The quantity of the alternating current is determined by A; and the value of α only affects the epoch of maximum current. If we make $\rho=0$, the effect is that of closing the circuit of x, and we find

$$A^2=\frac{M^2n^2}{R^2+L^2n^2}.$$

This expression shows that the condenser has no effect when the current is closed.

If we make $\rho=\infty$, the effect is that of removing the conductor y, and thus breaking the circuit. In this case

$$A^2=\frac{M^2n^2}{R^2+\left(Ln-\frac{1}{Cn}\right)^2}.$$

This expression gives a greater value of A than when the circuit is closed, provided $2CLn^2$ is greater than unity, which may be ensured by increasing the capacity of the condenser, the self-induction of the electromagnetic coil, or the velocity of rotation.

If $CLn^2=1$, the expression is reduced to

$$A=\frac{Mn}{R}.$$

This is the greatest effect which can be produced with a given velocity, and is the same as if the current in the coil had no "electromagnetic momentum."

If the electromagnet has a secondary coil outside the primary coil so as to form an ordinary induction-coil, the intensity of the secondary current will depend essentially on that of the primary which has just been found. Although the reaction of the secondary current on the primary coil will introduce a greater complication in the mathematical expressions, the remarkable phenomenon described by Mr. Grove does not require us to enter into this calculation, as the secondary sparks observed by him are a mere indication of what takes place in the primary coil.

XLIII. *On Geological Time, and the probable Date of the Glacial and the Upper Miocene Period.* By James Croll, *of the Geological Survey of Scotland**.

It is nearly a century ago since Lagrange's determinations of the superior limits of the excentricity of the planetary orbits were published†. The results at which he arrived were

* Communicated by the Author.

† Memoirs of the Berlin Academy for 1782.

very nearly the same as those which were afterwards obtained by Leverrier.

Before the beginning of the present century the effects which the excentricity of the earth's orbit may have on climate were discussed. But about thirty-five years ago the question as to how far a change in the amount of excentricity might affect the climate of our globe was considered by Sir John Herschel; and his results were published in the Transactions of the Geological Society of London, vol. iii. (2nd series). But he does not appear at the time to have been aware of the conclusions arrived at by Lagrange regarding the superior limit of the excentricity of the earth's orbit. He came, however, to the conclusion that possibly the climate of our globe may have been affected by variations in the excentricity of its orbit. "An amount of variation," he says, "which we need not hesitate to admit (at least provisionally) as a possible one, may be productive of considerable diversity of climate, and may operate during great periods of time either to mitigate or to exaggerate the difference of winter and summer temperatures, so as to produce alternately in the same latitude of either hemisphere a perpetual spring, or the extreme vicissitudes of a burning summer and a rigorous winter."

Unfortunately, however, this opinion was to a great extent nullified by a statement which afterwards appeared in his 'Treatise on Astronomy,' to which reference will be presently made. Shortly after the appearance of Herschel's paper, the subject was investigated by Arago, Poisson, Humboldt, and other astronomers. The general conclusion arrived at, however, was that the climate of our globe could not be much affected by any change which could take place in the ellipticity of its orbit. The reason which induced astronomers to come to that conclusion seems chiefly to be this: whatever be the extent of the excentricity, the total amount of heat falling on both hemispheres must be the same. The sun, for example, is much nearer the earth when he is over the southern hemisphere than he is when over the northern; but the southern hemisphere does not on this account receive more heat than the northern; for, owing to the greater velocity of the earth when nearest the sun, the sun does not remain so long on the southern hemisphere as he does on the northern. These two effects so exactly counterbalance each other that, whatever be the extent of the excentricity, the total amount of heat reaching both hemispheres is the same. And it was considered that this beautiful compensating principle would protect the climate of our globe from being seriously affected by an increase in the excentricity of its orbit, unless the extent of that increase was very great*. Arago, for example, states that so little is the climate of our globe affected by the excentricity of its orbit, that even were the orbit to become as excentric as that of the planet Pallas (that is, as great as 0·24), "still this would not alter in any appreciable manner the mean thermometrical state of the globe."

This idea, supported by these great authorities, got possession of the public mind; and ever since it has been almost universally regarded as settled that the great changes of climate indicated by geological phenomena could not have resulted from any change in the relation of the earth to the sun.

There is, however, one effect that was not regarded as compensated. The total amount of heat received by the earth is inversely proportional to the minor axis of its orbit; and it follows, therefore, that the greater the excentricity, the greater is the total amount of heat received by the earth. On this account it was concluded that an increase of excentricity would tend to a certain extent to produce a warmer climate†.

All those conclusions to which I refer, arrived at by astronomers, are perfectly legitimate so far as the direct effects of excentricity are concerned; and it was quite natural and, in fact, proper to conclude that there was nothing in the mere increase of excentricity that could produce a glacial epoch. How unnatural and even absurd would it have been to have concluded that an increase in the quantity of heat received from the sun should lower the temperature and cover the country with snow and ice. Neither would excessively cold winters followed by excessively hot summers produce a glacial epoch. If any person had asserted that the purely astronomical causes could produce such an effect, he would certainly and deservedly have been regarded as a fool.

The important fact, however, was overlooked that, although the glacial epoch could not result directly from an increase of excentricity, it might nevertheless do so indirectly. As was stated on a former occasion, the glacial epoch was not due directly to an increase in the excentricity of the earth's orbit, but to a number of physical agents that were brought into operation

* Herschel in "Treatise of Astronomy," Cabinet Cyclopædia, § 315; Outlines of Astronomy, § 368.

Arago in the *Annuaire* for 1834, p. 199. Edinb. New Phil. Journ. for April 1834, p. 224.

Poisson in *Connaissance des Temps* for 1836, pp. 38–54.

Humboldt in *Cosmos*, vol. iv. p. 459. Physical Description of the Heavens, p. 336.

† Herschel in "Discourse on the Study of Natural Philosophy," § 140, Trans. of Geol. Soc. of London, vol. iii. p. 297 (2nd series).

Lyell in 'Principles of Geology,' p. 126, 7th edit.

Professor Haughton in Phil. Mag. for May 1866.

as a result of an increase. Until those physical circumstances were discovered it was impossible that the true cause of the glacial epoch could be known. Many of the indirect and physical effects, which in reality were those that brought about the glacial epoch, could not, from the nature of things, have been known previously to recent discoveries in the science of heat. When the excentricity is about its superior limit, the combined effect of all those causes to which I allude is to lower to a very great extent the temperature of the hemisphere whose winters occur in aphelion, and to raise to nearly as great an extent the temperature of the opposite hemisphere, where winter of course occurs in perihelion. I have made these remarks in order to obviate certain objections to which I shall afterwards have occasion to refer.

Astronomy and physics not only afford a cause for those abnormal conditions of climate during geological epochs, but they seem to afford also (at least so far as regards very recent epochs) a probable means of arriving at a pretty accurate determination of the date at which those conditions prevailed.

On examining the Tables of excentricity given in former papers* for a million of years back, it will be seen that there are two periods of great duration during which the excentricity continued at a high value. The one period extended from about 980,000 to about 720,000 years ago, and the other period began about 240,000 years ago and extended down to about 80,000 years ago. At first I felt disposed to refer the glacial epoch (the time of the true boulder-clay) to the former period; and the latter period, I was inclined to believe, must have corresponded to the time of local glaciers towards the close of the glacial epoch, the evidence of which, in the shape of moraines, is to be found in almost every one of our highland glens.

There was, however, one formidable objection to this view of the matter which presented itself to my mind at the time. I found, from calculations based on the amount of sediment carried into the Gulf of Mexico by the Mississippi River, that the North American continent is being lowered by subaërial denudation at the rate of $\frac{1}{1388}$ of a foot per annum, and that, consequently, if the rate of denudation be as great in this country as in America, which is by no means improbable, then about 500 feet must have been removed off the face of the country and carried by our rivers into the sea since the period of the boulder-clay, if that period is to be placed 700,000 years back†. It would therefore follow that the general features of the country must now be totally different from what they were at the close of the

* Phil. Mag. for January 1866 and February 1867.

† Phil. Mag. for February 1867, p. 130.

glacial epoch, a conclusion which we know from geological evidence is incorrect. Influenced by these considerations, I expressed the opinion that it might yet turn out that between 240,000 and 80,000 years ago was the period of the glacial epoch, and that that epoch of glaciation about 850,000 years ago might be that of the Upper Miocene period.

There are physical reasons of great weight against the opinion that the glacial epoch was so remote as 850,000 years ago. If we place the middle of the glacial epoch 850,000 years back, then we must lengthen out to a corresponding extent the entire geological history of our globe. Sir Charles Lyell considers that when we go back as far as the Lower Miocene formations, we arrive at a period when the marine shells differed as a whole from those now existing. But only 5 per cent. of the shells existing at the commencement of the glacial epoch have since died out. Hence, assuming the rate at which the species change to be uniform, it follows that the Lower Miocene period must be twenty times as remote as the commencement of the glacial epoch. Consequently, if it be 1 million of years since the commencement of the glacial epoch, 20 millions of years, Sir Charles concludes, must have elapsed since the time of the Lower Miocene period, and 60 millions of years since the begininng of the Eocene period, and about 160 millions of years since the Carboniferous period, and about 240 millions of years must be the time which has elapsed since the beginning of the Cambrian period. But, on the other hand, if we refer the glacial epoch to the later period of great excentricity, and take 250,000 years ago as the beginning of that period, then, according to the same mode of calculation, we have 15 millions of years since the beginning of the Eocene period, and 40 millions of years since the Carboniferous period, and 60 millions of years in all since the beginning of the Cambrian period.

A great many considerations seem to show that 850,000 years cannot possibly have elapsed since the glacial epoch, and that we must assign that epoch to the period commencing about 240,000 years ago and extending down to about 80,000 years ago. If the glacial epoch be placed at so remote a period as 850,000 years ago, then it is very probable, as Sir Charles Lyell concludes, that the beginning of the Cambrian period will require to be placed 240 millions of years back. But we have evidence of a physical nature which proves that it is absolutely impossible that the existing order of things, as regards our globe, can date so far back as anything like 240 millions of years. The arguments to which I refer are those which have been advanced by Professor Sir William Thomson at various times. These arguments are well known, and to all who have really given due attention to them must be felt to be conclusive. It would be superfluous to state them here; I shall, however, for reasons which will presently appear, refer briefly to one of them, and the one which seems to be the most conclusive of all, viz. the argument derived from the limit to the age of the sun's heat.

It is found that 83·4 foot-pounds of heat per second is incident upon a square foot of the earth's surface exposed to the perpendicular rays of the sun. The amount radiated from a square foot of the sun's surface to that incident on a square foot of the earth's surface is as the square of the sun's distance to the square of his radius, or as 46,400 to 1. Consequently 3,869,000 foot-pounds of heat is radiated off every square foot of the sun's surface per second—an amount equal to about 7000 horse-power. The total amount radiated from the whole surface of the sun per annum amounts to 8340×10^{30} foot-pounds. To maintain the present rate of radiation, it would require the combustion of about 1500 lbs. of coal per hour on every square foot of the sun's surface; and were the sun composed of that material, it would be all consumed in less than 5000 years. The opinion that the sun's heat is maintained by combustion cannot be entertained for a single moment. A pound of coal falling into the sun from an infinite distance would produce by its concussion more than 6000 times the amount of heat that would be generated by its combustion.

It is well known that the velocity with which a body falling from an infinite distance would reach the sun would be equal to that which would be generated by a constant force equal to the weight of the body at the sun's surface operating through a space equal to the sun's radius. One pound would at the sun's surface weigh about 28 pounds. Take the sun's radius at 441,000 miles*. The energy of a pound of matter falling into the sun from infinite space would equal that of a 28-pound weight descending upon the earth from an elevation of 441,000 miles, supposing the force of gravity to be as great at that elevation as it is at the earth's surface. It amounts to upwards of 65,000,000,000 foot-pounds. A better idea of this enormous amount of energy exerted by a one-pound weight falling into the sun will be conveyed by stating that it would be sufficient to raise 1000 tons to a height of $5\frac{1}{2}$ miles. It would project the 'Warrior,' fully equipped with guns, stores, and ammunition, over the top of Ben Nevis.

Gravitation is now generally admitted to be the only conceivable source of the sun's heat. But if we attribute the energy of the sun to gravitation as a source, we attribute it to a cause

* I have taken for the volume and mass of the sun the values given in Professor Sir William Thomson's paper, Phil. Mag. vol. viii. (1854).

the value of which can be accurately determined. Prodigious as is the energy of a single pound of matter falling into the sun, nevertheless a range of mountains, consisting of 176 cubic miles of solid rock, falling into the sun would only maintain his heat for a single second. A mass equal to that of the earth would maintain the heat for only 93 years, and a mass equal to that of the sun itself falling into the sun would only afford 33,000,000 years' sun-heat.

Suppose, with Helmholtz, that the sun originally existed as a nebulous mass, filling the entire space presently occupied by the solar system and extending into space indefinitely beyond the outermost planet. The total amount of work in foot-pounds performed by gravitation in the condensation of this mass to an orb of the sun's present size can be found by means of the following formula given by Helmholtz *,

$$\text{Work of condensation} = \frac{3}{5} \cdot \frac{r^2 M^2}{Rm};$$

M is the mass of the sun, m the mass of the earth, R the sun's radius, and r the earth's radius. Taking $M = 4230 \times 10^{27}$ lbs., $m = 11{,}920 \times 10^{21}$ lbs., $R = 2{,}328{,}500{,}000$ feet, and $r = 20{,}889{,}272$ feet; we have then for the total amount of work performed by gravitation in foot-pounds,

$$\text{Work} = \frac{3}{5} \cdot \frac{(20{,}889{,}272 \cdot 5)^2 \times (4230 \times 10^{27})^2}{2{,}328{,}500{,}000 \times 11{,}920 \times 10^{21}}$$
$$= 168{,}790 \times 10^{36} \text{ foot-pounds.}$$

The amount of heat thus produced by gravitation would suffice for nearly 20,237,500 years.

These calculations are based upon the assumption that the density of the sun is uniform throughout. But it is highly probable that the sun's density increases towards the centre. In this case the amount of work performed by gravitation would be somewhat more than the above.

Must we have a greater amount of heat than what could have resulted from gravitation? If so, then what other possible source of energy can there be? There is still another possible source. The foregoing calculation in regard to the total amount of heat radiated from the sun is made upon the assumption that the matter composing the sun, when it existed in space as a nebulous mass, was not originally possessed of temperature, but that the temperature was given to it as the mass became condensed under the force of gravitation. We have supposed the heat given out to be simply the heat of condensation. But it

* Phil. Mag. S. 4. vol. xi. p. 516 (1856).

is quite conceivable that this nebulous mass might have been possessed of an original store of heat previous to condensation. It is quite possible that the very reason why it existed in such a rarefied or gaseous condition was its excessive temperature, and that condensation only began to take place as the mass began to cool down. It seems far more probable that this should have been the case than that the mass existed in so rarefied a condition without temperature. For why should the particles have existed in this separated form when devoid of the repulsive energy of heat, seeing that in virtue of gravitation they had such a tendency to approach to one another? But if the mass was originally in a heated condition, then in condensing it would have to part not only with the heat generated in condensing, but also with the heat which it originally possessed, a quantity which would no doubt much exceed that produced by condensation. To illustrate this principle, let us suppose a pound of air, for example, to be placed in a cylinder and heat applied to it. If the piston be so fixed that it cannot move, 234·5 foot-pounds of heat will raise the temperature of the air 1° C. But if the piston be allowed to rise as the heat is applied, then it will require 330·2 foot-pounds of heat to raise the temperature 1° C. It requires 95·7 foot-pounds more heat in the latter case than in the former. The same amount of energy, viz. 234·5 foot-pounds, in both cases goes to produce temperature; but in the latter case, where the piston is allowed to move, 95·7 foot-pounds of additional heat is consumed in the mechanical work of raising the piston. Suppose, now, that the air is allowed to cool under the same conditions: in the one case 234·5 foot-pounds of heat will be given out while the temperature of the air sinks 1° C.; in the other case, where the piston is allowed to descend, 330·2 foot-pounds will be given out while the temperature sinks 1° C. In the former case, the air in cooling has simply to part with the energy which it possesses in the form of temperature; but in the latter case it has, in addition to this, to part with the energy bestowed upon its molecules by the descending piston. While the temperature of the gas is sinking 1°, 95·7 foot-pounds of energy in the form of heat is being imparted to it by the descending piston; and this has to be got rid of before the temperature is lowered by 1°. Consequently 234·5 foot-pounds of the heat given out previously existed in the air under the form of temperature, and the remaining 95·7 foot-pounds given out was imparted to the air by the descending piston while the gas was losing its temperature. 234·5 foot-pounds is the energy or heat which the air previously possessed, and 95·7 is the energy or heat of condensation.

In the case of the cooling of the sun from a nebulous mass, there would of course be no external force or pressure exerted on the mass analogous to that of the piston on the air; but there would be, what is equivalent to the same, the gravitation of the particles to each other. There would be the pressure of the whole mass towards the centre of convergence. In the case of air, and all perfect gases cooling under pressure, about 234 foot-pounds of the original heat possessed by the gas is given out while 95 foot-pounds is generated by condensation. We have, however, no reason whatever to believe that in the case of the cooling of the sun the same proportions would hold true. The proportion of original heat possessed by the mass of the sun to that produced by condensation may have been much greater than 234 to 95, or it may have been much less. In the absence of all knowledge on this point, we may in the meantime assume that to be the proportion. The total quantity of heat given out by the sun resulting from the condensation of his mass, on the supposition that the density of the sun is uniform throughout, we have seen to be equal to 20,237,500 years' sun-heat. Then the quantity of heat given out, which previously existed in the mass as original temperature, must have been 49,850,000 years' heat. In all, 70,087,500 years' heat as the total amount.

The above quantity represents, of course, the total amount of heat given out by the mass since it began to condense. But the geological history of our globe must date its beginning to a period posterior to that. For at that time the mass would probably occupy a much greater amount of space than is presently possessed by the entire solar system; and consequently, before it had cooled down to within the limits of the earth's present orbit, our earth could not have had an existence as a separate planet. Previously to that time it must have existed as a portion of the sun's fiery mass. If we assume that it existed as a globe previously to that, and came in from space after the condensation of the sun, then it is difficult to conceive how its orbit should be so nearly circular as it is at present.

Let us assume that by the time that the mass of the sun had condensed to within the space encircled by the orbit of the planet Mercury (that is, to a sphere having, say, a radius of 18,000,000 miles) the earth's crust began to form; and let this be the time when the geological history of our globe dates its commencement. The total amount of heat generated by the condensation of the sun's mass from a sphere of this size to its present volume would equal 19,740,000 years' sun-heat. The amount of original heat given out during that time would equal 48,625,000 years' sun-heat,—thus giving a total of 68,365,000 years' sun-heat enjoyed by our globe since that period. The total quantity may possibly, of course, be considerably more than that, owing to the fact that the sun's density may increase greatly towards his centre. But we should require to make extravagant assumptions regarding the interior density of the sun and the proportion of original heat to that produced by condensation before we could manage to account for anything like the period that geological phenomena are supposed by some to demand.

There seems to be an impression on the mind of a great many geologists, that, notwithstanding all that has been advanced regarding the limitation to the age of the sun's heat, there may yet be found some possible way of accounting for a duration of sun-heat equal to a few million of centuries.

If the sun has really given out some two or three hundred million years' heat, one of three things must follow: (1) the sun must have been in possession of that enormous store of energy prior to the commencement of the geological history of our globe; or (2) he must have received it after the commencement; or (3) a part must have been originally possessed by the sun prior to the commencement, and the rest derived afterwards. We must admit that one or other of these three suppositions is true; for these exhaust the whole field of the possible. But if the sun had originally possessed the amount of energy supposed, then his volume would have extended beyond our earth's orbit, and, of course, our earth could not at that time have existed as a separate planet. That amount of energy could not have been imparted to the sun since the commencement of our globe's geological history. It could not under any conditions have been communicated by chemical agency, nor by any means conceivable except by that of meteors or other bodies falling into the sun with enormous velocity. And if we suppose gravitation to be the agent that gave these bodies their velocity, then it is found by calculation that gravitation falls far short of affording the necessary amount of velocity. If we adopt the third supposition, that part of the energy was originally possessed by the sun, and part imparted, this will not remove the difficulty; for as the energy imparted by gravitation could be only but a small fraction of the amount required, the hundreds of millions of years' heat demanded, with the exception of that portion imparted, must have been in the possession of the sun at the commencement of our globe's geological history. But the sun cannot, as we have seen, have been a sphere with the earth revolving round it as a planet if it was in possession of such a store of energy; for its volume would have more than filled the entire space encircled by the earth's orbit.

It is quite possible, however, that a meteor may reach the sun with a velocity far greater than that which it could acquire by gravitation; for it might have been moving in a direct line towards the sun with an original velocity before coming under the sensible influence of the sun's attraction. In this case a greater amount of heat would be generated by the meteor than would have resulted from its merely falling into the sun under the influence of gravitation. But then meteors of this sort must be of rare occurrence. And we have but very little warrant on this ground to conclude that the amount of energy communicated to the sun since the geological history of our globe began could have been much more than the equivalent of the work performed by gravitation in the condensation of his mass.

It is highly probable, as Professor Sir William Thomson has concluded, that the sun in the early geological periods must have been far hotter than at present, owing to his excessively high temperature. When the sun's mass was in an intensely heated condition, filling perhaps the entire sphere occupied by the planetary system, it would no doubt be in a gaseous state, and of excessively small density. Gases are known to be bad radiators; and it is probable that a gaseous mass of such rarity would radiate its heat into space with some difficulty; and this might tend in a great measure to lessen the excessive rate of radiation which would otherwise result from so prodigious a temperature.

The question naturally suggests itself, how could the sun's mass have been originally raised to such a high temperature as we have assumed? What power could raise the temperature of the sun's mass to such an extent as to cause it to become an incandescent gas of such rarity? By what means could this mass become possessed of 50,000,000 years' heat, as we have concluded, even before it began to condense? There is nothing at all absurd or improbable in the supposition that such an amount of energy might have been communicated to the mass. The Dynamical Theory of Heat affords an easy explanation of at least *how* such an amount of energy *may* have been communicated. Two bodies, each one-half the mass of the sun, moving directly towards each other with a velocity of 476 miles per second, would by their concussion generate in a *single moment* the 50,000,000 years' heat. For two bodies of that mass moving with a velocity of 476 miles per second would possess 4149×10^{38} foot-pounds of energy in the form of *vis viva*; and this, converted into heat by the stoppage of their motion, would give an amount of heat which would cover the present rate of the sun's radiation for a period of 50,000,000 years.

Why may not the sun have been composed of two such bodies? And why may not the original store of heat possessed by him have all been derived from the concussion of these two bodies? Two such bodies coming into collision with that velocity would be dissipated into vapour by such an inconceivable amount of heat as would thus be generated; and when they condensed on cooling, they would form one spherical mass like the sun. It is perfectly true that two such bodies could never attain the required amount of velocity by their mutual gravitation towards each other. But there is no necessity whatever for supposing that their velocities were derived from their mutual attraction alone. They might have been approaching towards each other with the required velocity wholly independent of gravitation.

We know nothing whatever regarding the absolute motion of bodies in space. And beyond the limited sphere of our observation, we know nothing regarding even their relative motions. There may be bodies moving in relation to our system with inconceivable velocity. For anything that we know to the contrary, were one of these bodies to strike our earth, the shock might be sufficient to generate an amount of heat that would dissipate the earth into vapour, though the striking body might not be heavier than a cannon-ball. There is, however, nothing very extraordinary in the velocity which we have found would be required in the two supposed bodies to generate the 50,000,000 years' heat. A comet, having an orbit extending to the path of the planet Neptune, approaching so near the sun as to almost graze his surface in passing, would have a velocity of about 390 miles per second, which is within 86 miles of the required velocity.

But in the original heating and expansion of the sun into a gaseous mass, an amount of work must have been performed against gravitation equal to that which has been performed by gravitation during his cooling and condensation, a quantity which we have found amounts to about 20,000,000 years' heat. The total amount of energy originally communicated by the concussion must have been equal to 70,000,000 years' sun-heat. A velocity of 563 miles per second would give this amount. All, however, that is intended by this hypothesis is merely to show how easy it is to explain how the sun may originally have become an incandescent gas filling the entire space occupied by the planetary system.

But is it the case that geology really requires such enormous periods as is generally supposed? At present, geological estimates of time are little else than mere conjectures. Geological science has hitherto afforded no trustworthy means of estimating the positive length of geological epochs. Geological phenomena tell us most emphatically that these periods must be long; but how long, these phenomena have, as yet, failed to inform us. Geological phenomena represent time to the mind under a most striking and imposing form. They present to the eye, as it were, a sensuous representation of time; the mind thus becomes deeply impressed with a sense of immense duration; and when one under these feelings is called upon to put down in figures what he believes will represent that duration, he is very apt to be deceived. If, for example, a million of years as represented by geological phenomena and a million of years as represented by figures were placed before our eyes, we should certainly feel startled. We should probably feel that a unit with six ciphers after it was really something far more formidable than we had hitherto supposed it to be. Could we stand upon the edge of a gorge a mile and a half in depth that had been cut out of the solid rock by a tiny stream, scarcely visible at the bottom of this fearful abyss, and were we informed that this little streamlet was able to wear off annually only $\frac{1}{10}$ of an inch from its rocky bed, what would our conceptions be of the prodigious length of time that this stream must have taken to excavate the gorge? We should certainly feel startled when, on making the necessary calculations, we found that the stream had performed this enormous amount of work in something less than a million of years.

If we could possibly form some adequate conception of a period so prodigious as one hundred millions of years, we should not then feel so dissatisfied at being told that the age of the earth's crust is not greater than that.

Here is one way of conveying to the mind some idea of what a million of years really is. Take a narrow strip of paper an inch broad, or more, and 83 feet 4 inches in length, and stretch it along the wall of a large hall, or round the walls of an apartment somewhat over 20 feet square. Recall to memory the days of your boyhood, so as to get some adequate conception of what a period of a hundred years is. Then mark off from one of the ends of the strip $\frac{1}{10}$ of an inch. The $\frac{1}{10}$ of the inch will then represent one hundred years, and the entire length of the strip a million of years. It is well worth making the experiment, just in order to feel the striking impression that it produces on the mind.

The methods which have been adopted in estimating geological time not only fail to give us the positive length of geological periods, but some of them are actually calculated to mislead. The method of calculating the length of a period from the thickness of the stratified rocks belonging to that period can give no reliable estimate; for the thickness of the deposit will depend upon a great many circumstances, such as whether the deposition took place near to land or far away in the deep recesses of the ocean, whether it took place at the mouth of a great river or along the sea-shore, whether it took place when the sea-bottom was rising, subsiding, or remaining stationary.

Stratified formations 10,000 feet in thickness, for example, may, under some conditions, have been formed in as many years, while under other conditions it may have required as many centuries. Nothing whatever can be safely inferred as to the absolute length of a period from the thickness of the stratified formations belonging to that period. Neither will this method give us a trustworthy estimate of the *relative* lengths of geological periods. Suppose we find the average thickness of the Cambrian rocks to be 26,000 feet, the Silurian to be 28,000 feet, the Devonian to be 6000 feet, and the Tertiary to be 10,000 feet, it would not be safe to assume, as is sometimes done, that the relative duration of those periods must have corresponded to these numbers. Were we sure that we had got the correct average thickness of all the rocks belonging to each of those formations, we might probably be able to arrive at the relative lengths of those periods; but we can never be sure of this. Those formations all, at one time, formed sea-bottoms; and we can only measure those deposits that are now raised above the sea-level. But is it not probable that the relative positions of sea and land during the Cambrian, Silurian, Old-Red-Sandstone, Carboniferous, and other early periods of the earth's history differed more from the present relative positions than the relative positions of sea and land during the Tertiary period differed from the relative positions which obtain at present? May not the greater portion of the Tertiary deposits be still under the sea-bottom? And if this be the case, it may yet be found at some day in the distant future, when these deposits are elevated into dry land, that they are much thicker than we now conclude them to be. Of course it is by no means asserted that they are thicker than we conclude them to be. It is simply asserted that they *may* be thicker for anything that we know to the contrary; and the possibility that they may, destroys our confidence in the accuracy of this method of determining the relative lengths of geological periods.

The palæontological method of estimating geological time, either absolute or relative, from the rate at which species change appears to be even still more unsatisfactory. If we could ascertain by some means or other the time that has elapsed from some given epoch (say, for example, the glacial) till the present day, and were we sure at the same time that species have changed at a uniform rate during all past ages, then, by ascertaining the percentage of change that has taken place since the glacial epoch, we should have a means of making something like a rough estimate of the length of the various periods. But without some such period to start with, the palæontological method is useless. It will not do to take the historic period as a base-line.

It is far too short to be used with safety in determining the distance of periods so remote as those which concern the geologist. But even supposing the palæontologist had a period of sufficient length measured off correctly to begin with, his results would still be unsatisfactory; for it is perfectly obvious, that unless the climatic conditions of the globe during the various periods were nearly the same, the rate at which the species change would certainly not be uniform. But we have evidence, geological as well as cosmical, that the climate of our globe has at various periods undergone changes of the most excessive character.

The palæontological method, as we have already seen, will give 60 millions of years or 240 millions of years as the period that has elapsed since the commencement of the Cambrian period, just as we choose to adopt 250,000 years ago or 1,000,000 years ago as the commencement of the glacial epoch.

It is the modern and philosophic doctrine of uniformity that has chiefly led geologists to overestimate the length of geological periods. This philosophic school teaches, and that truly, that the great changes undergone by the earth's crust must have been produced not by great convulsions and cataclysms of nature, but by those ordinary agencies that we see at work every day around us, such as rain, snow, frost, ice, and chemical action, &c. It teaches that the valleys were not produced by violent dislocations, nor the hills by sudden upheavals, but that they were actually carved out of the solid rock by the silent and gentle agency of chemical action, frost, rain, ice, and running water. It teaches, in short, that the rocky face of our globe has been carved into hill and dale, and ultimately worn down to the sea-level, by means of these apparently trifling agents, not only once or twice, but probably dozens of times over during past ages. Now, when we reflect that with such extreme slowness do these agents perform their work, that we might watch their operations from year to year, and from century to century, if we could, without being able to perceive that they make any very sensible advance, we are necessitated to conclude that geological periods must be enormous. And the conclusion at which we thus arrive is undoubtedly correct. It is, in fact, impossible to form an adequate conception of the length of geological time. It is something too vast to be fully grasped by our conceptions. What those to whom we have been alluding err in is not in forming too great a conception of the extent of geological periods, but in the way in which they represent the length of these periods in numbers. When we speak of units, tens, hundreds, thousands, we can form some notion of what these quantities represent; but when we come to millions, tens of millions, hundreds of millions, thousands of millions, the mind is then totally unable to follow, and we can only use these numbers as representations of quantities that turn up in calculation. We know, from the way in which they do turn up in our process of calculation, whether they are correct representations of things in actual nature or not; but we could not, from a mere comparison of these quantities with the thing represented by them, say whether they were actually too small or too great. It is here that some geologists have erred: they have not made the necessary calculations, and found by the known rules of arithmetic that 100,000,000 is too small a number to represent in years the probable age of the earth's crust; but they look first at the phenomena and then at the figures; and as the two produce totally different impressions, they pronounce the figures to be too small to represent the phenomena.

If the geologist could find a method of ascertaining the actual rate at which these denuding agents do perform their work; if it could be ascertained at what rate the face of the country is at present being denuded, how much, for example, per annum the general level of the country is being lowered and the valleys deepened, then we should have a means of ascertaining whether or not the agents to which we refer were really capable of producing the required amount of change in the earth's surface in the allotted time. But mere conjectures in the absence of some positive determinations are worse than useless.

But happily there is a method of ascertaining, with the most perfect accuracy, the rate at which the face of the globe is being denuded by subaërial agency. And it is somewhat remarkable that this method has been so long overlooked by geologists. The method to which I allude is that which has already been incidentally referred to, viz. that of determining the amount of solid materials which is being carried down annually by our rivers into the sea. Were it ascertained (and this might be easily done) how much sediment is being carried down by our rivers into the sea, then we should be able to determine exactly the extent to which the area of drainage of those rivers was being lowered annually by subaërial denudation; for the material carried down by those rivers must all be derived from the surface of the country drained by them. When I published the result of my calculations, from the amount of sediment carried down by the Mississippi, regarding the rate at which the North-American continent is being lowered by denudation, I was not at the time aware that Mr. Alfred Tylor had arrived at somewhat similar results by the self-same method nearly fifteen years ago*. His object was to show that the relative level of the

* Phil. Mag. S. 4. vol. v. (1853).

sea is being affected by the transference of sediment from the land to the sea. Such a result, however, is very doubtful; for it is quite possible that it may be more than neutralized by upheavals of the land.

The amount of sediment carried down into the Gulf of Mexico by the Mississippi River has been estimated with the greatest accuracy by Messrs. Humphreys and Abbot. It is found that the average amount of sediment held in suspension in the waters of the Mississippi is about $\frac{1}{1500}$ of the weight of the water, or $\frac{1}{2900}$ by bulk. The annual discharge of the river is 19,500,000,000,000 cubic feet of water. The quantity of sediment carried down into the Gulf of Mexico amounts to 6,724,000,000 cubic feet. But besides that which is held in suspension, the river pushes down into the sea about 750,000,000 cubic feet of earthy matter, making in all a total of 7,474,000,000 cubic feet transferred from the land to the sea annually. Where does this enormous mass of material come from? Unquestionably it comes from the ground drained by the Mississippi. The area drained by the river is 1,244,000 square miles. Now 7,474,000,000 cubic feet removed off 1,224,000 square miles of surface is equal to $\frac{1}{4566}$ of a foot off that surface per annum, or one foot in 4566 years. The specific gravity of the sediment is taken at 1·9, that of rock is about 2·5; consequently the amount removed is equal to one foot of rock in about 6000 years. The average height of the North-American continent above the sea-level, according to Humboldt, is 748 feet; consequently, at the present rate of denudation, the whole area of drainage will be brought down to the sea-level in less than 4,500,000 years* if no elevation of the land takes place.

The rate of denudation of the area drained by the river Ganges is much greater than this. The annual discharge of that river is 6,523,000,000,000 cubic feet of water. The sediment held in suspension is equal $\frac{1}{510}$ by weight; area of drainage 432,480 square miles. This gives 1 foot of rock in 2358 years as the amount removed.

Rough estimates have been made of the amount of sediment carried down by some eight or ten European rivers; and although those estimates cannot be depended upon as being anything like perfectly accurate, still they show (what there is very little reason to doubt) that it is extremely probable that the European continent is being denuded about as rapidly as the American. By means of subaërial agencies continents are cut up into islands,

* My former estimate was incorrect. It was derived from imperfect data obtained previously to the observations of Humphreys and Abbot. And, besides, I omitted to take into account the difference between the specific gravity of sediment and rock.

the islands into smaller islands, and so on till the whole ultimately disappears.

There can be no doubt about the rate at which the American continent is being denuded by subaërial agency. The sediment carried down into the Gulf of Mexico assuredly comes all off the land. It is not derived from the banks of the river itself, as has been clearly shown by Mr. Tylor. "The Mississippi," he says, "must draw its vast supplies of mud from its tributaries; for it could obtain them from no other source, unless we suppose it abstracts them from its own plains. Certainly in many places soil is being removed from one part or other of its plains; but an equal quantity must be added to some other part; for the river could not make a permanent inroad into its plains without enlarging its channel. This it does not do, or it would be able to carry off the winter freshets without overflowing, and the present artificial bank would be unnecessary."

Every river running through an alluvial plain will cut a channel for itself of a definite capacity, which capacity will be determined by the volume and velocity of the river. If you attempt to increase the size of the channel, it will silt up and assume its former capacity. Or if you attempt to diminish its channel by throwing in loose materials, the river will remove these. We have a good example in the river Clyde of the tendency of a river to preserve its normal size of channel. It is necessary for commercial purposes that the channel of the river below the city of Glasgow should be kept much deeper than the volume and velocity of the river necessarily demands, and the consequence is that it requires the continued efforts of several powerful dredging-machines to counteract the tendency that this little river has to silt up to its normal depth and size of channel.

So long as the present order of things remains, the rate of denudation will continue while land remains above the sea-level; and we have no warrant for supposing that the rate was during past ages less than it is at the present day. It will not do as an objection to say that, as a considerable amount of the sediment carried down by rivers is boulder-clay and other materials belonging to the ice-age, the total amount removed by the rivers is on that account greater than it would otherwise be. Were this objection true, it would follow that, prior to the glacial period, when it is assumed that there was no boulder-clay, the face of the country must have consisted of bare rock; for in this case no soil could have accumulated from the disintegration and decomposition of the rocks, *since, unless the rocks of a country disintegrate more rapidly than the river-systems are able to carry the disintegrated materials to the sea, no surface-soil can form on that country.* The rate at which rivers carry down sediment is evidently not determined by the rate at which the rocks are disintegrated and decomposed, but by the quantity of rain falling, and the velocity with which it moves off the face of the country. Every river-system possesses a definite amount of carrying-power, depending upon the slope of the ground, the quantity of rain falling per annum, the manner in which the rain falls, whether it falls gradually or in torrents, and a few other circumstances. When it so happens, as it generally does, that the amount of rock disintegrated on the face of the country is greater than the carrying-power of the river-systems can remove, then a soil necessarily forms. But when the reverse is the case no soil can form on that country, and it will present nothing but barren rock. This is no doubt the reason why in places like the Island of Skye, for example, where the rocks are exceedingly hard and difficult to decompose and separate, the ground steep, and the quantity of rain falling very great, there is so much bare rock to be seen. If, prior to the glacial epoch, the rocks of the area drained by the Mississippi would not produce annually more material from their destruction under atmospheric agency than was being carried down by that river, then it follows that the country must have presented nothing but bare rock, if the amount of rain falling was then as great as at present.

No proper estimate has been made of the quantity of sediment carried down into the sea by our British rivers. But, from the principles just stated, we are warranted to infer that it must be as great in proportion to the area of drainage as that carried down by the Mississippi. For example, the river Tay, which drains a great portion of the central Highlands of Scotland, carries to the sea three times as much water in proportion to its area of drainage as is carried by the Mississippi. And any one who has seen this rapidly running river during a flood, red and turbid with sediment, will easily be convinced that the quantity of solid material carried down by it into the German Ocean must be very great. Mr. John Dougall has found that the waters of the Clyde during a flood hold in suspension $\frac{1}{800}$ by bulk of sediment. The observations were made about a mile above the city of Glasgow. But even supposing (what is certainly an under-estimate) the amount of sediment held in suspension by the waters of the Tay to be only one-third of that of the Mississippi, viz. $\frac{1}{4500}$ by weight, still this would give the rate of denudation of the central Highlands at 1 foot in 6000 years, or 1000 feet in 6 millions of years*.

But, after all, one foot removed off the general level of the

* See a valuable paper by Mr. Archibald Geikie on "Denudation as a measure of Geological Time," which will shortly be published in the Transactions of the Glasgow Geological Society.

country since the creation of man, according to Mosaic chronology, is certainly not a very great quantity. No person but one who had some preconceived opinions to maintain would ever think of concluding that one foot of soil during 6000 years was an extravagant quantity to be washed off the face of the country by rain and floods during that long period. Those who reside in the country and are eye-witnesses of the actual effects of heavy rains upon the soil, our soft country roads, ditches, burns, and rivers, will have considerable difficulty in actually believing that only one foot has been washed away during the past 6000 years.

Some may probably admit that a foot of soil may be washed off during a period so long as 6000 years, and may tell us that what they deny is not that a foot of loose and soft soil, but a foot of solid rock can be washed away during that period. But a moment's reflexion must convince them that, unless the rocks of the country were disintegrating and decomposing as rapidly into soil as the rain is carrying the soil away, the surface of the country would ultimately become bare rock. It is true that the surface of our country in many places is protected by a thick covering of boulder-clay; but when this has once been removed, the rocks will then disintegrate far more rapidly than they are doing at present.

But slow as is the rate at which the country is being denuded, yet when we take into consideration a period so enormous as 6 millions of years, we find that the results of denudation are really startling. One thousand feet of solid rock during that period would be removed from off the face of the country. But if the mean level of the country would be lowered 1000 feet in 6 millions of years, how much would our valleys and glens be deepened during that period? This is a problem well worthy of the consideration of those who treat with ridicule the idea that the general features of our country have been carved out by subaërial agency.

In consequence of the retardation of the earth's rotation, occasioned by the friction of the tidal-wave, the sea-level must be slowly sinking at the equator and rising at the poles. But it is probable that the land at the equator is being lowered by denudation as rapidly as the sea-level is sinking. Nearly one mile must have been worn off the equator during the past 12 millions of years, if the rate of denudation all along the equator be equal to that of the basin of the Ganges.

But if the rate of denudation be at present so great, what must it have been during the glacial epoch? It must have been something enormous. At present, denudation is greatly retarded by the limited power of our river-systems to remove the loose materials resulting from the destruction of the rocks. These materials accumulate and form a thick soil over the surface of the rocks, which protects them, to a great extent, from the weathering effects of atmospheric agents. So long as the amount of rock disintegrated exceeds that which is being removed by the river-systems, the soil will continue to accumulate till the amount of rock destroyed per annum is brought to equal that which is being removed. It therefore follows from this principle that the *carrying-power of our river-systems is the true measure of denudation.* But during the glacial epoch the thickness of the soil would have but little effect in diminishing the waste of the rocks; for at that period the rocks were not decomposed by atmospheric agency, but were ground down by the mechanical friction of the ice. But the presence of a thick soil at this period, instead of retarding the rate of denudation, would tend to increase it tenfold, for the soil would then be used as grinding-material for the ice-sheet. In places where the ice was, say, 2000 feet in thickness, the soil would be forced along over the rocky face of the country, exerting a pressure on the rocks equal to 50 tons on the square foot.

It is true that the rate at which many kinds of rocks decompose and disintegrate is far less than what has been concluded is the mean rate of denudation of the whole country. This is evident from the fact which has been adduced by some writers, that inscriptions on stones which have been exposed to atmospheric agency for a period of 2000 years, or so, have not been obliterated. But in most cases epitaphs on monuments and tombstones, and inscriptions on the walls of buildings, 200 years old, can hardly be read. And this is not all: the stone on which the letters were cut has during that time been rotted in probably to the depth of several inches; and during the course of a few centuries more the whole mass will crumble into dust.

The facts which we have been considering show also how trifling is the amount of denudation effected by the sea in comparison with that by subaërial agents. The entire sea-coast of the globe, according to Mr. Keith Johnston, is 116,531 miles. Suppose we take the average height of the coast-line at 25 feet, and take also the rate at which the sea is advancing on the land at 1 foot in 100 years, then this gives 15,382,500,000 cubic feet of rock as the total amount removed in 100 years by the action of the sea. The total amount of land is 57,600,000 square miles, or 1,605,750,000,000,000 square feet; and if 1 foot is removed off the surface in 6000 years, then 26,763,000,000,000 cubic feet is removed by subaërial agency in 100 years, or about 1740 times as much as that removed by the sea. Before the sea could denude the globe as rapidly as the subaërial agents, it would have to advance on the land at the rate of upwards of 17 feet annually.

[To be continued.]

XLIV. *On the Specific Magnetism of Iron.* *By* PLINY EARLE CHASE*.

IN my communication on the numerical relations of gravity and magnetism †, after adducing various evidences of a correlation that had been long suspected, I endeavoured to obtain approximate valuations for the constant factor K, which was introduced in the comparison of the tidal forces with the force of equilibrium. These approximations led me to "suggest the propriety of considering the element of density (or of its correlative, the square of the time of molecular diffusion), in connexion with both A and M."

In the year after this suggestion was made, Dr. Menzzer announced, as an experimental result (Poggendorff's *Annalen*, November 1865; Phil. Mag. vol. xxx. p. 456), that "the magnetizing-powers of two coils which give the maximum of intensity are as the square roots of their weights."

It therefore appears,

(1) From Graham's and other well-known laws:—

Elasticity $\propto$ specific heat $\propto$ (wave velocity)^2.
Density $\propto$ (time of molecular diffusion)^2.
Weight $\propto$ (time of sonorous vibration)^2.

(2) From observations on terrestrial magnetism:—

Tidal differences $\propto$ (magnetic differences)^2.
Magnetic variation $\propto$ (time)^2.

(3) From Menzzer's experiments:—

Weight $\propto$ (magnetizing-power)^2.

This indirect confirmation of a conjecture which was at first based on a plausible analogy, encourages me to hope that the following comparisons between molecular and cosmical kinetic values may help to explain the specific magnetism of iron.

According to Tredgold, iron may be elongated about $\frac{1}{1400}$ without permanent alteration of structure. Now the ratio, at the earth's surface, of solar terrestrial attraction is about $\frac{1}{1640}$; and four times the ratio of the specific gravity of air to that of iron varies approximately between $\frac{1}{1400}$ and $\frac{1}{1700}$. Although this range of variation is somewhat more than $\frac{1}{5}$ of the least

* From Silliman's American Journal for March 1868.

† Trans. Amer. Phil. Soc. vol. xiii. p. 126. [Phil. Mag. July 1865, p. 52.]

the mean excursion of a molecule between its collisions; and it is, at all events, not likely that it is *much* more than $\frac{1}{60}$ in the gases experimented on by Professor Maxwell. Hence the mean distance at ordinary temperatures and pressures is probably of the same order as a ninth-metre, which is the millionth part of a millimetre. It is therefore probable that there are not fewer than something like a unit-eighteen of molecules in each cubic millimetre of a gas at ordinary temperatures and pressures*.

XIX. *On Geological Time, and the probable Date of the Glacial and the Upper Miocene Period. By* James Croll, *of the Geological Survey of Scotland.*

[Continued from vol. xxxv. p. 384.]

IT will not do, however, to measure marine denudation by the rate at which the sea is advancing on the land. There is no relation whatever between the rate at which the sea is *advancing* on the land and the rate at which the sea is *denuding* the land. For it is evident that as the subaërial agents bring the coast down to the sea-level, all that the sea has got to do is simply to advance, or at most to remove the loose materials which may lie in its path. The amount of denudation which has been effected by the sea during past geological ages, compared with what has been effected by subaërial agency, is evidently but trifling. Denudation is not the proper function of the sea. The great denuding agents are land-ice, frost, rain, running water, chemical agency, &c. The proper work which belongs to the sea is the transporting of the loose materials carried down by the rivers, and the spreading of these out so as to form the stratified beds of future ages.

We have thus seen that geology, alike with physics, is opposed to the idea of an unlimited age to our globe. And it is perfectly plain that if there be physical reasons, as there certainly are, for limiting the age of the earth to something less than 100 millions of years, geological phenomena, when properly interpreted, do not offer any opposition.

Perhaps one of the things which has tended to mislead on this point is a false analogy which is supposed to subsist between

* Hence we may see how entirely remote from a state of emptiness that which usually passes under the name of a vacuum chamber really is. If there be a unit-eighteen of molecules in every cubic millimetre of the air about us, there will remain a unit-XV in every cubic millimetre of the best vacuums of our ordinary air-pumps. The molecules are still closely packed, within about an eighth-metre of one another; *i. e.* there are about sixty of them in a wave-length of orange light.

astronomy and geology, viz. that geology deals with unlimited *time,* as astronomy deals with unlimited *space.* A little consideration, however, will show that there is not much analogy between the two cases.

Astronomy deals with the countless worlds which lie spread out in the boundless infinity of space; but geology deals with but one world. No doubt both reason and analogy are favourable to the idea that the age of the material universe, like its magnitude, is immeasurable—although, however, we have no reason to conclude that it is eternal, any more than we have to conclude that it is infinite. But when we compare the age of the material universe with its magnitude, we must not take the age of one of its members (say, our globe) and compare it with the size of the universe. More than this, we must not take the age of all the presently existing systems of worlds and compare their age with the magnitude of the universe; but we must take the past history of the universe as it stretches back into the infinity of bygone *time,* and compare it with the presently existing universe as it stretches out on all sides into the infinity of *space.* For worlds precede worlds in time as worlds lie beyond worlds in space. Each world, each individual, each atom is evidently working out a final purpose, according to a plan prearranged and predetermined by the Divine Mind from all eternity. And each world, like each individual, when it serves the end for which it was called into existence, disappears to make room for others. This is the grand conception of the universe which naturally impresses itself on every thoughtful mind that has not got into confusion about those things called in science the Laws of Nature.

But the geologist does not pass back from world to world as they stand related to each other in the order of *succession in time,* as the astronomer passes from world to world as they stand related to each other in the order of *coexistence in space.* Besides, the researches of the geologist are not only confined to one world, but it is only a portion of the history of that one world that can come under his observation. The oldest of existing formations, so far as is yet known, the Laurentian Gneiss, are made up of the waste of previously existing rocks, and these, again, probably of the waste of rocks still prior. Regarding what succeeds these old Laurentians, geology tells us much; but regarding what preceded, we know nothing whatever. For anything that geology shows to the contrary, the time which may have elapsed from the solidifying of the earth's crust to the deposition of the Laurentians—an absolute blank—may have been as great as the time that has elapsed since then till the present day. Physical science limits the age of the globe to a period not exceeding something like 100 millions of years. Taking the age of the earth's crust at 100 millions of years, which probably is a high estimate, how long is it since the materials which now form these Laurentian rocks were laid down in the ancient sea-bottom in the form of sand and mud? This is a question which no one can answer; for we have no means of knowing how much of the 100 millions of years were exhausted before the Laurentian age. At all events, it must be very considerably less than 100 millions of years since the commencement of the Laurentian period.

Sir William Logan thinks that the time which separated the limestones containing the *Eozoon Canadense* from the Upper-Cambrian period may be as great as the time which elapsed between the Upper Cambrian and the nummulitic limestones of the Tertiary period. If this conjecture be anything like correct, then it is hardly possible that 50 millions of years can have elapsed since the Cambrian period. Assuming that the glacial epoch began about a quarter of a million years ago, and that the rate at which species change is uniform, we found that, adopting Sir Charles Lyell's mode of calculation, 60 millions of years have probably elapsed since the beginning of the Cambrian period. But I presume little or no weight can be placed on this mode of calculation; for it is based upon an assumption for which there is, I fear, very little warrant, viz. that the rate at which species change has been anything like uniform during geological ages. If any very great amount of time elapsed between the solidifying of the earth's crust and the Laurentian period, the probability is that the commencement of the Cambrian period does not date so far back as 60 millions of years.

Table I. shows the excentricity of the earth's orbit and longitude of the perihelion for 3 millions of years back, and 1 million of years to come, at periods fifty thousand years apart. From what we have already seen, it is very evident that 3 millions of years must stretch a considerable distance back into the geological history of our globe. And if geological climate depends upon, or is much affected by, the condition of the earth's orbit as regards excentricity, we have in this Table, combined with the other three, the means by which a rough idea of the character of the climate during at least a considerable portion of the Tertiary period may be arrived at.

TABLE I.

Number of years before A.D. 1800.	Excentricity of orbit.	Longitude of perihelion.	Number of years before and after A.D. 1800.	Excentrici t of orbit.	Longitude of perihelion.
−3,000,000	0·0365	39° 30′	−1,000,000	0·0151	248° 22′
−2,950,000	0·0170	210 39	− 950,000	0·0517	97 51
−2,900,000	0·0442	200 52	− 900,000	0·0102	135 2
−2,850,000	0·0416	0 18	− 850,000	0·0747	239 28
−2,800,000	0·0352	339 14	− 800,000	0·0132	343 49
−2,750,000	0·0326	161 22	− 750,000	0·0575	27 18
−2,700,000	0·0330	65 37	− 700,000	0·0220	208 13
−2,650,000	0 0053	318 40	− 650,000	0·0226	141 29
−2,600,000	0·0660	190 4	− 600,000	0·0417	32 34
−2,550,000	0·0167	298 34	− 550,000	0·0166	251 50
−2,500,000	0 0721	338 36	− 500,000	0·0388	193 56
−2,450,000	0·0252	109 33	− 450,000	0·0308	356 52
−2,400,000	0·0415	116 40	− 400,000	0·0170	290 7
−2,350,000	0·0281	308 23	− 350,000	0·0195	182 50
−2,300,000	0·0238	195 25	− 300,000	0·0424	23 29
−2,250,000	0·0328	141 18	− 250,000	0·0258	59 39
−2,200,000	0·0352	307 6	− 200,000	0·0569	168 18
−2,150,000	0·0183	307 5	− 150,000	0·0332	242 56
−2,100,000	0·0304	98 40	− 100,000	0·0473	316 18
−2,050,000	0·0170	334 46	− 50,000	0 0131	50 14
−2,000,000	0·0138	324 4	A.D. 1800. 0	0·0168	99 30
−1,950,000	0 0427	120 32	+ 50,000	0·0173	38 12
−1,900,000	0·0336	188 31	+ 100,000	0·0191	114 50
−1,850,000	0·0503	272 14	+ 150,000	0·0353	201 57
−1,800,000	0·0334	354 52	+ 200,000	0·0246	279 41
−1,750,000	0·0350	65 25	+ 250,000	0·0286	350 54
−1,700,000	0·0085	95 13	+ 300,000	0·0158	172 29
−1,650,000	0·0035	168 23	+ 350,000	0·0098	201 40
−1,600,000	0·0305	158 42	+ 400,000	0 0429	6 9
−1,550,000	0·0239	225 57	+ 450,000	0·0231	98 37
−1,500,000	0·0430	303 29	+ 500,000	0·0534	157 26
−1,450,000	0·0195	57 11	+ 550,000	0·0259	287 31
−1,400,000	0·0315	97 35	+ 600,000	0·0395	285 43
−1,350,000	0·0322	293 38	+ 650,000	0·0169	144 3
−1,300,000	0·0022	0 48	+ 700,000	0·0357	17 12
−1,250,000	0·0475	105 50	+ 750,000	0·0195	0 53
−1,200,000	0·0289	239 34	+ 800,000	0 0639	140 38
−1,150,000	0·0473	250 27	+ 850,000	0·0144	176 41
−1,100,000	0·0311	55 24	+ 900,000	0·0659	291 16
−1,000,000	0·0326	4 8	+ 950,000	0 0086	115 13
			+1,000,000	0·0528	57 31

On looking over Table I., it will be seen that there are three principal periods when the excentricity rose to a very high value, with a few subordinate maxima between. It will be perceived also that during each of those periods the excentricity does not remain at the same uniform value, but rises and falls, in one case twice, and in the other two cases three times. About 2,650,000 years back we have the excentricity almost at its inferior limit. It then begins to increase, and fifty thousand years afterwards, namely at 2,600,000 years ago, it reaches ·0660; fifty thousand years after this period it has diminished to ·0167, which is about its present value. It then begins to increase, and in another fifty thousand years, viz. at 2,500,000 years ago, it approaches to almost the superior limit, its value being then ·0721. It then begins to diminish, and at 2,450,000 years ago it has diminished to ·0252. These two maxima, separated by a minimum and extending over a period of 200,000 years, constitute the first great period of high excentricity. We then pass onwards for upwards of a million and a half years, and we come to the second great period. It consists of three maxima separated by two minima. The first maximum occurred at 950,000 years ago, the second or middle one at 850,000 years ago, and the third and last at 750,000 years ago—the whole extending over a period of nearly 300,000 years. Passing onwards for another million and half years, or to about 800,000 years in the future, we come to the third great period. It also consists of three maxima one hundred thousand years apart. These occur at the periods 800,000, 900,000, and 1,000,000 years to come, respectively, separated also by two minima. Those three great periods, two of them in the past and one of them in the future, included in the Table, are therefore separated from each other by an interval of upwards of 1,700,000 years.

In this Table there are three periods when the excentricity approaches to about its inferior limit, and the orbit becomes almost circular. The first is 2,650,000 years ago, when the excentricity was ·0053; the next is at 1,300,000 years ago, when it was only ·0022; and the next will occur in about 24,000 years hence, when its value will be ·0033. There is, therefore, an interval of 1,350,000 years between the minima in the one case, and an interval of 1,324,000 years in the other case.

The Table shows also four subordinate periods of high excentricity, of which the one that I have supposed corresponds to the glacial epoch is the chief. Three are in the past, and one in the future.

If the glacial epoch resulted from a high state of excentricity, there must have been at least five ice-periods, including the glacial epoch, during the past three millions of years—two as severe, and two much less severe than the glacial epoch.

It may be thought that so many as four or five ice-periods in the course of 3 millions of years past is inconsistent with the evidence of geology on that point. This, however, is at least very doubtful. It is quite possible that these three millions of years may embrace the greater part of the Tertiary period. Now we have evidence of at least three ice-periods since the beginning of the Tertiary period—one about the middle of the Eocene period, another during the Upper Miocene period, and the third and last the well-known glacial epoch*; and it is quite possible that the evidence of more may yet be found.

But before discussing the nature of the evidence which geology affords of the existence of former glacial periods, we shall turn our attention briefly to Tables II., III., and IV. These three Tables embrace the three periods of greatest excentricity during the past 3 millions of years. The excentricity, longitude of the perihelion, &c. are given at periods of ten thousand years apart.

There are still eminent astronomers and physicists who are of opinion that the climate of the globe never could have been seriously affected by changes in the excentricity of its orbit. This opinion results, no doubt, from viewing the question as a purely astronomical one. Viewed from an astronomical stand-point, as has been already remarked, there is actually nothing from which any one could reasonably conclude with certainty whether a change of excentricity would seriously affect climate or not. By means of astronomy we ascertain the extent of the excentricity at any given period, how much the winter may exceed the summer in length (or the reverse), how much the sun's heat is increased or decreased by a decrease or an increase of distance, and so forth; but we obtain no information whatever regarding how these will actually affect climate. This must be determined wholly from physical considerations, and it is an exceedingly complicated problem. An astronomer, unless he has given special attention to the physics of the question, is just as apt to come to a wrong conclusion as any one else. The question involves certain astronomical elements; but when these are determined, everything then connected with the matter is purely physical. Nearly all the astronomical elements of the question are comprehended in the accompanying Tables.

In Tables II., III. and IV., column I. represents the dates of the periods, column II. the excentricity, column III. the

* See Lyell's 'Principles,' vol. i. chap. x. (tenth edition).

longitude of the perihelion. In Table IV. the excentricity and the longitude of the perihelion of the six periods marked with an S are copied from a letter of Mr. Stone to Sir Charles Lyell, published in the Supplement of the Phil. Mag. for June 1865; the eight periods marked L are copied from M. Leverrier's Memoir in the *Connaissance des Temps* for 1843. For the correctness of everything else, both in this Table and in the other three, I alone am responsible.

Column IV. gives the number of degrees passed over by the perihelion during each 10,000 years. From this column it will be seen how irregular is the motion of the perihelion. At four different periods it had a retrograde motion for 20,000 years. Column V. shows the number of days by which the winter exceeds the summer when the winter occurs in aphelion. Column VI. shows the intensity of the sun's heat during midwinter, when the winter occurs in aphelion, the present midwinter intensity being taken at 1000. These six columns comprehend all the astronomical part of the Tables. Regarding the correctness of the principles upon which these columns are constructed, there is no diversity of opinion. But these columns afford no direct information as to the character of the climate, or how much the temperature is increased or diminished. To find this we pass on to columns VII., VIII. and IX., calculated on physical principles. Now, unless the physical principles upon which these three columns are calculated be wholly erroneous, undoubtedly change of excentricity must very seriously affect climate. Column VII. shows how many degrees Fahrenheit the temperature is lowered by a decrease in the intensity of the sun's heat corresponding to column VI. For example, 850,000 years ago, if the winters occurred then in aphelion, the direct heat of the sun during midwinter would be only $\frac{837}{1000}$ of what it is at present at the same season of the year. Column VII. shows that this decrease in the intensity of the sun's heat would lower the temperature 45°·3 F.

The principle upon which this result is arrived at is this:— The temperature of space, as determined by Sir John Herschel, is $-239°$ F. M. Pouillet, by a different method, arrived at almost the same result. Taking the midwinter temperature of Great Britain at 39°, consequently, $239°+39°=278°$ represent the number of degrees of rise due to the sun's heat at midwinter; in other words, it takes a quantity of sun-heat which we have represented by 1000 to maintain the temperature of the earth's surface at Great Britain 278° above the temperature of space. Were the sun extinguished, the temperature of our island would sink 278° below its present midwinter temperature, or to the temperature of space. But 850,000 years ago, if the

Table II.

I.	II.	III.	IV.	Winter occurring in aphelion.				
				V.	VI.	VII.	VIII.	IX.
Number of years before A.D. 1800.	Excentricity of orbit.	Longitude of perihelion.	Number of degrees passed over by the perihelion. Motion retrograde at periods marked R.	Excess of winter over summer, in days.	Midwinter intensity of the sun's heat. Present intensity = 1000.	Number of degrees by which the midwinter temperature is lowered.	Midwinter temperature.	Midwinter temperature, the Gulf-stream being diminished in proportion to the excentricity.
		° ′	° ′			°F.	°F.	°F.
2,650,000	0·0053	318 40	95 45					
2,640,000	0·0173	54 25	39 12					
2,630,000	0·0331	93 37	33 35	15·4	906	26·2	12·8	6·6
2,620,000	0·0479	127 12	31 24	22·2	884	33·3	5·7	− 3·3
2,610,000	0·0591	158 36	31 28	27·4	862	38·3	0·7	−10·5
2,600,000	0·0660	190 4	30 24	30·6	851	41·5	−2·5	−15·0
2,590,000	0·0666	220 28	29 28	30·9	850	41·8	−2·8	−15·4
2,580,000	0·0609	249 56	27 28	28·3	859	39·2	−0·2	−11·7
2,570,000	0·0492	277 24	27 38	22·9	878	33·9	5·1	− 4·2
2,560,000	0·0350	305 2	R 6 28	16·2	902	27·1	11·9	5·3
2,550,000	0·0167	298 34	R 44 36					
2,540,000	0·0192	253 58	5 21					
2,530,000	0·0369	259 19	23 48	17·1	899	28·0	11·0	4·0
2,520,000	0·0537	283 7	26 57	25·0	871	35·9	3·1	− 7·0
2,510,000	0·0660	310 4	28 32	30·6	851	41·5	−2·5	−15·0
2,500,000	0·0721	338 36	29 0	33·5	841	44·2	−5·2	−18·8
2,490,000	0·0722	7 36	28 10	33·6	841	44·3	−5·3	−19·0
2,480,000	0·0662	35 46	27 40	30·8	850	41·7	−2·7	−15·2
2,470,000	0·0553	63 26	25 47	25·7	868	36·6	2·4	− 8·1
2,460,000	0·0410	89 13	20 20	19·1	892	30·0	9·0	1·3
2,450,000	0·0252	109 33		11·7				

TABLE III.

1,000,000	0·0151	248 22	65 28					
990,000	0·0224	313 50	44 12					
980,000	0·0329	358 2	34 38	15·3	906	26·1	12·9	6·7
970,000	0·0441	32 40	34 9	20·5	887	31·5	7·5	−0·4
960,000	0·0491	66 49	31 2	22·8	878	33·8	5·2	−4·1
950,000	0·0517	97 51	29 51	24·0	874	35·0	4·0	−5·8
940,000	0·0195	127 42	28 29	23·0	878	34·0	5·0	−4·3
930,000	0·0423	156 11	25 29	19·7	890	30·6	8·4	0·4
920,000	0·0305	181 40	12 35	14·2	910	25·0	14·0	8·2
910,000	0·0156	194 15	R 59 13					
900,000	0·0102	135 2	R 8 1					
890,000	0·0285	127 1	25 32					
880,000	0·0456	152 33	27 50	21·2	884	32·2	6·8	− 2·0
870,000	0·0607	180 23	29 18	28·2	859	39·0	0·0	−11·5
860,000	0·0708	209 41	29 47	32·9	843	43·6	−4·6	−18·0
850,000	0·0747	239 28	29 46	34·7	837	45·3	−6·3	−20·3
840,000	0·0698	269 14	29 14	32·4	845	43·2	−4·2	−17·4
830,000	0·0623	298 28	27 36	29·0	857	40·0	−1·0	−12·8
820,000	0·0476	326 4	22 26	22·1	881	33·1	5·9	− 3·1
810,000	0·0296	348 30	R 4 41					
800,000	0·0132	343 49	R 50 30					
790,000	0·0171	293 19	10 18					
780,000	0·0325	303 37	25 1	15·2	907	26·0	13·0	6·7
770,000	0·0455	328 38	28 34	21·2	884	32·2	6·8	−2·0
760,000	0·0540	357 12	30 6	25·1	870	36·0	3·0	−7·2
750,000	0·0575	27 18	31 12	26·7	864	37·7	1·3	−9·5
740,000	0·0561	58 30	32 25	26·1	867	37·0	2·0	−8·6
730,000	0·0507	90 55	34 19	23·6	876	34·6	4·4	−5·2
720,000	0·0422	125 14	52 12	19·6	890	30·6	8·4	0·4
710,000	0·0307	177 26	30 47	14·3	910	25·0	14·0	8·2
700,000	0·0220	208 13						

TABLE IV.

I.	II.	III.	IV.	Winter occurring in aphelion.				
				V.	VI.	VII.	VIII.	IX.
Number of years before A.D. 1800.	Excentricity of orbit.	Longitude of perihelion.	Number of degrees passed over by the perihelion. Motion retrograde at periods marked R.	Excess of winter over summer, in days.	Midwinter intensity of the sun's heat. Present intensity =1000.	Number of degrees by which the midwinter temperature is lowered.	Midwinter temperature.	Midwinter temperature, the Gulf-stream being diminished in proportion to the excentricity.
250,000	0·0258	59° 39′				°F.	°F	°F.
240,000	0·0374	74 58	15° 19′	17·4	898	28·3	10·7	3·7
S 230,000	0·0477	102 49	27 51	22·2	885	33·2	5·8	—3·2
S 220,000	0·0497	124 33	21 44	23·2	877	34·1	4·9	—4·6
S 210,000	0·0575	144 55	20 22	26·7	864	37·7	1·3	—9·5
200,000	0·0569	168 18	23 23	26·5	865	37·4	1·6	—9·1
S 190,000	0·0532	190 4	21 46	24·7	871	35·7	3·3	—6·7
S 180,000	0·0476	209 22	19 18	22·1	881	33·1	5·9	—3·1
S 170,000	0·0437	228 7	18 45	20·3	887	31·3	7·7	—0·6
160,000	0·0364	236 38	8 31	16·9	900	27·8	11·2	4·3
150,000	0·0332	242 56	6 18	15·4	905	26·2	12·8	6·5
140,000	0·0346	246 29	3 33	16·1	903	26·9	12·1	5·6
130,000	0·0384	259 34	13 5	17·8	896	28·8	10·2	3·0
120,000	0·0431	274 47	15 13	20·1	888	31·0	8·0	—0·1
110,000	0·0460	293 48	19 1	21·4	883	32·4	6·6	—2·1
100,000	0·0473	316 18	22 30	22·0	881	33·0	6·0	—2·9
L 90,000	0·0452	340 2	23 44	21·0	885	32·0	7·0	—1·5
L 80,000	0·0398	4 13	24 11	18·5	894	29·4	9·6	2·1
L 70,000	0·0316	27 22	23 9	14·7	908	25·5	13·5	7·5
L 60,000	0·0218	46 8	18 46					
50,000	0·0131	50 14	4 6					
L 40,000	0·0109	28 36	R 21 38					
L 30,000	0·0151	5 50	R 22 46					
L 20,000	0·0188	44 0	38 10					
L 10,000	0·0187	78 28	34 28					
0	0·0168	99 30	21 2					

winters occurred in aphelion, the heat of the sun at midwinter would only equal 837 instead of 1000 as at present. Consequently, if it takes 1000 parts of heat to maintain the temperature 278° above the temperature of space, 837 parts of heat will only be able to maintain the temperature 232°·7 above the temperature of space; for 232°·7 is to 278 as 837 is to 1000. Therefore, if the temperature was then only 232°·7 above that of space, it would be 45°·3 below what it is at present.

This method of calculating how much the temperature is lowered by a given reduction of the sun's heat is that given by Sir John Herschel in his 'Outlines of Astronomy,' § 369 *a*. About three years ago, in an article in 'The Reader,' I endeavoured to show that this method is not rigidly correct. It results from the principles of the dynamical theory of heat, and is also supported by experiments made by Professor Draper, of New York, and others, that the rate at which a body radiates its heat off into space is not directly proportionate to its absolute temperature. The rate at which a body loses its heat as its temperature rises, increases more rapidly than the temperature. As a body rises in temperature, the rate at which it radiates off its heat increases; but the *rate* of this increase is not uniform, but increases with the temperature; consequently the temperature is not lowered in proportion to the decrease of the sun's heat. But this error is probably neutralized by one of an opposite nature, to which I shall now refer.

We know that absolute zero is at least 493° below the melting-point of ice; consequently, if the heat derived from the stars is able to maintain a temperature of −239° or 222° of absolute temperature, then nearly as much heat is derived from the stars as from the sun. But if so, why do the stars give so much heat and so very little light? If the radiation from the stars could maintain a thermometer 222° above absolute zero, then space must be far more transparent to heat-rays than to light-rays, or else the stars give out a great amount of heat but very little light, neither of which suppositions is probably true. The probability is, I venture to presume, that the temperature of space is not much above absolute zero. In this case, by adopting −239° as the temperature of space, we make the values given in column VII. too small. But as the two errors tend to neutralize each other, these values may in the meantime be accepted as not very far from the truth, or at least as near as can be arrived at in the present state of science on this point. But whether these values be too high or too low, one thing is certain, that a very slight increase or a very slight decrease in the quantity of heat received from the sun must affect temperature to a considerable extent. The direct heat of the moon, for example, cannot be detected by the finest instruments which we possess; yet from 238,000 observations made at Prague during 1840–66, it would seem that the temperature is sensibly affected by the mere change in the lunar perigee and inclination of the moon's orbit*.

Column VIII. gives the midwinter temperature. It is found by subtracting the numbers in column VII. from 39°, the midwinter temperature. Column IX. shows the midwinter temperature of the centre of Scotland, on the supposition that the Gulf-stream was diminished in volume in proportion to the excentricity. In former papers it was explained how a change of excentricity must affect ocean-currents†.

I have not given a Table showing the temperature of the summers at the corresponding periods. This could not well be done; for there is no relation at the periods in question between the intensity of the sun's heat and the temperature of the summers. One is apt to suppose, without due consideration, that the summers ought to be then as much warmer than they are at present, as the winters were then colder than now. Sir Charles Lyell, in his 'Principles,' has given a column of summer temperatures calculated upon this principle. Astronomically the principle is correct, but physically it is totally erroneous, and calculated to convey a wrong impression regarding the whole subject of geological climate. The summers at those periods, instead of being much warmer than they are at present, would in reality be far colder than they are now, notwithstanding the great increase in the intensity of the sun's heat resulting from the diminished distance of the sun. If a country is free from snow and ice, then no doubt the temperature will rise during summer as the intensity of the sun's heat increases; but when such a country is enveloped in perpetual snow and ice, the temperature of the summers will never rise much above the freezing-point, no matter what the intensity of the sun's heat may be. The physical reason of this was explained on a former occasion‡. In a country covered with ice, the direct heat of the sun is often very intense, in fact scorching. It will raise the temperature of the mercury in the thermometer exposed to the direct rays of the sun, but it fails to heat the air. Captain Scoresby, for example, when in lat. 80° 19′ N., found the side of his ship heated by the direct rays of the sun to about 100°,

* See Professor C. V. Zenger's paper "On the Periodic Change of Climate caused by the Moon," Phil. Mag. for June 1868.
† Phil. Mag. for August 1864 and February 1867.
‡ Phil. Mag. for February 1867.

while the air surrounding the ship was actually 18° below the freezing-point. On another occasion he found the pitch melting on the one side of the ship by the heat of the sun, while water was freezing on the other side from the intense coldness of the air.

The mean temperature of Van Rensselaer Harbour, in lat. 78° 37′ N., long. 70° 53′ W., was accurately determined from hourly observations made day and night over a period of two years by Dr. Kane. It was found to be as follows:—

Winter . .	−28°·59
Spring . . .	−10·59
Summer . .	+33·38
Autumn . .	− 4·03

But although the quantity of heat received from the sun at that latitude ought to have been greater during the summer than in England*, yet, nevertheless, the temperature is only 1°·38 above the freezing-point.

The temperature of Port Bowen, lat. 73° 14′ N., was found to be as follows:—

Winter . .	−25°·09
Spring . . .	− 5·77
Summer . .	+34·40
Autumn . .	+10·58

Here the summer is only 2°·4 above the freezing-point.

If we go to the Antartic regions, where the influence of ice is still more felt, we find the summers even still colder. Capt. Sir James Ross found, when between lat. 60° and 77° S., that the mean temperature never rose even to the freezing-point during the entire southern summer; and when near the ice-barrier on the 8th of February, 1841, a season of the year equivalent to August in England, he had the thermometer at 12° at noon, and so rapidly was the young ice forming around the ships that it was with difficulty that he escaped being frozen in for the winter. And on the February of the following year, when he again visited that place, he had the thermometer standing at 19° at noon, and the sea covered with an unbroken sheet of young ice as far as the eye could reach from the mast-head. This extraordinary low temperature at that season of the year was wholly owing to the presence of the ice. Had there been no ice on the Antarctic continent, Sir James would have had a summer

* See Mr. Meech's memoir "On the Intensity of the Sun's Heat and Light," Smithsonian Contributions, vol. ix.

hotter than that of England, instead of one actually below the freezing-point.

Now, during the glacial epoch, when Europe was almost covered with snow and ice, the summers could not possibly have been much warmer than they are at present in Arctic and Antarctic regions. In other words, during the glacial epoch the mean summer temperature would be very little above the freezing-point.

[To be continued.]

XX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 68.]

May 28, 1868.—Lieut.-General Sabine, President, in the Chair.

THE following communication was read:—

"On the Impact of Compressible Bodies, considered with reference to the Theory of Pressure." By R. Moon, M.A., Honorary Fellow of Queen's College, Cambridge.

Suppose that we have two *rigid* cylinders of equal dimensions, which have their axes in the same straight line; suppose, also, that one of the cylinders is at rest while the other moves towards the first with the velocity V in a direction parallel to both the axes; the consequence of the collision which under such circumstances must take place, will manifestly be that half the momentum of the moving cylinder will be withdrawn from it, and will be transferred to the cylinder which originally was at rest.

The mode in which velocity or momentum will thus be collected from the different parts of the one cylinder, and distributed amongst those of the other, is obvious. Exactly the same amount will be withdrawn from the velocity of each particle of the impinging cylinder, and exactly the same amount of velocity will be impressed on each particle of the cylinder struck.

And the reason of this is equally obvious, since, if such were not the case, the particles of each cylinder would *contract*—a supposition which is forbidden by the very definition of rigidity.

But if, instead of being perfectly rigid, each cylinder is in the slightest degree compressible, a variation in the effect will occur.

As before, momentum of finite amount will be transferred from the one cylinder to the other, but the mode of collection of the velocity withdrawn from the one, and the mode of distribution of that injected into the other, will no longer be the same as before.

In order that the moving cylinder may not be reduced to absolute rest by the collision, it is obvious that the cylinder originally at rest, or a portion of it, must be moved out of the way, so as to allow of the continuance, even in a modified degree, of the other's out, for the experiments which speak in its favour are not more numerous or more accurate.

Moreover the result which these heatings of the poles give is nothing more than a confirmation of the conclusions which I have drawn from my earlier investigation on the voltaic arc. No one will doubt that the electromotive force which, from the experiments adduced, is met with in the just-extinguished arc is the same as that which, from my former investigations, occurs in the luminous arc during its existence. But it has been shown * in regard to this force, that it is independent of the intensity of the current. It is only when the intensity approaches the minimum with which a luminous arc can be produced, that a decrease in the magnitude of the force begins to be seen †. But the temperature of the pole-points is dependent on the intensity of the current, and the same is also obviously the case with their difference in temperature. The force found is thus independent of the heating of the poles, and therefore cannot be of thermoelectric origin.

XLVII. *On Geological Time, and the probable Date of the Glacial and the Upper Miocene Period. By* JAMES CROLL, *of the Geological Survey of Scotland.*

[Continued from p. 154.]

AS has already been remarked, if the glacial epoch resulted from a high state of excentricity there must, according to the foregoing Tables, have been at least five ice-periods during the last three millions of years, viz. the glacial epoch, two as severe, and two much less severe. The question which now remains to be discussed is whether this conclusion is consistent with the facts of geology or not. Can it be shown from geological evidence that there have not within the past three millions of years been two ice-periods as severe as the glacial period? In other words, do the testimony of geology and the testimony of physics agree in regard to this point?

From what has already been shown respecting the limit to geological time, I presume we may reasonably conclude that three millions of years may probably embrace the greater portion of the Tertiary period. Now, as has been stated, we have good evidence of at least three ice-periods since the beginning of the Tertiary period—one about the middle of the Eocene period, another during the Upper Miocene period, and the third and last the well-known glacial epoch.

* Phil. Mag. S. 4. vol. xxxv. p. 103.
† *Œfversigt af K. Vet. Akad. Förhandl.* December 1867.

Let it be observed that it is not essential to the object in view to be able to show from the facts of geology that former cold periods, such as those of the Eocene and the Miocene, were as severe as that of the glacial epoch. All that is required is simply to show that, for anything that geology can prove to the contrary, those cold periods *may* have been as severe as the glacial epoch. What I wish to show is, that although the conclusions derived from astronomical and physical considerations regarding the severity of former ice-periods may not at present be fully borne out by geological science, the imperfection of geological records upon this point is such that the absence of direct geological evidence cannot reasonably be regarded as sufficient proof that the physical conclusions arrived at are improbable. But this is not all; I shall endeavour to show that the records of geology in regard to former glacial epochs are not only imperfect, but that this imperfection *follows as a necessary consequence from the principles of geology itself*—that there is not simply a want of records, but a *reason* in the very nature of geological evidence why there is such a want.

Geological evidence in reference to past glacial epochs is evidently much more imperfect than many suppose.

It is on a land-surface that the principal traces of the action of ice during a glacial epoch are left; for it is there that the stones are chiefly striated, the rocks ground down, and the boulder-clay formed. But where are all our ancient land-surfaces? They are not to be found. The total thickness of the stratified rocks of Great Britain, according to Professor Ramsay, is nearly fourteen miles. But from the top to the bottom of this enormous deposit there is hardly a single land-surface to be found. True, there are patches of old land-surfaces of a local character, such, for example, as the dirt-beds of Portland; but, with the exception of the coal-beds, every general formation from top to bottom was formed under water, and none but the under-clays *ever existed as a land-surface.* And it is here in such a formation that the geologist has to collect all his information regarding the existence of former glacial epochs. The entire stratified rocks of the globe, with the exception of the coal-beds and under-clays (places where no one would expect to find traces of ice-action), consist almost entirely of a *series of old sea-bottoms,* with here and there an occasional freshwater deposit. Bearing this in mind, what is the sort of evidence which we can now hope to find in these old sea-bottoms of the existence of former ice-periods?

Every one, of course, who has ever reflected on the matter admits that the stratified rocks are not old land-surfaces, but a series of old sea-bottoms formed out of the destruction of old land-surfaces. And it is true that all land-surfaces once existed as sea-bottoms; but the stratified rocks consist of a series of old sea-bottoms which never were land-surfaces. Many of them no doubt have been repeatedly above the sea-level, and may once have possessed land-surfaces; but these, with the exception of the under-clays of the coal-measures, the dirt-beds of Portland, and one or two more patches, have all been annihilated. The important bearing that this consideration has on the nature of the evidence which we can now expect to find of the existence of former glacial epochs has certainly been very much overlooked.

When we examine the matter fully, we find that the *transformation of a land-surface into a sea-bottom* will probably completely obliterate every trace of glaciation that may have existed on that land-surface. For example, we cannot expect to find the polished and striated stones belonging to a former land-glaciation; for stones are not carried down by our rivers and deposited in the sea. They are first disintegrated by subaërial agency into sand or clay, as the case may be, and then carried down by our rivers and deposited as such on the sea-bottom. But supposing striated stones were carried down by our rivers out of the boulder-clay, they could not retain their ice-markings, for they would soon become waterworn in their passage seawards.

Neither can we expect to find boulder-clay in the stratified rocks, for boulder-clay is not carried down as such and deposited in the sea. The boulder-clay is washed off the land and carried down as soft mud, clay, sand, and gravel. Patches of boulder-clay may have been now and again forced into the sea by means of the ice and become covered up; but such cases are wholly exceptional, and the absence of examples of this sort in any formation cannot fairly be adduced as a proof that that formation does not belong to a glacial period.

The only evidence which we can now reasonably expect to find in the stratified rocks of the existence of land-ice of former epochs, is the presence of erratic blocks which may have been transported by icebergs and dropped into the sea. But unless the glaciers of that epoch reached the sea or the sea was frozen, we could not possibly have even this evidence. Traces in the stratified rocks of the effects of land-ice of former epochs must, from the very nature of things, be rare indeed. The only sort of evidence which, as a general rule, we may always expect to find is the presence of large blocks of older rocks found imbedded in strata which, we know from their constitution, must have been formed in still water. But this is quite enough; for it proves the existence of ice at the time that the deposit containing the blocks was being formed as conclusively as though we saw the ice floating with the blocks upon it. This sort of evidence, when found in low latitudes, ought to be received as conclusive of the existence of former glacial epochs, and, no doubt, would have been so received had it not been for the idea that, if these blocks had been transported by ice, there ought in addition to have been found striated stones, boulder-clay, and other indications of the agency of land-ice.

The reason why we now have, comparatively speaking, so little direct evidence of the existence of former glacial periods will be more forcibly impressed upon the mind, if we reflect on how difficult it would be in a million or so of years hence to find any trace of what we now call the glacial epoch. The striated stones would by that time be all, or nearly all disintegrated, and the till washed away and deposited in the bottom of the sea as stratified sands and clays. And when these became consolidated into rock and were raised into dry land, the only evidence that we should probably then have that there ever had been a glacial epoch would be the presence of large blocks of the older rocks, which would be found imbedded in the upraised formation. We could only infer that there had been ice at work from the fact that by no other known agency could we conceive such blocks to have been transported and dropped in a still sea.

Probably few geologists believe that during the Middle Eocene and the Upper Miocene periods our country passed through a condition of glaciation as severe as it has done during the Post-pliocene period; yet when we examine the subject carefully, we find that there is actually no just ground to conclude that it has not. For in all probability, in the strata formed out of the destruction of the now existing land-surfaces, evidence of ice-action will be as scarce as in the Eocene or Miocene strata.

If the stratified rocks forming the earth's crust consisted of a series of old land-surfaces instead (as they actually do) of a series of old sea-bottoms, then probably dozens of glacial periods might be detected.

Nearly all the evidence which we have regarding the glacial epoch has been derived from what we find on the now existing land-surfaces of the globe. But probably not a trace of this will be found in the stratified beds of future ages, formed out of the destruction of the present land-surfaces. Even the very arctic shell-beds themselves, which have afforded to the geologist such clear proofs of a frozen sea during the glacial epoch, will not be found in those stratified rocks; for they must suffer destruction along with everything else which now exists above the sea-level. There is probably not a single relic of the glacial epoch which has ever been seen by the eye of man that will be found in the stratified rocks of future ages. Nothing but what lies buried in the deep recesses of the ocean will escape complete disintegration and appear imbedded in those formations. It is only those objects which lie in our existing sea-bottoms that will remain as monuments of the glacial epoch of the Posttertiary period. And, besides, it will only be those portions of the sea-bottoms that may happen to be upraised into dry land that will be available to the geologist of future ages. The point to be determined now is this:—*Is it probable that the geologist of the future will find in the rocks formed out of the now existing sea-bottoms more evidence of a glacial epoch during Posttertiary times than we now do of one during the Miocene, the Eocene, or the Permian period?* Unless this can be proved to be the case, we have no ground whatever to conclude that the cold periods of the Miocene, Eocene, and Permian periods were not as severe as that of the glacial epoch. This is evident; for the only relics which now remain of the glacial epochs of those periods are simply what happened to be protected in the then existing sea-bottoms. Every vestige that lay on the land would in all probability be destroyed by subaërial agency and carried into the sea in a sedimentary form. But before we can determine whether or not there is more evidence of the glacial epoch in our now existing sea-bottoms than there is of former glacial epochs in the stratified rocks (which are in reality the sea-bottoms belonging to ancient epochs), we must first ascertain what is the nature of those marks of glaciation which are to be found in a sea-bottom.

We know that the rocky face of the country was ground down and striated during the glacial epoch; and this is now generally believed to have been done by land-ice. But we have no direct evidence that the floor of the ocean, beyond where it may have been covered with land-ice, was striated. Beyond the limits of the land-ice it could be striated only by means of icebergs. But do icebergs striate the rocky bed of the ocean? Are they adapted for such work? It seems to be almost universally assumed that they are. But I have been totally unable to find any rational grounds for such a belief. Clean ice can have but little or no erosive power, and never could scratch a rock. To do this it must have grinding materials in the form of sand, mud, or stones. But the bottoms of icebergs are devoid of all such materials. Icebergs carry the grinding materials on their backs, not on their bottoms. No doubt, when the iceberg is launched into the deep, great masses of sand, mud, and stones will be adhering to its bottom. But no sooner is the berg immersed, than a melting process commences at its sides and lower surface in contact with the water; and the consequence is, the materials adhering to the lower surface soon drop off and sink to the bottom of the sea. The iceberg, divested of these materials, can now do very little harm to the rocky sea-bottom over which it floats. It is true that an iceberg moving with a velocity of a few miles an hour, if it came in contact with the sea-bottom, would, by the mere force of concussion, tear up loose and disjointed rocks, and hurl some of the loose materials to a distance; but it would do but little in the way of grinding down the rock against which it struck. But even supposing the bottom of the iceberg were properly shoed with the necessary grinding materials, still it would be but a very inefficient grinding agent; for a *floating* iceberg would not be in contact with the sea-bottom. And if it were in contact with the sea-bottom, it would soon become stranded and, of course, motionless, and under such conditions could produce no effect.

It is perfectly true that although the bottom of the berg may be devoid of grinding materials, yet these may be found lying on the surface of the submarine rock over which the ice moves. But it must be borne in mind that the same current which will move the icebergs over the surface of the rock will move the sand, mud, and other materials over it also; so that the markings effected by the ice will in all probability be erased by the current. In the deep recesses of the ocean the water has been found to have but little or no motion. But icebergs always follow the path of currents; and it is very evident that at the comparatively small depth of a thousand feet or so reached by icebergs the motion of the water will be considerable; and the continual shifting of the small particles of the mud and sand will in all probability efface the markings which may be made now and again by a passing berg.

Much has been said regarding the superiority of icebergs as grinding and striating agents in consequence of the great velocity of their motion in comparison with that of land-ice. But it must be remembered that it is while the iceberg is floating, and before it touches the rock, that it possesses high velocity. When the iceberg runs aground, its motion is suddenly arrested or greatly reduced. But if the iceberg advancing upon a sloping sea-bottom is raised up so as to exert great pressure, it will on this account be the more suddenly arrested, the motion will be slow, and the distance passed over short, before the berg becomes stranded. If it exerts but little pressure on the sea-bottom, it may retain a considerable amount of motion and advance to a considerable distance before it is brought to a stand; but, exerting little pressure, it can perform but little work. Land-ice moves slowly, but then it exerts enormous pressure. A glacier 1000 feet in thickness has a pressure on its rocky bed equal to about 25 tons on the square foot; but an iceberg a mile in thickness, forced up on a sloping sea-bottom to an elevation of

20 feet (and this is perhaps more than any ocean-current could effect), would only exert a pressure of about half a ton on the square foot, or about $\frac{1}{50}$ part the pressure of the glacier 1000 feet in thickness. A great deal has been said about the erosive and crushing-power of icebergs of enormous thickness, as if their thickness gave them any additional pressure. An iceberg 100 feet in thickness will exert just as much pressure as one a mile in thickness. The pressure of an iceberg is not like that of a glacier, in proportion to its thickness, but to the height to which it is raised out of the water. An iceberg 100 feet in thickness raised 10 feet will exert exactly the same pressure as one a mile in thickness raised to an equal height.

To be an efficient grinding agent, steadiness of motion, as well as pressure, is essential. A rolling or rocking motion is ill-adapted for grinding down and striating a rock. A steady rubbing motion under pressure is the thing required. But an iceberg is not only deficient in pressure, but also deficient in steadiness of motion. When an iceberg moving with considerable velocity comes on an elevated portion of the sea-bottom, it does not move steadily onwards over the rock, unless the pressure of the berg on the rock be trifling. The resistance being entirely at the bottom of the iceberg, its momentum, combined with the pressure of the current, applied wholly above the point of resistance tends to make the berg bend forward, and in some cases upset (when it is of a cubical form). The momentum of the moving berg, instead of being applied in forcing it over the rock against which it comes in contact, is probably all consumed in work against gravitation in raising the berg upon its front edge. After the momentum is consumed, unless the berg be completely upset, it will fall back under the force of gravitation to its original position. But the momentum which it acquires from gravitation in falling backwards carries it beyond its position of repose in an opposite direction. It will thus continue to rock backwards and forwards until the friction of the water brings it to rest. The momentum of the berg, instead of being applied to the work of grinding and striating the sea-bottom, will chiefly be consumed in heat in the agitation of the water. But if the berg does advance, it will do so with a rocking unsteady motion, which, as Mr. Couthouy* and Professor Dana† observe, will tend rather to obliterate striations than produce them.

A floating berg moves with great steadiness; but a berg that has run aground cannot advance with a steady motion. If the rock over which the berg moves offers little resistance, it may

* Report on Icebergs, read before the Association of American Geologists. Silliman's Journal, vol. xliii. p. 163 (1842).

† Manual of Geology, p. 677.

do so; but in such a case the berg could produce but little effect on the rock.

Dr. Sutherland, who has had good opportunities to witness the effects of icebergs, makes some most judicious remarks on the subject. "It will be well," he says, "to bear in mind that when an iceberg *touches the ground, if that ground be hard and resisting, it must come to a stand,* and, the propelling power continuing, a slight leaning over in the water, or yielding motion of the whole mass, may compensate readily for being so suddenly arrested. If, however, the ground be soft, so as not to arrest the motion of the iceberg at once, a moraine will be the result; but the moraine thus raised will tend to bring it to a stand"*.

There is another cause referred to by Professor Dana, which, to a great extent, must prevent the iceberg from having an opportunity of striating the sea-bottom, even though it were otherwise well adapted for so doing. It is this: the bed of the ocean in the track of icebergs must be pretty much covered with stones and rubbish dropped from the melting bergs. And this mass of rubbish will tend to protect the rock†.

If icebergs cannot be shown *à priori,* from mechanical considerations, to be well adapted for striating the sea-bottom, one would naturally expect, from the confident way in which it is asserted that they are so adapted, that the fact has been at least established by actual observation. But, strange as it may appear, we seem to have little or no proof that icebergs actually striate the bed of the ocean. This can be proved from the direct testimony of the advocates of the iceberg theory themselves.

We shall take the testimony of Mr. Campbell, the author of two well-known works in defence of the iceberg theory, viz. 'Frost and Fire' and 'A Short American Tramp.' Mr. Campbell went in the fall of the year 1864 to the coast of Labrador, the Straits of Belle Isle, and the Gulf of St. Lawrence, for the express purpose of witnessing the effects of icebergs, and testing the theory which he had formed, that the ice-markings of the glacial epoch were caused by floating ice and not by land-ice, as is now generally believed.

The following is the result of his observations on the coast of Labrador.

Hanly Harbour, Strait of Belle Isle:—"The water is 37° F. in July. . . . As fast as one island of ice grounds and bursts, another takes its place; and in winter the whole strait is blocked up by a mass which swings bodily up and down, grating along the bottom at all depths. . . . Examined the beaches and rocks at the water-line, especially in sounds. Found the rocks ground

* Quart. Journ. Geol. Soc. vol. ix. p. 306.

† Dana's 'Manual of Geology,' p. 677.

smooth, *but not striated,* in the sounds."—Short American Tramp, pp. 68, 107.

Cape Charles and Battle Harbour:—"But though these harbours are all frozen every winter, the *rocks at the water-line are not striated*" (p. 68).

At St. Francis Harbour:—"The water-line is much rubbed, smooth, *but not striated*" (p. 72).

Cape Bluff:—"Watched the rocks with a telescope, and *failed to make out striæ anywhere*; but the water-line is everywhere rubbed smooth" (p. 75).

Seal Islands:—"*No striæ are to be seen at the land-wash in these sounds or on open sea-coasts near the present water-line*" (p. 76).

He only mentions having here found striations in the three following places along the entire coast of Labrador visited by him; and in regard to two of these, it seems very doubtful that the markings were made by modern icebergs.

Murray's Harbour:—"This harbour was blocked up with ice on the 20th of July. The water-line is rubbed, and in *some places* striated" (p. 69).

Pack Island:—"The water-line in a narrow sound was polished and striated in the direction of the sound, about N.N.W. This seems to be fresh work done by heavy ice drifting from Sandwich Bay; *but, on the other hand, stages with their legs in the sea, and resting on these very rocks, are not swept away by the ice*" (p. 96). If these markings were modern, why did not the "heavy ice" remove the small fir poles supporting the fishing-stages?

Red Bay:—"Landed half-dressed, and found some striæ perfectly fresh at the water-level, but weathered out a short distance *inland*" (p. 107). The striations "inland" could not have been made by modern icebergs; and it does not follow that because the markings at the water-level were not weathered they were produced by modern ice.

These are the evidences which he found that icebergs striate rocks, on a coast of which he says that, during the year he visited it, "the winter-drift was one vast solid raft of floes and bergs more than 150 miles wide and perhaps 3000 feet thick at spots, driven by a whole current bodily over one definite course, year after year, since this land was found" (p. 85).

But Mr. Campbell himself freely admits that the floating ice which comes aground along the shores does not produce striæ. "It is sufficiently evident," he says, "*that glacial striæ are not produced by thin bay-ice*" (p. 76). And in 'Frost and Fire,' vol. ii. p. 237, he states that, "from a careful examination of the water-line at many spots, it appears that bay-ice grinds rocks, *but does not produce striation.*"

"It is impossible," he continues, "to get at rocks over which heavy icebergs now move; but a mass 150 miles wide, perhaps 3000 feet thick in some parts, and moving at the rate of a mile an hour or more, *appears to be an engine amply sufficient* to account for striæ on rising rocks." And in 'American Tramp,' p. 76, he says, "*striæ must be made* in deep water by the large masses which seem to pursue the even tenor of their way in the steady current which flows down the coast."

Mr. Campbell, from a careful examination of the sea-bottom along the coast, finds that the small icebergs do not produce striæ, but the large ones, which move over rocks impossible to be got at, "must" produce them. They "appear" to be amply sufficient to do so. If the smaller bergs cannot striate the sea-bottom, why must the larger ones do so? There is no reason why the smaller bergs should not move as swiftly and exert as much pressure on the sea-bottom as the larger ones. And even supposing that they did not, one would expect that the light bergs would effect on a smaller scale what the heavy ones would do on a larger.

I have no doubt that when Mr. Campbell visited Labrador he expected to find the sea-coast under the water-line striated by means of icebergs, and was probably not a little surprised to find that it actually was not. And I have no doubt that were the sea-bottom in the tracks of the large icebergs elevated into view, he would find to his surprise that it was free from striations also.

So far as observation is concerned, we have no grounds from what Mr. Campbell witnessed to conclude that icebergs striate the sea-bottom.

The testimony of Dr. Sutherland, who has had opportunities of seeing the effects of icebergs in Arctic regions, leads us to the same conclusion. "Except," he says, "from the evidence afforded by plants and animals at the bottom, we have *no means whatever* to ascertain the effect produced by icebergs upon the rocks*. In the Malegat and Waigat I have seen whole clusters of these floating islands, drawing from 100 to 250 fathoms, moving to and fro with every return and recession of the tides. I looked very earnestly for grooves and scratches left by icebergs and glaciers in the rocks, but always failed to discover any"†.

We shall now see whether river-ice actually produces striations or not. If floating ice under any form can striate rocks, one would expect that it ought to be done by river-ice, seeing that such ice is obliged to follow one narrow definite track.

St. John's River, New Brunswick:—"This river," says Mr. Campbell, "is obstructed by ice during five months of the year.

* Quart. Journ. Geol. Soc. vol. ix. p. 306.

† Journal, vol. i. p. 38.

When the ice goes, there is wild work on the bank. Arrived at St. John, drove to the suspension-bridge. . . . At this spot, if *anywhere in the world*, river-ice ought to produce striation. The whole drainage of a wide basin, and one of the strongest tides in the world, here work continually in one rock-groove; and in winter this water-power is armed with heavy ice. *There are no striæ* about the water-line "*.

River St. Lawrence:—"In winter the power of ice-floats driven by water-power is tremendous. The river freezes and packs ice till the flow of water is obstructed. The rock-pass at Quebec is like the Narrows at St. John's, Newfoundland, in the frontispiece. The whole pass, about a mile wide, was paved with great broken slabs and round boulders of worn ice as big as small stacks, piled and tossed, and heaped and scattered upon the level water below and frozen solid. . . . This kind of ice does NOT *produce striation* at the water-margin at Quebec. At Montreal, when the river 'goes,' the ice goes with it with a vengeance. . . . The *piers are not yet striated* by river-ice at Montreal. . . . The rocks at the high-water level have *no trace* of glacial striæ. . . . The rock at Ottawa is rubbed by river-ice every spring, and *always in one direction, but it is not striated*. . . . The surfaces are all rubbed smooth, and the edges of broken beds are rounded where exposed to the ice; *but there are no striæ* "†.

When Sir Charles Lyell visited the St. Lawrence in 1842, at Quebec he went along with Colonel Codrington "and searched carefully below the city in the channel of the St. Lawrence, at low water, near the shore, for the signs of glacial action at the precise point where the chief pressure and friction of packed ice are exerted every year," but found none.

"At the bridge above the Falls of Montmorenci, over which a large quantity of ice passes every year, the gneiss is polished, and kept perfectly free from lichens, but not more so than rocks similarly situated at waterfalls in Scotland. In none of these places were any long straight grooves observable "‡.

The only thing in the shape of modern ice-markings which he seems to have met with in North America was a few straight furrows ½ an inch broad in soft sandstone, at the base of a cliff at Cape Blomidon in the Bay of Fundy, at a place where during the preceding winter "packed" ice 15 feet thick had been pushed along when the tide rose over the sandstone ledges §.

The very fact that a geologist so eminent as Sir Charles Lyell, after having twice visited North America, and searched specially for modern ice-markings, was able to find only two or

* Short American Tramp, pp. 168, 174. † Ibid. pp. 239–241.

‡ Travels in North America, vol. ii, p. 137.

§ Ibid. vol. ii. p. 174.

three scratches, upon a soft sandstone rock, which he could reasonably attribute to floating ice, ought to have aroused the suspicion of the advocates of the iceberg theory that they had really formed too extravagant notions regarding the potency of floating ice as a striating agent.

There is no reason to believe that the grooves and markings noticed by M. Weibye and others on the Scandinavian coast and other parts of northern Europe were made by icebergs. Mr. Geikie has clearly shown, from the character and direction of the markings, that they are the production of land-ice*. If the floating ice of the St. Lawrence and the icebergs of Labrador are unable to striate and groove the rocks, it is not likely that those of northern Europe will be able to do so.

It will not do for the advocates of the iceberg theory to assume, as they have hitherto done, that, as a matter of course, the sea-bottom is being striated and grooved by means of icebergs. They must prove that. They must either show that, as a matter of fact, icebergs are actually efficient agents in striating the sea-bottom, or prove from mechanical principles that they must be so. The question must be settled either by observation or by reason; mere opinion will not do.

The transporting of boulders and rubbish, and not the grinding and striating of rocks, is evidently the proper function of the iceberg. But even in this respect I fear too much has been attributed to it.

In reading the details of voyages in the Arctic regions one cannot help feeling surprised how seldom reference is made to stones and rubbish being seen on icebergs. Arctic voyagers, like other people when they are alluding to the geological effects of icebergs, speak of enormous quantities of stones being transported by them; but in reading the details of their voyages, the impression conveyed is that icebergs with stones and blocks of rock upon them are the exceptions. The greater portion of the narratives of voyages in Arctic regions consists of interesting and detailed accounts of the voyagers' adventures among the ice. The general appearance of the icebergs, their shape, their size, their height, their colour are all noticed; but rarely is mention made of stones being seen. That the greater number of icebergs have no stones or rubbish on them is borne out by the positive evidence of geologists who have had opportunities of seeing icebergs.

Mr. Campbell says:—"It is remarkable that up to this time we have only seen a few doubtful stones on bergs which we have passed. . . . Though no bergs with stones *on them or in them*

* Proceedings of the Royal Society of Edinburgh, Session 1865–66, page 537.

have been approached during this voyage, many on board the 'Ariel' have been close to bergs heavily laden. . . . . . A man who has had some experience of ice has *never seen a stone on a berg* in these latitudes. Captain Anderson of the 'Europa,' who is a geologist, has *never seen a stone on a berg* in crossing the Atlantic. *No stones were clearly seen on this trip*"*. Captain Sir James Anderson (who has long been familiar with geology, has spent a considerable part of his life on the Atlantic, and has been accustomed to view the iceberg as a geologist as well as a seaman) has never seen a stone on an iceberg in the Atlantic. This is rather a significant fact.

Sir Charles Lyell states that, when passing icebergs on the Atlantic, he "was most anxious to ascertain whether there was any mud, stones, or fragments of rocks on any one of these floating masses; but after examining about forty of them without perceiving any signs of frozen matter, I left the deck when it was growing dusk"†. After he had gone below, one was said to be seen with something like stones upon it. The Captain and officers of the ship assured him that they had *never seen a stone upon a berg*.

There is still another point connected with icebergs to which we must allude, viz. the opinion that great masses of the boulder-clay of the glacial epoch was formed from the droppings of icebergs. It is perfectly obvious that *unstratified* boulder-clay could not have been formed in this way. Stones, gravel, sand, clay, and mud, the ingredients of boulder-clay, tumbled all together from the back of an iceberg, could not sink to the bottom of the sea without separating. The stones would reach the bottom first, then the gravel, then the sand, then the clay, and last of all the mud, and the whole would settle down in a stratified form. But, besides, how could the *clay* be derived from icebergs? Icebergs derive their materials from the land before they are launched into the deep, and while they are in the form of land-ice. The materials which are found on the backs of icebergs are what fell upon the ice from mountain tops and crags projecting above the ice. Icebergs are chiefly derived from continental ice, such as that of Greenland, where the whole country is buried under one continuous mass, with only a lofty mountain peak here and there rising above the surface. And this is no doubt the chief reason why so few icebergs have stones upon their backs. The continental ice of Greenland is not, like the glaciers of the Alps, covered with loose stones. It is perfectly plain that clay does not fall upon the ice. What falls upon the ice is stones, blocks of rocks, and the loose débris. Clay and

* Short American Tramp, pp. 77, 81, 111.
† Second visit, vol. ii. p. 367.

mud we know, from the accounts given by arctic voyagers, are sometimes washed down upon the coast-ice; but certainly very little of either can possibly get upon an iceberg. Arctic voyagers sometimes speak of seeing clay and mud upon bergs; but it is probable that if they had been near enough they would have found that what they took for clay and mud was merely dust and rubbish.

Undoubtedly the boulder-clay of many places bears unmistakeable evidence of having been formed under water; but it does not on that account follow that it was formed from the droppings of icebergs. The fact that the boulder-clay in every case *is chiefly composed of materials derived from the country on which the clay lies*, proves that it was not formed from matter transported by icebergs. The clay no doubt contains stones and boulders belonging to other countries, which evidently have been transported by icebergs; but the clay itself has not come from another country. But if the clay itself has been derived from the country on which it lies, then it is absurd to suppose that it was deposited from icebergs. The clay and materials which are found on icebergs are derived from the land on which the iceberg is formed; but to suppose that icebergs, after floating about upon the ocean, should always return to the country which gave them birth and deposit their loads is rather an extravagant supposition. And if they did not do that, then how could the boulder-clay always be derived from the country in which it is found?

Boulder-clay, whether formed on dry land or under water, is evidently the product of land-ice. Boulder-clay will be formed under land-ice whether the ice be moving on dry land or along the sea-bottom. In many parts of England, as at Norwich for example, the boulder-clay contains huge blocks which have evidently come from Scandinavia and other distant parts on the back of icebergs. These have been dropped among the clay then being brought down under the land-ice. In fact during the glacial epoch, unless the seas around a country were completely blocked up by land-ice, it would be impossible to prevent icebergs from coming and discharging their loads occasionally amongst the clay forced from under the land-ice. Few Scandinavian or other far transported blocks are found in the lower boulder-clay of Scotland; and this is owing, no doubt, to the fact that the seas surrounding that country during the glacial epoch were so completely blocked up by land-ice that no berg could possibly approach near to the coast.

If we simply admit (what follows from theory) that as the ice began to accumulate on the land the sea began to rise, and that the time of the greatest extension of the ice was also the time of the greatest submergence, many a difficulty will disappear.

The view which at one time prevailed in regard to this point was, that at the period of continental ice the land stood much higher than at present, and that as the cold began to abate, the land began slowly to sink and went down under the sea covered with ice. The land, after remaining for a long course of ages under water, ultimately emerged still covered with ice; but, the cold continuing to abate, the general covering of ice began to break up, and then followed the period of local glaciers.

I have never been able to find any proof given, either geological or physical, that at the time that the country was enveloped in ice the land stood higher. Instead of a greater elevation of the land during the period of ice, geological facts seem to prove the reverse; for in every country where we have evidence of glaciation, we have also evidence of a corresponding submergence of the land. The two so constantly accompany each other, that many geologists have been led to suspect the existence of some physical bond of connexion between them. We have good evidence, no doubt, that the land in relation to the sea must have stood much higher as well as much lower than it does at present, but we have no evidence that the country was at that particular time covered with ice. On the contrary, geological facts go rather to show that the land was then covered with forests and peat-bogs, and possessed an abundance of animal life.

It follows from theory, as has been shown on former occasions*, that during the cold periods of the glacial epoch the sea must have stood much higher than at present, and during the warm periods much lower. When certain considerations are taken into account which were overlooked on former occasions, a submergence to the extent of at least 500 feet may be accounted for. Although the repeated submergence and emergence of the land during Posttertiary times were probably due to oscillations of the sea rather than of the land, still we have evidence that there must have been local elevations and depressions of the land during the glacial epoch; for we find in some places that the land was submerged to the extent of upwards of 1000 feet, while in other parts it remained above water altogether. The oscillations of the sea-level resulting from cosmical causes would not in any way interfere with upheavals and depressions of the land resulting from volcanic or other agencies.

It was evidently not the facts of geology, but the notion that the cold of the glacial epoch must have resulted from a greater elevation of the land, which gave rise to the opinion that the country stood much higher than at present at the time that it was covered with ice. In order to account for the cold of the

* Phil. Mag. April 1866. 'Reader,' September 2 & 14, 1865; November 27, 1865.

glacial epoch and the general covering of ice, the land was first elevated; and then to make matters harmonize with the facts of geology, the land was submerged.

There is no necessity for this hypothesis in order to account for the cold of the glacial epoch; and even supposing that there was, it would not suit. The cold and ice of the glacial epoch were too general to be accounted for upon the hypothesis of elevation of the land. It is the elevation of a part *in relation to the surrounding* continents that can cool the climate of that part and cover it with ice. A general elevation would produce but little effect; for in such a case the atmosphere would to a considerable extent rise along with the land, and no very sensible change would result. We shall have to return to this part of the subject on a future occasion.

From the facts and considerations adduced we are, I would venture to presume, warranted to conclude that, with the exception of what may have been produced by land-ice, very little in the shape of boulder-clay or striated rocks belonging to the glacial epoch lie buried under the ocean—and that when the now existing land-surfaces are all denuded, probably scarcely a trace of the glacial epoch will then be found, except the huge blocks that were transported by icebergs and dropped into the sea. It is therefore probable that we have as much evidence of the existence of a glacial epoch during the Miocene, Eocene, and Permian periods as the geologists of future ages will have of the existence of a glacial epoch during the Posttertiary period, and that consequently we have no warrant whatever to conclude that the glacial epoch was a something unique in the geological history of our globe.

It might be thought that if glacial epochs have been so numerous as Table No. I. represents, we ought to have abundance of palæontological evidence of their existence. I do not know if this necessarily follows. Take the glacial epoch itself, quite a modern affair. Here we do not require to go and search in the bottom of the sea for the evidence of its existence; for we have the surface of the land in almost identically the same state in which it was when the ice left it, with the boulder-clay and all the wreck of the ice lying upon it. But what geologist, with all these materials before him, would be able to find out from palæontological evidence alone that there had even been such an epoch? He might search the whole, but would not be able to find fossil evidence from which he would be warranted to infer that the country had ever been covered with ice. We have evidence in the fossils of the Crag and other deposits of the existence of a colder condition of climate prior to the true glacial period, and in the shell-beds of the Clyde and other places of a cold condition of climate after the true glacial period. But in regard to the period of the true boulder-clay or till, when the country was enveloped in ice, all is almost, comparatively speaking, a perfect blank so far as palæontology is concerned. "Whatever may be the cause," says Sir Charles Lyell, "the fact is certain that over large areas in Scotland, Ireland, and Wales, I might add throughout the northern hemisphere on both sides of the Atlantic, the stratified drift of the glacial period is very commonly devoid of fossils"*.

In the "flysch" of the Eocenes of the Alps, in which the huge blocks are found which prove the existence of ice-action during that period, few or no fossils have been found. So devoid of organic remains is that formation, that it is only from its position, says Sir Charles, that it is known to belong to the middle or "nummulitic" portion of the great Eocene series. Again, in the conglomerates at Turin, belonging to the Upper-Miocene period, in which the angular blocks of limestone are found which prove that during that period Alpine glaciers reached the sea-level in the latitude of Italy, not a single organic remain has been found. It would seem that an extreme paucity of organic life is a characteristic of a glacial period, which warrants us to conclude that the absence of organic remains in any formation otherwise indicative of a cold climate cannot be regarded as sufficient evidence that that formation does not belong to a cold period.

But if there is a deficiency of direct positive evidence of a general glaciation of the northern hemisphere during the Middle-Eocene, Upper-Miocene, and other periods similar to what we know took place during the Postpliocene period, there is, however, abundance of indirect evidence in favour of it.

Those facts to which I allude that appear to lead indirectly to the conclusion that a general condition of glaciation must have prevailed during the Upper-Miocene and other periods, are, strange to say, the very facts which have all along been adduced as a reason against the possibility of a cold condition of climate at those periods—notwithstanding the positive evidence which we have that the Alps and the Pyrenees† must have possessed enormous glaciers during that period, those of the former reaching the sea-level in the latitude of Italy.

If a cold and glacial condition of climate prevailed at those periods, we may be perfectly certain that a very warm and equable condition of climate must have also prevailed immediately before

* Antiquity of Man, p. 268 (third edition).

† For an account of the evidence of a glaciation of the Pyrenees during the Miocene period, see a paper "On the Glacial Phenomena of the Pyrenees," by Mr. P. W. Stuart Menteath, read before the Edinburgh Geological Society, 1867. See also *Bulletin de la Société Ramond*, 1866.

or immediately after those periods, or perhaps both before and after the cold period. This follows as a necessary consequence from the cosmical theory of climate. For it is physically impossible that we can have a cold and Arctic condition of climate on the one hemisphere, resulting from a great increase of excentricity, without at the same time having a warm, equable, if not an almost tropical, condition of climate prevailing on the other hemisphere. Whether a high state of excentricity will produce in our northern hemisphere a cold and glacial condition of climate or a warm and equable condition, depends solely upon the position of the winter solstice in relation to the perihelion at the time. But if the excentricity continues at a high value during a long course of ages, then, owing to the precession of the equinoxes and motion of the perihelion, the conditions of climate on the two hemispheres will be reversed every ten or twelve thousand years. Consequently when we find, as we do in the Upper-Miocene period and Middle-Eocene period, evidence of a cold condition of climate, we may reasonably expect to find also evidence of a much warmer and more equable condition than now prevails. And this is actually what we do find. "The Upper-Miocene flora and fauna of the whole of Central Europe," says Sir Charles Lyell, "afford unmistakeable evidence of a climate approaching that now only experienced in subtropical regions." "In the present state of the globe the island of Madeira presents the nearest approach to such a flora. The proportion of arborescent as compared to the herbaceous plants is very great; and among the former the predominance of evergreens implies an absence of severe winter cold"*. And in regard to the conglomerates of the Superga, near Turin, in which the erratic blocks are found, "the fauna and flora both of the overlying and underlying rock," says Sir Charles, "have the same subtropical character as that of Miocene date in Switzerland and Central Europe generally." "Hence," he continues, "the hypothesis of the transport of such huge blocks by ice-action has naturally been resorted to most unwillingly; but in the present state of our knowledge it is the only one which appears tenable."

Here is a case which, if found to be general, ought to settle the whole question of geological climate. Here is a bed of conglomerate indicating a cold and arctic condition of things when it was formed, with icebergs floating around the place now occupied by the city of Turin, overlain and underlain conformably by strata indicating a subtropical condition of climate. But this is not all. This warm condition of things was not confined to Central Europe, but extended probably over the entire northern

* 'Principles,' vol. i. p. 200 (10th edition).

hemisphere, even up to the North Pole; for it has been proved that Greenland at this time must have been free of ice and covered with a luxuriant flora. (See Heer's *Flora Fossilis Arctica.*) The facts are wholly inexplicable on the ordinary theories of climate. The cosmical theory, however, not only explains them, but they follow according to this theory as a necessary consequence; for if it should actually turn out that there is no such thing as a warm and equable condition of climate somewhere about the time of an ice-period, then the whole theory would have to be given up, because a warm period, according to theory, is just as necessary a result of an increase of excentricity as a cold period.

The occurrence of a warm condition of climate close beside a cold condition is not a mere accidental circumstance which has only been observed during the Upper-Miocene period. For when we go back to the Middle-Eocene period, we find the "flysch," which bears the marks of having been formed during an ice-period, closely associated with the nummulitic strata, indicating a warm condition of climate. "It has always been objected," says Sir Charles Lyell, "to the hypothesis that these huge masses were transported to their present sites by glaciers or floating ice, that the Eocene strata of nummulitic age in Switzerland, as well as in other parts of Europe, contain genera of fossil plants and animals characteristic of a warm climate. It has been particularly remarked by M. Desor that the strata most *nearly associated* with the flysch in the Alps are rich in echinoderms of the Spatangus family, which have a decidedly tropical aspect."

Passing back to the Cretaceous period, we find, closely associated with the floating ice in the sea of the White Chalk, fossil evidence of a warm condition of climate. And then, if we go back to the Permian period, we find glaciers reaching the sea-level in the very centre of England, and other indications of an age of ice, as has been clearly proved by Professor Ramsay. But the fossil remains of the Permian period declare emphatically the prevalence of a warm and equable condition of climate also during that age.

Sir Roderick Murchison has done me the honour to refer, in the last edition of the 'Siluria,' page 548, to my views on the occurrence of glacial epochs during past ages. He is opposed to the opinion held by many geologists and expressed in one of my papers, that there probably was ice-action during the Old-Red-Sandstone and other early periods. Supposing that it were proved that there was no ice during the Palæozoic age, this would not affect the cosmical theory in the least degree; for it is quite possible, as has been already shown*, that the direct heat of the

* Phil. Mag. S. 4. vol. xxxv. p. 373.

sun may have been greater during the earlier periods of the earth's history. It must also be borne in mind that the simple fact of the fauna and flora of that age indicating a warm and equable condition of climate is not a sufficient proof that there were no cold periods; for a warm and equable condition of climate is just as necessary a result of an increase of excentricity as a cold condition; and the warm periods may be represented by organic remains, and the cold periods not.

Returning to the Postpliocene period. If this period afforded no geological evidence of a warmer condition of climate in Europe than now prevails, it would be so far a presumptive evidence against the assumption that the glacial epoch resulted from cosmical causes. But we have undoubted evidence that the climate of Europe during a portion of that period was much warmer and more equable than at present; for we find evidence of animals and shells existing in latitudes where they could not now live in consequence of the cold. The *Cyrena fluminalis* is a shell which does not live at present in any European river, but inhabits the Nile and parts of Asia, and especially of Cashmere. The *Unio littoralis,* extinct in Britain, is still abundant in the Loire. The *Paludina marginata* does not exist in this country, but inhabits the more southern parts of Europe. These shells have been found in Posttertiary deposits at Gray's Thurrock in Essex, in the valley of the Ouse near Bedford, at Hoxne in Suffolk, in the fluviomarine beds of the Norfolk cliffs, in the freshwater formation at Mundesley, and other parts of England. Along with these shells of a southern type have been found the bones of the Hippopotamus, of a species closely allied to that now inhabiting the Nile, and the *Elephas antiquus,* an animal remarkable also for its southern range, and the *Rhinoceros megarhinus.* But what is most remarkable, along with these have been found such animals of an arctic type as the Mammoth, the woolly Rhinoceros, and the Reindeer. How these could all have lived under the same conditions of climate has long been a puzzle to geologists. But many geologists now, as Sir Charles Lyell* and Mr. Boyd Dawkins†, are inclined to believe that these did not all live under the same conditions of climate, but imply oscillations of climate. During a cold condition of climate the Mammoth and other arctic animals would move southwards; and when the climate assumed a warm and equable condition, the animals and shells of a southern type would migrate northwards.

This warm condition of climate was not confined to temperate regions, but extended to high northern latitudes; for we find all over the Arctic regions remains of ancient forests where now the

* 'Principles,' vol. i. p. 193 (tenth edition).

† Quart. Journ. Geol. Soc. May 1867, p. 104.

entire country is covered with snow and ice. The trunk of a white spruce tree was dug up by Sir E. Belcher near Wellington Sound, in lat. 75° 32′ N. The remains of an ancient forest were discovered by Captain M'Clure in Bank's Land in lat. 74° 48′. "This remarkable phenomenon," says Captain M'Clure, "opens a vast field for conjecture; and the imagination becomes bewildered in trying to realize that period in the world's history, when the absence of ice and a milder climate allowed forest-trees to grow in a region where now the ground-willow and dwarf-birch have to struggle for existence." Evidence of ancient forests was found in Prince Patrick's Island and in Melville Island, one of the coldest spots, perhaps, in the northern hemisphere.

These phenomena have excited a very great amount of astonishment, and have been considered very puzzling; but they need not be so regarded if it be the case that the climate of the globe is greatly affected by variations in the excentricity of its orbit; for in such a case there could not possibly be a glacial epoch extending over a long course of time without the frequent recurrence of warm periods. From geological evidence alone, independently of cosmical considerations, we are warranted by analogy from the Posttertiary period to expect the existence of a glacial condition of things during the Upper-Miocene and Middle-Eocene period, from the fact that a warm condition of climate prevailed in Europe and extended to high latitudes during a part of those periods, similar to what we know occurred in the Posttertiary period.

The occurrence of a warm condition of climate always along with a cold condition shows that the two are in some way or other physically connected, and that they are both related to a common cause.

At one time I was under the impression that when the excentricity was near to its superior limit and the winter solstice in the perihelion, the mean annual temperature would not in the temperate regions be higher than it is at the present day, although in all probability at that time the winters would be as warm as the summers, the effect of this condition of things being to produce what Sir John Herschel has called a "perpetual spring." But after making calculations regarding the amount of energy in the form of heat that is being continually transferred from the tropical regions to the temperate and polar regions by ocean-currents, and to what extent the increase in the volume of those currents, which would result from that condition of things, would affect the climate, I felt persuaded that that opinion must be abandoned, and that a "perpetual summer" would better represent the condition of climate which must then prevail.

In the discussion of the subject of geological climate another point has been greatly overlooked, viz. the enormous influence that a mantle of ice continually lying on the continent of Greenland has upon the climate of the northern hemisphere. If we could by some means or other remove this cold mantle off that continent, though all things else should remain the same, the effect that its simple removal would have on the climate of the entire northern hemisphere would be quite magical. On a former occasion we referred to the physical reason of this, but shall have occasion to return again to this part of the question.

In looking over Table I., which, as has already been remarked, probably embraces the greater portion of the Tertiary age, the excentricity several times attained pretty high values, although it is only at the three periods embraced in Tables II., III., and IV. that the values reached would be likely to lead to a condition of things during the past three millions of years that might be properly designated a glacial epoch. But as the excentricity during the greater part of that vast period was greater than it is at the present day (for we are just now much nearer to the inferior limit than we are to the superior limit), its effect on climate would be more marked than it is at present. The effect would be that, as a general rule, during the greater portion of the three millions of years past, the climate would be somewhat colder when the winters occurred in the aphelion than it is at the present day, and a good deal warmer when they occurred in the perihelion than at present.

Judging from geological evidence alone, we naturally conclude that, as a general rule, the climate of the Tertiary period was somewhat warmer than it is at the present day. It is from fossil remains that the geologist principally forms his estimate of the character of the climate during any period. Now, in regard to fossil remains, the warm periods will always be far better represented than the cold; for we find that, as a *general rule, those formations which geologists are inclined to believe indicate a cold condition of climate are remarkably devoid of fossil remains.* If a geologist does not keep this principle in view, he will be very apt to form a wrong estimate of the general character of the climate of a period of such enormous length as the Tertiary.

Suppose that the presently existing sea-bottoms, which have been forming since the commencement of the glacial epoch, were to become consolidated into rock and then elevated into dry land, we should then have a formation which might be properly designated the Postpliocene. It would represent the time which has elapsed from the beginning of the glacial epoch to the present day. Suppose one to be called upon as a geologist to determine from that formation what was the general character of the climate during the period in question, what would probably be the conclusion at which he would arrive? He would probably find here and there patches of boulder-clay containing striated and ice-worn stones. Now and again he would meet with bones of the mammoth and the reindeer, and shells of an Arctic type. He would likewise find huge blocks of the older rocks imbedded in the formation, from which he would infer the existence of icebergs and glaciers reaching the sea-level. But, on the whole, he would find that the greater portion of the fossil remains met with in this formation implied a warm and temperate condition of climate. At the lower part of the formation, corresponding to the time of the true boulder-clay, there would be such a scarcity of organic remains that he would probably feel at a loss to say whether the climate at that time was cold or hot. But if the intense cold of the glacial epoch was not continuous, but was broken up by one or more warm periods*, during which the ice, to a considerable extent at least, disappeared for a long period of time (and there are few geologists who have properly studied the subject that will positively deny that such was the case), then the country would no doubt during those warm periods possess an abundance of plant and animal life. It is very probable that it was during those periods that the Arctic forests flourished. It is quite true that we may almost search in vain on the present land-surface for the organic remains which belonged to those interglacial periods; for they were nearly all swept away by the ice which followed. But no doubt in the deep recesses of the ocean, buried under hundreds of feet of sand, mud, clay, and gravel, lie multitudes of the plants and animals which then flourished on the land, and were carried down by rivers into the sea. And along with these lie the shells and other marine faunæ which flourished in the warm seas of those periods. A geologist, thus judging from the great abundance of organic remains that this lower portion of the formation would contain indicating a warm condition of climate, and the almost total

* For geological evidence of warm periods during the glacial epoch, see:—Mr. Geikie's memoir "On the Glacial Drift of Scotland;" M. Morlot "On the Posttertiary and Quaternary Formations of Switzerland," Edin. New Phil. Journ., New Series, vol. ii. 1855; Lyell's 'Antiquity of Man,' p. 321, second edition; 'Principles,' vol. i. p. 198, tenth edition; Heer, *Urwelt der Schweiz*; Vogt's 'Lectures on Man,' pp. 318–321; Mr. Edward Hull "On the Drift-deposits in the Neighbourhood of Manchester," Mem. Lit. and Phil. Soc. of Manchester, 1863. For some remarkable facts bearing on the subject, see a valuable memoir by Mr. James Bennie of Glasgow, "On the Surface Geology of the District round Glasgow," Trans. of the Geol. Soc. of Glasgow, vol. iii. part 1. See also a paper by Mr. James Geikie "On the Remains of the *Bos primigenius* found in an Interglacial Bed," Geol. Mag. for September 1868.

absence of fossil remains corresponding to the glacial condition of things which we now know also did prevail at that epoch, would very likely come to the conclusion that the former part of the Postpliocene period was a warm period; whereas we, at the present day, looking at the matter from a different standpoint, declare that part to be a glacial epoch. No doubt, if the beds formed during the cold periods of the glacial epoch could be distinguished from those formed during the warm periods, the fossil remains of the one would indicate a cold condition of climate, and that of the other a warm condition; but still, taking the entire epoch as a whole, the percentage of fossil remains indicative of a warm condition would probably so much exceed that indicative of a cold condition, that we should come to the conclusion that the character of the climate, as a whole, during the epoch in question was warm and equable.

As geologists we have, as a rule, no means of arriving at a knowledge of the character of the climate of any given period but through an examination of the sea-bottoms belonging to that period; for these contain all the evidence upon the subject. But unless we exercise caution, we shall be very apt, in judging of the climate of such a period, to fall into the same error that we have just now seen one would naturally fall into were he called upon to determine the character of the climate during the glacial epoch from the character of the organic remains which lie buried in our adjoining seas.

In conclusion, during the past three millions of years there were three periods when the excentricity attained a high value. These three periods are represented in Tables II., III., and IV. The first began about 2,630,000 years ago, and terminated about 2,460,000 years ago. The second began about 980,000 years ago, and terminated about 720,000 years ago. The third began about 240,000 years ago, and terminated about 80,000 years ago.

The third period, for reasons which have now been considered at some length, I believe to have been that of the Glacial Epoch; the second in all probability was that of the Upper-Miocene period; and the first might probably correspond to the glacial epoch of the Middle-Eocene period.

From the commencement of the glacial epoch, 240,000 years ago, back to the close of the glacial epoch of the Upper-Miocene period, represented in Table III., there is an interval of 480,000 years. Taking the ordinary rate of subaërial denudation, as indicated by the rate at which rivers carry the soil off the land, which we have already seen is not less than about one foot in 6000 years*, it follows that about 80 feet must have been removed off the face of the country during that long interval. It is there-

* Phil. Mag. for May 1868, p. 379. Trans. of Glasgow Geol. Soc. vol. iii. part. 1.

fore probable that almost every trace of the ice of the Miocene period would be removed before the glacial epoch began. From the close of the glacial epoch of the Middle-Eocene period, represented in Table II., to the commencement of the glacial epoch of the Miocene period, there is the enormous interval of 1,480,000 years. During that time no less than 247 feet would be removed off the general level of the country.

From the close of the glacial epoch of the Miocene period to the present day, 120 feet of rock must have been removed from the surface of the land and carried down in the form of sediment into the sea. And since the glacial epoch of the Eocene period, no less than 410 feet must have been removed. We need not, therefore, wonder that so few traces of the ice of those periods remain. Remove 410 feet of rock off the surface of the present existing continents, and where should we then find our "roches moutonnées," striated rocks, boulder-clay, or in fact any evidence whatever on the land that there had been a glacial epoch during the Posttertiary period?

XLVIII. *Note on the Solvibility of Equations by means of Radicals. By* Professor Cayley, *F.R.S.**

In regard to the theorem that the general quintic equation of the nth order is not solvible by radicals, I believe that the proofs which have been given depend, or at any rate that a proof may be given that shall depend, on the following two lemmas:—

I. A one-valued (or symmetrical) function of n letters is a perfect kth power, only when the kth root is a one-valued function of the n letters.

There is an exception in the case $k=2$, whatever be the value of n: viz. the product of the squares of the differences is a one-valued function, a perfect square; but its square root, or the product of the simple differences, is a two-valued function. It is in virtue of this exception that a quadric equation is solvible by radicals; we have the one-valued function $(x_1-x_2)^2$, the square of a two-valued function x_1-x_2, and thence the two roots are each expressible in the form

$$\tfrac{1}{2}\left\{x_1+x_2+\sqrt{(x_1-x_2)^2}\right\}.$$

II. A two-valued function of n letters is a perfect kth power, only when the kth root is a two-valued function of the n letters.

There is an exception in the case $k=3$, when $n=3$ or 4: viz. for $n=3$ we have $(x_1+\omega x_2+\omega^2 x_3)^3$ (ω an imaginary cube root of unity) a two-valued function, and a perfect cube; whereas its cube root is the six-valued function $x_1+\omega x_2+\omega^2 x_3$. And similarly for $n=4$ we have, for instance,

$$\left\{x_1x_2+x_3x_4+\omega(x_1x_3+x_2x_4)+\omega^2(x_1x_4+x_2x_3)\right\}^3$$

a two-valued function, and a perfect cube, whereas its cube root

* Communicated by the Author.