criticism of Ostwald (under date Sept. 16, 1891): "The electrostatic theory of cohesion is new to me, * * * but for electrolytes there is the question to be answered, why stuffs like alcohol, etc., do not conduct? whilst according to *your* theory, *all* elements have electric charges."

These objections could not be met then, and have not been met up to the present time, in spite of the fact that this new concept (of the ionic charge being a fundamental part of the atom, apart from its chemical functions) has proved a most fertile one, and has been considerably developed by the orignator and by later workers, Richartz, Chattock, Lorentz, Larmor and others. Nor will these objections ever be met until we know the nature of metallic conduction.

This is one of the great outstanding problems. It has long been known that there is a relation between electric and heat conductivity. The writer has shown that there is a connection between the velocity of sound (and hence the elasticity and density) and the electric conductivity of wires. J. J. Thomson, as mentioned above, suggested that the current was carried by the electrolysis of molecular groupings, and his later work renders it probable that it is by means of the corpuscles. It is possible that the atoms of a metal are really dissociated and the negatively charged corpuscles are in a state similar to that of the ions of a solution, *i. e.*, the metallic atom is not a fixed combination of certain corpuscles, but is constantly changing in composition, the negative corpuscles being, as it were, in solution in the metal, and changing about freely.

Such an hypothesis would account for the relation between the velocity of sound and the electric conductivity. For the cohesion of the atoms would be due to these negative corpuscles acting, as the mortar between bricks, to bind together the positive groupings, and hence the greater the number of free corpuscles the greater the elasticity and the greater the conductivity, the conductivity being simply the number of free corpuscles per cubic centimeter. The greater the number of corpuscles in the positive groupings, *i. e.*, the greater the molecular mass, the less the conductivity.

In presenting this summary I am aware, of course, that much of it is in need of further experimental evidence, and I hope, in time, to supply at least a part of this. It is considered, however, that the scheme here presented has a weight apart from its experimental foundation, in that it is a whole and consistent theory by which for the first time all physical phenomena are reduced to the simplest possible elements.

REGINALD A. FESSENDEN.

ADDRESS OF THE PRESIDENT OF THE SECTION OF GEOLOGY OF THE BRITISH ASSOCIATION.

I.

EVOLUTIONAL GEOLOGY.

THE close of one century, the dawn of another, may naturally suggest some brief retrospective glance over the path along which our science has advanced, and some general survey of its present position from which we may gather hope of its future progress; but other connection with geology the beginnings and endings of centuries have none. The great periods of movement have hitherto begun, as it were, in the early twilight hours, long before the dawn. Thus the first step forward, since which there has been no retreat, was taken by Steno in the year 1669; more than a century elapsed before James Hutton (1785) gave fresh energy and better direction to the faltering steps of the young science; while it was less than a century later (1863) when Lord Kelvin brought to its aid the powers of the higher mathematics and instructed it in the teachings of modern physics. From Steno onward the spirit of geology was catastrophic; from Hutton onward it grew increasingly uniformitarian; from the time of Darwin and Kelvin it has become evolutional. The ambiguity of the word 'uniformitarian' has led to a good deal of fruitless logomachy, against which it may be as well at once to guard by indicating the sense in which it is used here. In one way we are all uniformitarians, *i. e.*, we accept the doctrine of the 'uniform action of natural causes,' but, as applied to geology, uniformity means more than this. Defined in the briefest fashion it is the geology of Lyell. Hutton had given us a 'Theory of the Earth,' in its main outlines still faithful and true; and this Lyell spent his life in illustrating and advocating; but as so commonly happens the zeal of the disciple outran the wisdom of the master, and mere opinions were insisted on as necessary dogma. What did it matter if Hutton as a result of his inquiries into terrestrial history had declared that he found no vestige of a beginning, no prospect of an end? It would have been marvellous if he had! Consider that when Hutton's 'Theory' was published William Smith's famous discovery had not been made, and that nothing was then known of the orderly succession of forms of life, which it is one of the triumphs of geology to have revealed; consider, too, the existing state of physics at the time, and that the modern theories of energy had still to be formulated; consider also that spectroscopy had not yet lent its aid to astronomy and the consequent ignorance of the nature of nebulæ; and then, if you will, cast a stone at Hutton. With Lyell, however, the case was different: in pressing his uniformitarian creed upon geology he omitted to take into account the great advances made by its sister sciences, although he had knowledge of them, and thus sinned against the light. In the last edition of the famous 'Principles' we read: "It is a favorite dogma of some physicists that not only the earth, but the sun itself, is continually losing a portion of its heat, and that as there is no known source by which it can be restored we can foresee the time when all life will cease to exist on this planet, and on the other hand we can look back to a period when the heat was so intense as to be incompatible with the existence of any organic beings such as are known to us in the living or fossil world. * * * A geologist in search of some renovating power by which the amount of heat may be made to continue unimpaired for millions of years, past and future, in the solid parts of the earth * * * has been compared by an eminent physicist to one who dreams he can discover a source of perpetual motion and invent a clock with a self-winding apparatus. *But why should we despair of detecting proofs of such generating and self-sustaining power in the works of a Divine Artificer?*" Here we catch the true spirit of uniformity; it admittedly regards the universe as a self-winding clock, and barely conceals a conviction that the clock was warranted to keep true Greenwich time. The law of the dissipation of energy is not a dogma, but a doctrine drawn from observation, while the uniformity of Lyell is in no sense an induction; it is a dogma in the narrowest sense of the word, unproved, incapable of proof, hence perhaps its power upon the human mind; hence also the transitoriness of that power. Again, it is only by restricting its inquiries to the stratified rocks of our planet that the dogma of uniformity can be maintained with any pretence of argument. Directly we begin to search the heavens the possibility, nay even the likelihood, of the nebular origin of our system, with all that it involves, is borne in upon us. Lyell therefore consistently refused to extend his gaze beyond the rocks beneath his feet, and was thus lead to do a serious injury to our science; he severed it from cosmogony, for which he entertained and expressed the most profound contempt, and from the mutilation thus inflicted geology is only at length making a slow and painful recovery. Why do I dwell on these facts? To depreciate Lyell? By no means. No one is more conscious than I of the noble service which Lyell rendered to our cause; his reputation is of too robust a kind to suffer from my unskilful handling, and the fame of his solid contributions to science will endure long after these controversies are forgotten. The echoes of the combat are already dying away, and uniformitarians, in the sense already defined, are now no more; indeed, were I to attempt to exhibit any distinguished living geologist as a still surviving supporter of the narrow Lyellian creed, he would probably feel, if such a one there be, that I was unfairly singling him out for unmerited obloquy.

Our science has become evolutional, and in the transformation has grown more comprehensive; her petty parochial days are done, she is drawing her provinces closer around her, and is fusing them together into a united and single commonwealth—the science of the earth.

Not merely the earth's crust, but the whole of earth-knowledge is the subject of our research. To know all that can be known about our planet, this, and nothing less than this, is its aim and scope. From the morphological side geology inquires, not only into the existing form and structure of the earth, but also into the series of successive morphological states through which it has passed in a long and changeful development. Our science inquires also into the distribution of the earth in time and space; on the physiological side it studies the movements and activities of our planet; and not content with all this it extends its researches into etiology and endeavors to arrive at a science of causation. In these pursuits geology calls all the other sciences to her aid. In our commonwealth there are no outlanders; if an eminent physicist enter our territory we do not begin at once to prepare for war, because the very fact of his undertaking a geological inquiry of itself confers upon him all the duties and privileges of citizenship. A physicist studying geology is by definition a geologist. Our only regret is, not that physicists occasionally invade our borders, but that they do not visit us oftener and make closer acquaintance with us.

EARLY HISTORY OF THE EARTH: FIRST CRITICAL PERIOD.

If I am bold enough to assert that cosmogony is no longer alien to geology, I may proceed further, and taking advantage of my temerity pass on to speak of things once not permitted to us. I propose, therefore, to offer some short account of the early stages in the history of the earth. Into its nebular origin we need not inquire—that is a subject for astronomers. We are content to accept the infant earth from their hands as a molten globe ready made, its birth from a gaseous nebula duly certified. If we ask, as a matter of curiosity, what was the origin of the nebula, I fear even astronomers cannot tell us. There is an hypothesis which refers it to the clashing of meteorites, but in the form in which this is usually presented it does not help us much. Such meteorites as have been observed to penetrate our atmosphere and to fall on to the surface of the earth prove on examination to have had an eventful history of their own of which not the least important chapter was a passage through a molten state; they would thus appear to be the products rather than the progenitors of a nebula.

We commence our history, then, with a rapidly-rotating molten planet, not impossibly already solidified about the center and surrounded by an atmosphere of great depth, the larger part of which was contributed by the water of our present oceans, then existing in a state of gas. This atmosphere, which exerted a pressure of something like 5,000 pounds to the square inch, must have played a very important part in the evolution of our planet. The molten exterior absorbed it to an extent which depended on the pressure, and which may some day be learnt from experiment. Under the influence of the rapid rotation of the earth the atmosphere would be much deeper in equatorial than polar regions, so that in the latter the loss of heat by radiation would be in excess. This might of itself lead to convectional currents in the molten ocean. The effect on the atmosphere is very difficult to trace, but it is obvious that if a high-pressure area originated over some cooler region of the ocean, the winds blowing out of it would drive before them the cooler superficial layers of molten material, and as these were replaced by hotter lava streaming from below, the tendency would be to convert the high into a low-pressure area, and to reverse the direction of the winds. Conversely under a low-pressure area the in-blowing winds would drive in the cooler superficial layers of molten matter that had been swept away from the anticyclones. If the difference in pressure under the cyclonic and anticyclonic areas were considerable, some of the gas absorbed under the anticyclones might escape beneath the cyclones, and in a later stage of cooling might give rise to vast floating islands of scoria. Such islands might be the first foreshadowings of the future continents. Whatever the ultimate effect of the reaction of the winds on the currents of the molten ocean, it is probable that some kind of circulation was set up in the latter. The universal molten ocean was by no means homogeneous: it was constantly undergoing changes in composition as it reacted chemically with the internal metallic nucleus; its currents would streak the different portions out in directions which in the northern hemisphere would run from northeast to southwest, and thus the differences which distinguish particular petrological regions of our planet may have commenced their existence at a very early stage. Is it possible that as our knowledge extends we shall be able by a study of the distribution of igneous rocks and minerals to draw some conclusions as to the direction of these hypothetical lava currents? Our planet was profoundly disturbed by tides, produced by the sun; for as yet there was no moon; and it has been suggested that one of its tidal waves rose to a height so great as to sever its connection with the earth and to fly off as the infant moon. This event may be regarded as making the first critical period, or catastrophe if we please, in the history of our planet. The career of our satellite, after its escape from the earth, is not known till it attained a distance of nine terrestrial radii; after this its progress can be clearly followed. At the eventful time of parturition the earth was rotating, with a period of from two to four hours, about an axis inclined at some 11° or 12° to the ecliptic. The time which has elapsed since the moon occupied a position nine terrestial radii distant from the earth is at least fifty-six to fifty-seven millions of years, but may have been much more. Professor Darwin's story of the moon is certainly one of the most beautiful contributions ever made by astronomy to geology, and we shall all concur with him when he says, "A theory reposing on *veræ causæ*, which brings into quantitative correlation the length of the present day and month, the obliquity of the ecliptic, and the inclination and eccentricity of the lunar orbit, must, I think, have strong claims to acceptance."

The majority of geologists have long

hankered after a metallic nucleus for the earth, composed chiefly, by analogy with meteories, of iron. Lord Kelvin has admitted the probable existence of some such nucleus, and lately Professor Wiechert has furnished us with arguments—'powerful' arguments Professor Darwin terms them—in support of its existence. The interior of the earth for four-fifths of the radius is composed, according to Professor Wiechert, chiefly of metallic iron, with a density of 8.2; the outer envelope, one-fifth of the radius, or about 400 miles in thickness, consists of silicates, such as we are familiar with in igneous rocks and meteorites, and possesses a density of 3.2. It was from this outer envelop when molten that the moon was trundled off, twenty-seven miles in depth going to its formation. The density of this material, as we have just seen, is supposed to be 3.2; the density of the moon is 3.39, a close approximation, such difference as exists being completely explicable by the comparatively low temperature of the moon.

The outer envelope of the earth which was drawn off to form the moon was, as we have seen, charged with steam and other gases under a pressure of 5,000 lb. to the square inch; but as the satellite wandered away from the parent planet this pressure continuously diminished. Under these circumstances the moon would become as explosive as a charged bomb, steam would burst forth from numberless volcanoes, and while the face of the moon might thus have acquired its existing features, the ejected material might possibly have been shot so far away from its origin as to have acquired an independent orbit. If so we may ask whether it may not be possible that the meteorites, which sometimes descend upon our planet, are but portions of its own envelope returning to it. The facts that the average specific gravity of those meteorites which have been seen to fall is not much above 3.2, and that they have passed through a stage of fusion, are consistent with this suggestion.

SECOND CRITICAL PERIOD. 'CONSISTENTIOR STATUS.'

The solidification of the earth probably became completed soon after the birth of the moon. The temperature of its surface at the time of consolidation was about 1,170° C., and it was therefore still surrounded by its primitive deep atmosphere of steam and other gases. This was the second critical period in the history of the earth, the stage of the 'consistentior status,' the date of which Lord Kelvin would rather know than that of the Norman Conquest, though he thinks it lies between twenty and forty millions of years ago, probably nearer twenty than forty.

Now that the crust was solid there was less reason why movements of the atmosphere should be unsteady, and definite regions of high and low pressure might have been established. Under the high-pressure areas the surface of the crust would be depressed; correspondingly under the low-pressure areas it would be raised; and thus from the first the surface of the solid earth might be dimpled and embossed.*

THIRD CRITICAL PERIOD. ORIGIN OF THE OCEANS.

The cooling of the earth would continuously progress, till the temperature of the surface fell to 370° C., when that part of the atmosphere which consisted of steam would begin to liquefy; then the dimples on the surface would soon become filled with superheated water, and the pools so formed would expand and deepen, till they formed the oceans. This is the third crit-

*It would be difficult to discuss with sufficient brevity the probable distribution of these inequalities, but it may be pointed out that the moon is possibly responsible, and that in more ways than one, for much of the existing geographical asymmetry.

ical stage in the history of the earth, dating according to Professor Joly, from between eighty and ninety millions of years ago. With the growth of the oceans the distinction between land and sea arose—in what precise manner we may proceed to inquire. If we revert to the period of the 'consistentior status,' when the earth had just solidified, we shall find, according to Lord Kelvin, that the temperature continuously increased from the surface, where it was 1,170° C., down to a depth of twenty-five miles, where it was about 1,430° C., or 260° C. above the fusion point of the matter, forming a crust. That the crust at this depth was not molten but solid is to be explained by the very great pressure to which it was subjected—just so much pressure, indeed, as was required to counteract the influence of the additional 260° C. Thus if we could have reduced the pressure on the crust we should have caused it to liquefy; by restoring the pressure it would resolidify. By the time the earth's surface had cooled down to 370° C. the depth beneath the surface at which the pressure just kept the crust solid would have sunk some slight distance inwards, but not sufficiently to affect our argument.

The average pressure of the primitive atmosphere upon the crust can readily be calculated by supposing the water of the existing oceans to be uniformly distributed over the earth's surface, and then by a simple piece of arithmetic determining its depth; this is found to be 1.718 miles, the average depth of the oceans being taken at 2.393 miles. Thus the average pressure over the earth's surface, immediately before the formation of the oceans, was equivalent to that of a column of water 1.718 miles high on each square inch. Supposing that at its origin the oceans were all 'gathered together into one place,' and 'the dry land appeared,' then the pressure over the ocean floor would be increased from 1.718 miles to 2.393 miles, while that over those portions of the crust that now formed the land would be diminished by 1.718 miles. This difference in pressure would tend to exaggerate those faint depressions which had arisen under the primitive anti-cyclonic areas, and if the just solidified material of the earth's crust were set into a state of flow, it might move from under the ocean into the bulgings which were rising to form the land, until static equilibrium were established. Under these circumstances the pressure of the ocean would be just able to maintain a column of rock 0.886 miles in height, or ten twenty-sevenths of its own depth. It could do no more; but in order that the dry land may appear some cause must be found competent either to lower the ocean bed the remaining seventeen twenty-sevenths of its full depth, or to raise the continental bulgings to the same extent. Such a cause may, I think, be discovered in a further effect of the reduction in pressure over the continental areas. Previous to the condensation of the ocean, these, as we have seen, were subjected to an atmospheric pressure equal to that of a column of water 1.718 miles in height. This pressure was contributory to that which caused the outer twenty-five miles of the earth's crust to become solid; it furnished, indeed, just about one-fortieth of that pressure, or enough to raise the fusion point 6° C. What, then, might be expected to happen when the continental area was relieved of this load? Plainly a liquefaction and corresponding expansion of the underlying rock.

But we will not go so far as to assert that actual liquefaction would result; all we require for our explanation is a great expansion; and this would probably follow whether the crust were liquefied or not. For there is good reason to suppose that when matter at a temperature above its ordinary fusion point is compelled into the solid state by pressure, its volume is very responsive to changes either of pressure or temperature. The remarkable expansion of liquid carbon dioxide is a case in point: 120 volumes of this fluid at — 20° C. becomes 150 volumes at 33° C.; a temperature just below the critical point. A great change of volume also occurs when the material of igneous rocks passes from the crystalline stage to that of glass; in the case of diabase* the difference in volume of the rock in the two states at ordinary temperature is 13 per cent. If the relief of pressure over the site of continents were accompanied by volume changes at all approaching this, the additional elevation of seventeen twenty-sevenths required to raise the land to the sea-level would be accounted for.† How far down beneath the surface the unloading of the continents would be felt it is difficult to say, though the problem is probably not beyond the reach of mathematical analysis; if it affected an outer envelope twenty-five miles in thickness, a linear expansion of four per cent. would suffice to explain the origin of ocean basins. If now we refer to the dilatation determined by Carl Barus for rise in temperature in the case of diabase, we find that between 1093° and 1112° C. the increase in volume is 3.3 per cent. As a further factor in deepening the ocean basins may be included the compressive effect of the increase in load over the ocean floor; this increase is equal to the pressure of a column of water 0.675 mile in height, and its effect in raising the fusion point would be 2° C., from which we may gain some kind of idea of the amount of compression it might produce on the yielding interior of the crust. To admit that these views are speculative will be to confess nothing; but they certainly account for a good deal. They not only give us ocean basins, but basins of the kind we want, that is, to use a crude comparison once made by the late Dr. Carpenter, basins of a tea-tray form, having a somewhat flat floor and steeply sloping sides; they also help to explain how it is that the value of gravity is greater over the ocean than over the land.

The ocean when first formed would consist of highly heated water, and this, as is well known, is an energetic chemical reagent when brought into contact with sili-

* C. Barus so names the material on which he experimented; apparently the rock is a fresh dolorite without olivine.

† Professor Fitzgerald has been kind enough to express part of the preceding explanation in a more precise manner for me. He writes: "It would require a very nice adjustment of temperatures and pressures to work out in the simple way you state it; but what is really involved is that in a certain state diabase (and everything that changes state with a considerable change of volume) has an enormous isothermal compressibility. Although this is very enormous in the case of bodies which melt suddenly, like ice, it would also involve very great compressibilities in the case of bodies even which melted gradually, if they did so at all quickly, *i. e.*, within a small range of temperature. What you postulate, then, is that at a certain depth diabase is soft enough to be squeezed from under the oceans, and that, being near its melting point, the small relief of pressure is accompanied by an enormous increase in volume which helped to raise the continents. Now that I have written the thing out in my own way it seems very likely. It is, anyway, a suggestion quite worthy of serious consideration, and a process that in some places must almost certainly have been in operation, and may be is still operative. Looking at it again, I hardly think it is quite likely that there is or could be much squeezing sideways of liquid or other viscous material from under one place to another, because the elastic yielding of the inside of the earth would be much quicker than any flow of this kind. This would only modify your theory, because the diabase that expands so much on the relief of pressure might be that already under the land, and raising up this latter, partly by being pushed up itself by the elastic relief of the inside of the earth and partly by its own enormous expansibility near its melting point. The action would be quite slow, because it would cool itself so much by its expansion that it would have to be warmed up from below, or by tidal earth-squeezing, or by chemical action, before it could expand isothermally.

cates like those which formed the primitive crust. As a result of its action saline solutions and chemical deposits would be formed; the latter, however, would probably be of no great thickness, for the time occupied by the ocean in cooling to a temperature not far removed from the present would probably be included within a few hundreds of years.

THE STRATIFIED SERIES.

The course of events now becomes somewhat obscure, but sooner or later the familiar processes of denudation and the deposition started into activity, and have continued acting uninterruptedly ever since. The total maximum thickness of the sedimentary deposits, so far as I can discover, appears to amount to no less than 50 miles, made up as follows:

Recent and Pleistocene	4,000	Man.
Pliocene	5,000	Pithecanthropus.
Miocene	9,000	
Oligocene	12,000	
Eocene	12,000	Entheria.
Cretaceous	14,000	
Jurassic	8,000	
Trias	13,000	Mammals.
Permian	12,000	Reptiles.
Carboniferous	24,000	Amphibia.
Devonian	22,000	Fish.
Silurian	15,000	
Ordovician	17,000	
Cambrian	16,000	Invertebrata.
Keeweenawan	50,000	
Penokee	14,000	
Huronian	18,000	

Geologists, impressed with the tardy pace at which sediments appear to be accumulating at the present day, could not contemplate this colossal pile of strata without feeling that it spoke of an almost inconceivably long lapse of time. They were led to compare its duration with the distances which intervene between the heavenly bodies; but while some chose the distance of the nearest fixed star as their unit, others were content to measure the years in terms of miles from the sun.

EVOLUTION OF ORGANISMS.

The stratified rocks were eloquent of time, and not to the geologist alone; they appealed with equal force to the biologist. Accepting Darwin's explanation of the origin of species, the present rate at which form flows to form seemed so slow as almost to amount to immutability. How vast then must have been the period during which by slow degrees and innumerable stages the protozoon was transformed into the man! And if we turn to the stratified column, what do we find? Man, it is true, at the summit, the oldest fossiliferous rocks 34 miles lower down, and the fossils they contain already representing most of the great classes of the Invertebrata, including Crustacea and Worms. Thus the evolution of the Vertebrata alone is known to have occupied a period represented by a thickness of 34 miles of sediment. How much greater, then, must have been the interval required for the elaboration of the whole organic world! The human mind, dwelling on such considerations as these, seems at times to have been affected by a sur-excitation of the imagination, and a consequent paralysis of the understanding, which led to a refusal to measure geological time by years at all, or to reckon by anything less than 'eternities.'

GEOLOGIC PERIODS OF TIME.

After the admirable address of your President last year it might be thought needless for me to again enter into a consideration of this subject; it has been said, however, that the question of geological time is like the Djin in Arabian tales, and will irrepressibly come up again for discussion, however often it is disposed of. For my part I do not regard the question so despondingly, but rather hope that by persevering effort we may succeed in discovering the talisman by which we may compel the unwilling Djin into our service. How immeasurable would be the advance of our science could we but bring the chief events which it records into some relation with a standard of time!

Before proceeding to the discussion of estimates of time drawn from a study of stratified rocks let us first consider those which have been already suggested by other data. These are as follows: (1) Time which has elapsed since the separation of the earth and moon, fifty-six millions of years, minimum estimate by Professor G. H. Darwin. (2) Since the 'consistentior status,' twenty to forty millions (Lord Kelvin). (3) Since the condensation of the oceans, eighty to ninety millions, maximum estimate by Professor J. Joly.

It may be at once observed that these estimates, although independent, are all of the same order of magnitude, and so far confirmatory of each other. Nor are they opposed to conclusions drawn from a study of stratified rocks; thus Sir Archibald Geikie, in his address to this Section last year, affirmed that, so far as these were concerned, one hundred millions of years might suffice for their formation. There is then very little to quarrel about, and our task is reduced to an attempt, by a little stretching and a little paring, to bring these various estimates into closer harmony.

Professor Darwin's estimate is admittedly a minimum; the actual time, as he himself expressly states, 'may have been much longer.' Lord Kelvin's estimate, which he would make nearer twenty than forty millions, is founded on the assumption that since the period of the 'consistentior status' the earth has cooled simply as a solid body, the transference of heat from within outwards having been accomplished solely by conduction.*

It may be at once admitted that there is a large amount of truth in this assumption; there can be no possible doubt that the earth reacts towards forces applied for a short time as a solid body. Under the influence of the tides it behaves as though it possessed a rigidity approaching that of steel, and under sudden blows, such as those which give rise to earthquakes, with twice this rigidity, as Professor Milne informs me. Astronomical considerations lead to the conclusion that its effective rigidity has not varied greatly for a long period of past time.

Still, while fully recognizing these facts, the geologist knows—we all know—that the crust of the earth is not altogether solid. The existence of volcanoes by itself suggests the contrary, and although the total amount of fluid material which is brought up from the interior to the exterior of the earth by volcanic action may be, and certainly is, small—from data given by Professor Penck, I estimate it as equivalent to a layer of rock uniformly distributed 2 mm. thick per century; yet we have every reason to believe that volcanoes are but the superficial manifestation of far greater bodies of molten material which lie concealed beneath the ground. Even the wide areas of plutonic rock, which are sometimes exposed to view over a country that has suffered long-continued denudation, are merely the upper portion of more extensive masses which lie remote from view. The existence of molten material within the earth's crust naturally awakens a suspicion that the process of cooling has not been wholly by conduction, but also to some slight extent by convection, and to a still greater extent by the bodily migration of liquid lava from the deeper layers of the crust towards the surface.

The existence of local reservoirs of molten rock within the crust is even still more important in another connection, that is, in relation with the supposed 'average rate of increase of temperature with descent below

* The heat thus brought to the surface would amount to one-seventeenth of that conveyed by conduction.

the ground.' It is doubtful whether we have yet discovered a rate that in any useful sense can be spoken of as 'average.' The widely divergent views of different authorities as to the presumed value of this rate may well lead to reflection. The late Professor Prestwich thought a rise of 1° F. for every 45 feet of descent below the zone of constant temperature best represented the average; Lord Kelvin in his earliest estimates has adopted a value of 1° F. for every 51 feet; the committee of this association appointed to investigate this question arrived at a rate of 1° F. for every 60 feet of descent; Mr. Clarence King has made calculations in which a rate of 1° F. for 72 feet is adopted; a re-investigation of recorded measurements would, I believe, lead to a rate of 1° F. in 80 or 90 feet as more closely approaching the mean. This would raise Lord Kelvin's estimate to nearly fifty millions of years.

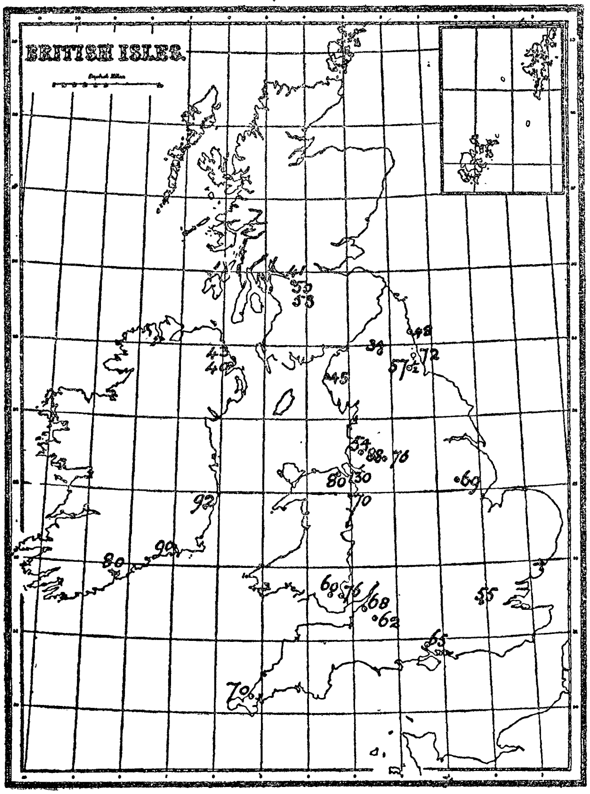

FIG. 1.—Map of the British Isles, showing the distribution of rates of increase of temperature with descent. The rates are taken from the 'British Association Report,' except in the case of those in the south of Ireland.

When from these various averages we turn to the observations on which they are based, we encounter a surprising divergence of extremes from the mean; thus in the British Isles alone the rate varies from 1° F. in 34 feet to 1° F. in 92 feet, or in one case to 1° F. in 130 feet. It has been suggested, and to some extent shown, that these irregularities may be connected with differences in conductivity of the rocks in which the observations were made, or to the circulation of underground water; but many cases exist which cannot be explained away in such a manner, but are suggestive of some deep-seated cause, such as the distribution of molten matter below the ground. Inspection of the accompanying map of the British Isles, on which the rates of increase in different localities have been plotted, will afford some evidence of the truth of this view. Comparatively low rates of increase are found over Wales and in the province of Leinster, districts of relatively great stability, the remnants of an island that have in all probability stood above the sea ever since the close of the Silurian period. To the north of this, as we enter a region which was subject to volcanic disturbances during the Tertiary period, the rate increases.

It is obvious that in any attempt to estimate the rate at which the earth is cooling as a solid body the disturbing influence of subterranean lakes of molten rock must as far as possible be eliminated; but this will not be effected by taking the accepted mean of observed rates of increase of temperature; such an average is merely a compromise, and a nearer approach to a correct result will possibly be attained by selecting some low rate of increase, provided it is based on accurate observations.

It is extremely doubtful whether an area such as the British Isles, which has so frequently been the theater of volcanic activity and other subterranean disturbance, is the best fitted to afford trustworthy results; the Archæan nucleus of a continent might be expected to afford surer indications. Unfortunately the hidden treasures of the earth are seldom buried in these regions, and bore-holes in consequence have rarely been made in them. One exception is afforded by the copper-bearing district of Lake Superior, and in one case, that of the Calumet and Hecla mine, which is 4,580 feet in depth, the rate of increase, as determined by Professor A. Agassiz, was 1° F. for every 223.7 feet. The Bohemian 'horst' is a somewhat ancient part of Europe, and in the Przibian mines, which are sunk in it, the rate was 1° F. for every 126 feet of descent. In the light of these facts it would seem that geologists are by no means compelled to accept the supposed mean rate of increase of temperature with descent into the crust as affording a safe guide to the rate of cooling of a solid globe; and if the much slower rate of increase observed in the more ancient and more stable regions of the earth has the importance which is suggested for it, then Lord Kelvin's estimate of the date of the 'consistentior status' may be pushed backwards into a remoter past.

If, as we have reason to hope, Lord Kelvin's somewhat contracted period will yield to a little stretching, Professor Joly's on the other hand, may take some paring. His argument, broadly stated, is as follows: The ocean consisted at first of fresh water; it is now salt, and its saltness is due to the dissolved matter that is constantly being carried into it by rivers. If, then, we know the quantity of salt which the rivers bring down each year into the sea, it is easy to calculate how many years they have taken to supply the sea with all the salt it at present contains. For several reasons it is found necessary to restrict attention to one only of the elements contained in sea salt: this is sodium. The quantity of sodium delivered to the sea every year by the rivers is about 160,000,000 tons; but the quantity of sodium which the sea contains is at least ninety millions of times greater than this. The periods during which rivers have been carrying sodium into the sea must, therefore, be about ninety millions of years. Nothing could be simpler; there is no serious flaw in the method, and Professor Joly's treatment of the subject is admirable in every way; but of course in calculations such as this everything depends on the accuracy of the data, which we may, therefore, proceed to discuss. Professor Joly's estimate of the amount of sodium in the ocean may be accepted as sufficiently near the truth for all practical purposes. We may, therefore, pass on to the other factor, the annual contribution of sodium by river water. Here there is more room for error. Two quantities must be ascertained: one the quantity of water which the rivers of the world carry into the sea, the other the quantity or proportion of sodium present in this water. The total volume of water discharged by rivers into the ocean is estimated by Sir John Murray as 6,524 cubic miles. The estimate being based on observations of thirty-three great rivers although only approximate, it is no doubt sufficiently exact; at all events such alternations as it is likely to undergo will not greatly affect the final result. When, however, we pass to the last quantity to be determined, the chemical composition of average river water, we find that only a very rough estimate is possible, and this is the more unfortunate because changes in this may very materially affect our conclusions. The total quantity of river water discharged into the sea is, as we have stated, 6,524 cubic miles. The average composition of this water is deduced from analyses of nineteen great rivers, which altogether discharge only 488 cubic miles, or 7.25 per cent. of the whole. The danger in using this estimate is two-fold: in the first place 7.25 is too small a fraction from which to argue to the remaining 92.75 per cent., and next, the rivers which furnish it are selected rivers, *i. e.*, they are all of large size. The effect of this is that the drainage of the volcanic regions of the earth is not sufficiently represented, and it is precisely this drainage which is richest in sodium salts. The lavas and ashes of active volcanoes rapidly disintegrate under the energetic action of various acid gases, and among volcanic exhalations sodium chloride has been especially noticed as abundant. Consequently we find that while the proportion of sodium in Professor Joly's average river water is only 5.73 per million, in the rivers of the volcanic island of Hawaii it rises to 24.5 per million (Walter Maxwell, 'Lavas and Soils of the Hawaiian Islands,' p. 170). No doubt the area occupied by volcanoes is trifling compared with the remaining land surface. On the other hand the majority of volcanoes are situated in regions of copious rainfall, of which they receive a full share owing to their mountainous form. Much of the fallen rain percolates through the porous material of the cone, and, richly charged with alkalies, finds its way by underground passages towards the sea, into which it sometimes discharges by submarine springs.

Again, several considerations lead to the belief that the supply of sodium to the ocean has proceeded, not at a uniform, but at a gradually diminishing rate. The rate of increase of temperature with descent into the crust has continuously diminished with the flow of time, and this must have had its influence on the temperature of springs, which furnish an important contribution to river water. The significance of this consideration may be judged from the composition of the water of geysers. Thus Geyser, in Iceland, contains 884 parts of sodium per million, or nearly 160 times as much as Sir John Murray estimates is present in average river water. A mean of the analyses of six geysers in different parts of the world gives 400 parts of sodium per million, existing partly as chloride, but also as sulphate and carbonate.

It should not be overlooked that the present is a calm and quiet epoch in the earth's history, following after a time of fiery activity. More than once, indeed, has the past been distinguished by unusual manifestations of volcanic energy, and these must have had some effect upon the supply of sodium to the ocean. Finally, although the existing ocean water has apparently but slight effect in corroding the rocks which form its bed, yet it certainly was not inert when its temperature was not far removed from the critical point. Water begins to exert a powerful destructive action on silicates at a temperature of 180° C., and during the interval occupied in cooling from 370 to 180° C. a considerable quantity of sodium may have entered into solution.

A review of the facts before us seems to render some reduction in Dr. Joly's estimate imperative. A precise assessment is impossible, but I should be inclined myself to take off some ten or thirty millions of years.

We may next take the evidence of the stratified rocks. Their total maximum thickness is, as we have seen, 265,000 feet, and consequently if they accumulated at the rate of one foot in a century, as evidence seems to suggest, more than twenty-six millions of years must have elapsed during their formation. W. J. Sollas.

(*To be concluded.*)