all at Edinburgh. Different weights were given to these observations, but how is not explained. Now, in considering the diffusivity of the earth it does not seem to me that the ragged pellicle of detrital matter on its surface need be considered. Over large areas it is absent and in most places the sedimentary rocks are saturated with water, so that their own intrinsic diffusivity is a minor feature of the flow of heat. The great bulk of the rocks in the shell affected by cooling are massive and at least comparable with the trap of Calton Hill, which is chiefly composed of Carboniferous basalt and andesite. The conductivity of this rock was observed by Forbes and Thomson (Kelvin) for no less than eighteen years; the thermal capacity was determined by Regnault, so that the value of the diffusivity, 0.00786, is undoubtedly very accurate. It does not stand alone. A committee of the British Association,¹ Herschel and Lebour, reported for whin and traps $\kappa = .0067$, and for serpentine from .00594 to .0073, while Ayrton and Perry got for porphyritic trachyte .0103. I do not think a better choice can be made than the Calton Hill trap, and its diffusivity with the meter and year as units is the value which will be assumed here, *i. e.*, $\kappa = 24.8037$.

That κ varies with temperature and with pressure is probable. That in iron it decreases with increasing temperature is known and analogy would point to the conclusion that it should increase with pressure. Possibly diffusivity is simply related to density and for the same or similar rocks tends in the earth to a nearly constant value. At present it seems unavoidable in this problem to regard it as constant.

The outer portion of the earth is composed of various rocks which are believed to be arranged roughly in the order of density. If so the peridotites underlie the basaltic rocks, while the andesites and rhyolites overlie them. These latter are less fusible than diabase. How deep the level lies which would answer to the upper surface of the basaltic rocks cannot be told with certainty. The best that can be done is to assume that Laplace's law of density is valid for a few score miles from the surface and to consider roughly the effects of heat and pressure. In this way I have reached the conclusion that at about 40 miles, or 0.01 times the radius, where the density should be 2.86, the temperature perhaps 1300° C. and the pressure 17,400 atmospheres, basaltic rocks may begin to appear in place. A pressure of 13 or 14 atmospheres per degree centigrade is probably of the order of magnitude needful to preserve constancy of volume in a heated solid, while at atmospheric pressures the densities of basaltic rocks are from 2.85 to 3.10, with minor exceptions. The outer crust to a depth of 40 miles is thus probably less fusible than basalt.

¹ Brit. Assoc. Ad. Sci., 1881.

As was pointed out above, the line representing the melting point of diabase in terms of depth as determined by Mr. Barus may be taken as rectilinear for depths up to a hundred miles, and is then represented by what I may call the diabase line,

$$y = 1170^\circ + \frac{430}{.01r} x = b + px$$

where r is the radius of the earth, and according to the results of the last paragraph the original temperature distribution in the globe must be such that only the layer of rock within 40 miles of the surface was heated to a higher point than that at which diabase would melt. Thus V being the original surface temperature and the original temperature at distance x being represented for distinction by u ,

$$u = V + cx$$

According to a preceding paragraph it is probable that y and u should have the same value at $0.01r$ or 63,710 meters from the surface, but the discussion can be made a little more general by supposing the intersection to be at a depth nr . Then

$$c = p - \frac{V - b}{nr} \quad (4)$$

It is easy to perceive that whatever values of the constants and the age are chosen, the temperature curve will have one and only one tangent which is parallel to the diabase line. Of course the point of tangency is that at which the curve approaches the melting point of diabase most closely or at which the additional temperature which would be required to melt diabase is a minimum. It is at this level of tangency that any access of temperature due to the dissipation of mechanical energy or to other causes is most likely to produce fusion at depths where the rock is diabasic. If the constants are assumed at any value and the courses of the curves are considered for various periods of time, it is easily seen that the point of nearest approach to the diabase line sinks to greater depths as time elapses.

Now, strains must exist in the earth at all times. They may be and are partially relieved by rupture and by solid flow, but most completely by fusion. Thus in an earth the cooling of which is represented by (2) such strains as may be incident to upheaval and subsidence and to orogeny will probably be most completely relieved at the slowly sinking surface of easiest fusion.

Messrs. Tittmann and Hayford have recently discussed the whole body of geodetic data for the United States and have shown that the deflections of the vertical are best accounted for by the hypothesis that isostatic com-

pensation is uniformly distributed and is complete at a depth of 114 kilometers or 71 miles from the surface.¹

I, therefore, adopt the hypothesis that the tangent of the temperature curve, or equation (1), is parallel to the diabase line at 114 kilometers from the surface.

V is the value of the original temperature of the earth at its surface. As was pointed out above, this must have been high enough to fuse rocks more refractory than diabase and was probably about equal to the temperature of the hottest eruptions which now reach the surface of the earth. It seems to me that 1300° is a reasonable estimate. This is considerably below the melting point of pure iron and lower than the blast furnace, but above the melting point of copper (1065°), which lavas are known to fuse, and of Barus's diabase (1170°). So far as I know, no precise determinations have yet been made of the temperatures at which lavas issue from their vents.

To make the use suggested above of the level of isostatic compensation let its depth be represented by x , and in equation (1) let $(dv/dx)_1 = p$, the gradient of the diabase line. Then since p also appears in the value of c , p disappears and $(dv/dx)_1 - c =$

$$\frac{V-b}{nr} = \frac{V}{\sqrt{\pi\kappa t}} e^{-x^2/4\kappa t}$$

On the assumptions here made this equation determines the age of the earth and this age is independent either of the gradient of Mr. Barus's diabase line or of the initial temperature gradient. The constants involved are the initial surface temperature (V), the melting point of diabase at the earth's surface (b), the diffusivity (κ), the distance from the surface of the top of the diabase couche (nr) and the depth of the level of isostatic compensation (x_1). The surface temperature gradient does not in any manner enter into this expression, which is thus wholly distinct from that employed to determine the age by Kelvin.

NUMERICAL RESULTS.

Solving for V and substituting the numerical values for π , κ , r and x_1 gives

$$\frac{1}{V} = \frac{1}{1170^\circ} \left(1 - \frac{721,720n}{\sqrt{t}} e^{-\frac{130.99 \times 10^6}{t}} \right)$$

in which n is left indeterminate to facilitate any variations in the depth of the top of the diabase couche thought desirable. The form of the equation is such that t is almost necessarily taken as the independent variable, but that is of no consequence.

¹ Rep. to 15th general conference of the International Geodetic Assoc., Washington, 1906.

In the Coast and Geodetic Surv. Report on the Figure of the Earth and Isostasy, 1909, Mr. Hayford gives the depth of compensation ("solution G") at 113.7 kilometers. In my paper in Science, vol. 27, 1908, p. 227, this depth was stated by a blunder in copying at 140 kilometers, but the correct value was used in the computations.

When V is computed or assumed c may be found from (4) and the surface gradient is then ascertained from (3). Taking n at 0.01 as indicated by Laplace's law I now get the following table of related values:

T	30.	50.	55.	60.	65.	100.
V	1190. ^o	1264.	1286.	1307.	1329.	1453.
c	0.00644	0.00527	0.00493	0.00459	0.00426	0.00231
$1/c$	155 ^m	190.	203.	218.	235.	433.
G ^o C.	32 ^m 2	39.2	40.7	42.2	43.6	53.3
G ^o F.	58.'7	71.4	74.2	76.9	79.5	97.1

T is the age in millions of years; V is the initial surface temperature; c is the initial gradient of internal temperature and $1/c$ gives this gradient in terms of meters per degree centigrade. G^o C. is the final surface gradient in terms of meters per degree centigrade and G^o F. is the same gradient in terms of feet per degree Fahrenheit.¹

In all of these earths the upper surface of the diabase couche is supposed to be at one one-hundredth of the radius from the surface, or 63,710 meters, but another assumption will be made below. All of the excess of temperature curves have tangents parallel to the diabase line at a depth of 114,000 meters. Tidal stability is provided for at all times by the equations; for the temperature curves never cross the diabase line excepting within 40 miles of the surface, or within the shell of more refractory rocks.²

Of the six earths computed the one whose initial temperature comes nearest to 1300^o C. is that of the 60-million-year earth, and it is the one

¹ Mr. Hayford's latest value for the level of isostatic compensation appeared while this paper was in press. It is contained in Supplementary Investigation of the Figure of the Earth and Isostasy, Coast and Geod. Surv., 1910, and is 120,900 meters. This new value would reduce the initial temperature of a 60×10^6 globe to 1272^o, or give for a globe whose initial temperature was 1300^o an age of 68 million years, with a surface gradient of 1^o C. in 44 meters.

² Immediately after the primitive consolidation of the earth it is almost certain that a period of aqueoigneous fusion supervened, as I pointed out in Bull. Geol. Soc. Amer., vol. 19, 1908, p. 142. This was the period of the formation of the granitoid rocks and it may have been of rather brief duration. The formulæ of the text are applicable to the very early stages of the cooling of a large lava field rather than to the very first part of the refrigeration of the globe, but after a time so brief as to be insignificant compared with the earth's age the equations used would apply. To illustrate this point fancy the hypothetical earth whose initial temperature V was 1307^o with gradient $c = 0.00459$ to have cooled until it emitted just heat enough to raise the temperature of a sheet of water 1 meter deep 1^o in 1 day. This means an emission of 36,524 gram calories per annum, and this would be totally expended by an evaporation of a layer of water about 62 centimeters deep. This is considerably less than the present mean evaporation on continental areas. Hence there is no difficulty in imagining the surface of the earth at this stage kept constant by emissivity, including evaporation, and from that period on the formulæ may be considered as applicable.

Now if the theory held good also prior to this period, the lapse of time since the consistentior status would be only a trifle more than 28 years, the gradient would be nearly 28^o per meter and the rock would be red-hot, or at 500^o, only 18 meters from the surface. These results depend on the conductivity of the rock (or the diffusivity multiplied by the thermal capacity) which for the Calton Hill trap is 0.00415 in c. g. s. units, the heat emitted per second per square centimeter being the product of the surface gradient and the conductivity. Doubtless the real duration of this initial stage was longer than 28 years. If it were a hundred times as long, or even 1000 times, the age of the earth would be substantially the same as if the formulæ accurately represented the entire process.

which appears to me most probable. The most evident objection to it is the low surface gradient of 1° F. in 77 feet, while Kelvin took 1° F. in 50.6 feet and King stated that in 1893 the last published value as reduced from all available data by the British Association committee was 64 feet per degree Fahrenheit. King himself considered 75 feet a maximum. To me, however, it does not seem that an average value is what is required. In discussing the cooling of the earth disturbing causes must be eliminated as far as possible. Now several causes must contribute more or less to raise the temperature of rocks near the surface; for example, thermal springs, volcanic heat, the dissipation of mechanical energy by faulting or solid flow, the liberation of heat in the decomposition of minerals and radioactivity. So far as I know, the only causes which can lead to a deceptively low gradient in rocks of a given type are the neighborhood of large bodies of cold water and abnormally high diffusivity. Furthermore, to include gradients observed in sedimentary rocks seems to me to complicate the problem unnecessarily. The gradients which should serve as a guide are those in massive rocks, especially the nearly anhydrous basaltic, andesitic and rhyolitic massives. All cases where there are local evidences of heat due to thermal springs, etc., should be excluded, and when for a normal rock the gradient is unusually high, it should be considered as suspicious. In short, for the present problem the lower gradients in massive rocks are those most likely to give a correct value of the earth's age.

Several writers have made suggestions of this kind, but Mr. Johann Koenigsberger¹ has given the only detailed discussion of the matter known to me, pointing out that the characteristic gradients are those observed in nearly level, inland regions, in chemically unaltered rocks. Of such he gives 26 cases, ranging from 1° C. in 27.8 meters to 1° in 37.9 meters. Five of these average 1° in 37.7 meters, so that, in round numbers, 1° in 38 meters seems to me best to represent the true terrestrial gradient. Comparing this gradient with the 1° in 42.2 meters computed for the 60-million-year earth leaves a difference of 1° in 385 meters to be accounted for by exothermic chemical action. This is no doubt an inaccurate remainder, but it is probably of the order of magnitude of the correction appropriate to radioactivity and similar causes.²

Mr. Koenigsberger in the same paper has also shown that the temperatures observed in the Swiss railway tunnels can be computed from Fourier's equations and average superficial temperature gradients when the topographic forms are duly represented in the formulæ. Hence it is not needful to assume radioactivity in order to account for these phenomena.

¹ Congrès géolog. Intern., tenth session, Mexico, Comptes Rendus, 1907, p. 1127.

² See "Relations of Radioactivity to Cosmogony and Geology," Bull. Geol. Soc. Amer., vol. 19, 1908, p. 113.

The least satisfactory of the assumptions made in the foregoing discussion of refrigeration is that the depth of the diabase couche is 40 miles or $0.01r$. This was estimated from Laplace's law, but that is a mere approximation or at best a mean. In order to test the consequences of a variation in the thickness of the refractory shell (nr) I have also computed some cases for $nr = 0.0075r$, or say 30 miles. Of these I will give only that appropriate to an initial surface temperature of $V = 1300^\circ$. The results are:

T	70.
c	0.00403
$1/c$	248.
G°C.	46.3
G°F.	84.3

Thus the age falls out notably greater and the gradient lower, leaving double the margin for chemical heat estimated in the case of the 60-million-year earth. While these results are less probable than those for the younger earth, they do not seem impossible and, in my opinion, 70 million years may be taken as a maximum value of the age of the earth as determined from refrigeration.

With such constants as are now available it seems to me that the age of the globe considered as a cooling body must be between 70 million and 55 million years, limits not differing greatly from those found by other means in the earlier part of this publication.

CONCLUSION.

In the stratigraphical method of determining the age of the ocean the weak point is the uncertainty of the duration of pre-Cambrian time. The best determination of the date of the base of the Cambrian seems to be that by Mr. Walcott, who places it at 27,640,000 years ago. The order of magnitude of the pre-Cambrian period is probably the same, so that stratigraphy indicates an age of the ocean of, say, between 50 and 65 million years. This is in accord with Mr. Sollas's most recent results, for he regards 80 million as a maximum without being able to give a definite account of nearly so long a period.

Considering sodium accumulation as an asymptotic process, as it unquestionably is, the weak point is the possibility that the primitive ocean was salt, or that there have been continents in the oceanic basin. These possibilities do not affect an estimate of the maximum age, 74 million years, but preclude a definite minimum. Assuming that neither of these possibilities was realized, the minimum would be about 46 million years.

Refrigeration, so dealt with as to exclude tidal instability, and computed on the basis of Mr. Hayford's level of isostatic compensation, with-

out employing as a datum any observed superficial temperature gradient, yields results which can hardly be forced above 70 million or below 55 million years. The weak point here is our ignorance of the depth of the top of the diabase couche; but if Laplace's law of density holds true, the limits would be about 65 and 55 million years.

These three methods seem to be mutually confirmatory and to give results which converge towards some value near 60 or perhaps 65 million years.

This being granted, it follows that radioactive minerals cannot have the great ages which have been attributed to them. Only something like a tenth of the heat emitted by the earth can be ascribed to radioactivity plus all other exothermic chemical transformations; the remaining nine-tenths is heat due to compression.