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*MODERN GEOGONIES EXAMINED IN THEIR BEARINGS ON THE ANTIQUITY OF MAN.* By the Rev. Professor BIRKS, M.A.

I.—INTRODUCTION.

**T**HE charge of error, freely brought in these days against the statements of the Bible concerning Creation and the Origin of Man, has been based on alleged scientific proofs of the high antiquity of the human race. When full allowance has been made for the various readings of the Hebrew and the Septuagint, it is perfectly clear that the Bible date for Adam's creation cannot be placed further back than seven or eight thousand years ago. These are no separable accidents, but main and integral parts of the grand message, that Adam was the first father of all men, that in him all die, through a common fall from innocence and uprightness, and that all are brought within the range of one great redemption, wrought by Jesus Christ, the Second Adam, the Lord from heaven.

Clergymen, as well as laymen, are now found who set aside these statements, as if they were only superstitious errors, which growing light and knowledge have disproved. A special sanction and currency has lately been given to this view, which many Christians must regard as a blow aimed directly, however unwittingly, against the historical foundation of the whole message to sinful man in the Word of God. The importance of the question thus raised is extreme. I propose in this paper to carry further the course of thought in two former papers read before this Society, and to analyze the data upon which some have reared a conjectural pre-Adamite human history of two hundred thousand years.

2. The modern doctrine of Man's high Antiquity rests mainly on two premises, though these are supplemented by other

presumptions of a secondary kind. First, certain flints from Brixham Cave, the valley of the Somme, and caverns in Belgium, are affirmed to have been plainly fashioned into tools, spears, or hatchets, by the hands of savage men. And next, the beds of gravel or stalagmite, where they were found, are said to have been deposited many myriads of years ago. Human deposits are thought to occur in quaternary strata or drift, directly after the close of a great ice period. This period, again, has three different estimates of its remoteness by different geological speculators. One of them assigns two glacial periods to the dates 13,000 and 44,000 years before Christ. Another offers the dates 210,000 and 850,000 years B.C. for a Post-Pliocene and a Miocene glacial period, while others have suggested a date still more remote for man's first appearance on the earth.

3. Mr. Whitley, in two able papers read before this Society, denies even the first premise. The so-called flint implements were formed, he thinks, by the natural change of flint nodules broken under strong pressure. He offers many reasons—from their position, their great number, their relation to the neighbouring beds, and the effects of artificial fracture, to support this view. Mr. Pattison agrees with Mr. Whitley as to a large proportion of the alleged implements, but admits that some are apparently of human origin. He maintains, however, on a full review of all the features both of Brixham Cave and the valley of the Somme, that six or seven thousand years are time enough to account for all the later changes. Mr. Callard, in his short and able essay on the Geological Evidences of Man's Antiquity, argues forcibly for the same view. Whether or not Mr. Whitley is right in his denial of an artificial origin to each and all the so-called implements of the Drift, I think that Mr. Pattison and Mr. Callard are fully justified in their dissent from the other main premise of the theory. It may be shown that there is no scientific proof of these immense ages since the close of a real or imaginary glacial epoch, but only a series of mere conjectures, based on wholly inadequate data; and a more probable theory than any of those hitherto offered would reduce the distance of man's first appearance within a limit in complete harmony with the Scripture statement. Man has, doubtless, been contemporary with many animals now extinct; but this can never prove his entrance on our planet to have been 200,000 or even 20,000 years ago.

The theories I shall examine in succession are these:—First, Sir C. Lyell's doctrine of uniformity; secondly, the thermodynamic theory of Sir W. Thomson; thirdly, the excentric-pre-

cession theory of Lieut.-Col. Drayson, who refers it to a great increase in the obliquity; and, fourthly, the view advocated with great labour and ability by Mr. Croll, in his work "Climate and Time." He there employs more than 500 pages in attempting to prove that a series of glacial periods have been due to successive maxima of excentricity of the earth's orbit during a space of three millions of past years.

## II.—THE DOCTRINE OF UNIFORMITY.

4. The title of Sir C. Lyell's work is "Principles of Geology; or an Attempt to Explain the Former Changes of the Earth's Surface by Reference to Causes now in Operation." And he recommends an "earnest and patient endeavour to reconcile the former indications of change with these existing causes." And in Mr. Page's Advanced Text-book we are told, "When such hypotheses as nebular condensation, igneous fluidity, change of axis, secular contraction of the earth's mass, highly carbonated atmosphere, passage of the system through colder and warmer regions of space, are advanced to account for geological phenomena, the student must receive them as mere hypotheses, not as the true and sufficient causes of inductive philosophy. The legitimate progress of science lies over a pathway of observation, fact, and deduction, and is little aided by conjecture, however plausible. Let us strive first to exhaust the range of normal causation in existing nature, and even then continue to work and watch, rather than fall back on the idle and unphilosophical resort of abnormal conditions in primeval nature." And, again, p. 374, "There are two great schools of geology, the one ascribing every result to the ordinary operations of nature, combined with the element of unlimited time; the other, appealing to agents that operated during the earlier epochs of the world with greater intensity, and over wider areas. The former belief is certainly more in accordance with the spirit of right philosophy, though it must be confessed that many problems in geology seem to find their solution only through the admission of the latter hypothesis." And Sir C. Lyell, in his "Treatise on the Antiquity of Man," though his statements are indefinite, says, that the historical period seems "quite insignificant in duration, when compared with the antiquity of the human race" (p. 289), "and that natural barriers would ensure the isolation, for tens of thousands of centuries, of tribes in a primitive state of barbarism" (p. 386). This implies a con-

viction of man's past existence on earth for several millions of years.

5. Here, in the fundamental maxim assumed, there is a serious ambiguity. What is meant by "causes now in operation"? Does it mean simply the central forces, the attractions and repulsions, varying by certain laws of distance, of all the bodies or their component atoms that now exist? If so, the doctrine becomes only a sort of truism. The sudden bursting of a reservoir, the explosion of a magazine, the firing of a broadside, or a volcanic eruption, are as much from causes now in operation, as the quiet state, with no sudden or sensible change, which may have gone before, and lasted months or years. But if we mean by causes now in operation, all acting forces, with merely the same conditions as now exist, which vary with every hour, day, and year of their own action, the maxim is unphilosophical and untrue. We should explain the changes of the earth by causes acting under the conditions of the time when they occurred, and not under new conditions which may have come into being, through the action of those very causes, after many thousands or myriads of years.

6. Averages give a fair approximation, or are wholly fallacious, according to the nature of the facts to which they are applied. They are safe, chiefly when they are taken between two observed limits, since a small part of any curve does not vary widely from the line which joins its extreme points. In many cases the error may not be great for parts which lie beyond this limit, on one side at least. But let a chord of a hyperbola, near the vertex, be prolonged towards the vertex a hundred times beyond its own length, the distance from the answering point of the curve will be very great, and the two will be tending in wholly opposite directions.

Now most of the cases to which the law of averages has been applied by uniformitarian geologists are of this very kind. Each step of past change tends to lessen the motive power on which the future changes depend. Thus every river transports a certain amount of soil in suspension from the high ground near its sources or from the bed through which it travels to the sea. But every year the high ground is wasted, the mouth is silted up, and the soil probably hardens and becomes less easy to remove. The quantity annually carried down will thus diminish for three different reasons. It will also come to be spread over a wider area. Hence the present depth of the annual deposit is no proper test by which to give the average for many thousand years.

7. Let us take one case often referred to,—the Delta of the

Mississippi. Sir C. Lyell, from the present amount of solid matter conveyed by it, and the area and depth of the accumulation near its mouth, inferred that 67,000 years would be needed for the Delta proper, and 33,000 more for the plain above to be transported to its present site. Hence he speaks of the whole period as "perhaps far exceeding 100,000 years." But in 1869 he says that "the data had considerably altered since first he wrote. Recent calculations had doubled the volume of water flowing into the sea, and thus the same effect might be produced in half the time previously calculated." Thus 50,000 years were struck off by the first correction.

But now let us assume, instead of a fixed annual amount of detritus, that there has been a steady decrease of only one four hundredth part of the present quantity. The 50,000 years would then reduce themselves to 5,937, which would bring the commencement of the process within the limits of the known or biblical age of mankind.

8. Again, Mr. Croll makes a calculation, that the same river at its present rate would carry down the whole area drained by it to the sea-level in  $4\frac{1}{2}$  million years. But, adopting a similar law, or supposing the decrease each year to be only one part in a thousand of the present amount, how long would have been needed to waste away a double quantity of land or rock to its present amount? Rather less than 94,000 years.

The same principle applies to the mud of the Nile, and a vast number of cases of a similar kind. The doctrine of averages, when so applied, rests on a mere assumption, not only unproved, but highly improbable, and almost certainly untrue. In a single year of high flood a river may transport an amount and kind of material, which could not have been removed by a hundred years in which no flooding has occurred.

9. The case is the same as to upheaval and volcanic eruptions. It is plain that whenever the crust is broken through, and a stream of lava, before pent in, comes from below, the motive force must tend to exhaust itself by the effort. The heat, generated by internal pressure, will partly escape through the opening, while the pressure also is lessened by the rupture of the crust. The approach must be constantly towards a limit, when the upward and expansive force has spent itself, and though the renewal may have gone on through long ages, the first intensity or amount of action can never return. The process of condensation, with the generation of internal heat, and its conflict with the cooling ocean at the surface, or the intense cold of the interstellar

spaces, has a natural limit, beyond which it cannot go, and to which it must approach more and more slowly as the change proceeds.

### III.—THE THERMO-DYNAMIC THEORY.

10. The doctrine of uniformity, in its extreme form, as held by Sir C. Lyell and many others, has found of late some strong opponents among our foremost analysts. Sir W. Thomson and Professor Tait would replace it by what may be called a Thermo-dynamic theory. They maintain that the solar energy is in process of constant dissipation, and that hypotheses assuming an average constancy of sun and storms for a million years "cannot possibly be true." It is quite certain, Sir William thinks, that the solar system cannot have gone on as at present for a few million years, without the irrevocable loss, by dissipation, of a very considerable portion of the entire initial energy. He calculates, from Fourier's theory of the rate of conduction, and the specific heat of rocks at Edinburgh and Greenock, that the consolidation of the earth's crust cannot have taken place less than 20 nor more than 400 millions of years ago; also that the general climate cannot have been sensibly affected by conducted heat from the centre, except within the first 10,000 years after the solidification, and that in 96 millions of years the thickness of the crust, through which a given amount of cooling would be experienced, would have increased fivefold. He admits that a wholly different view is maintainable, that internal heat is due to chemical combination, going on slowly everywhere at great unknown depths, and creeping onward gradually as the chemical affinities of each layer are saturated. But he thinks also that "the less hypothetical view, that the earth is merely a warm, chemically inert body, cooling, is clearly to be preferred in the present state of science."

11. The objection may be urged, that the earth cannot well be supposed ever to have been a solid, uniformly heated, and  $7,000^{\circ}$  warmer than the present heat of the surface, which is the hypothesis assumed. But Sir William replies that the solution may be easily modified, to meet the case of a liquid gradually becoming solid, *at least when three fresh data have been supplied*. And he argues further that the earth, "although once all melted, did in all probability become a solid at its melting temperature all through, or all through the outer layer which had been melted; and that not until it was thus completely solidified, or nearly so, did the crust begin to cool."

12. It is clear, from this very statement, how much remains merely hypothetical in this solution, on which the calculation of the age of the earth's crust is to depend. Professor Tait has since replaced the estimate of the limits of 20 and 400 millions of years by a suggested period of 10 millions only. In the statement quoted it is owned that three further data must be supplied, before the solution can be altered so as to suit the real conditions. The view, which Sir William rejects as more hypothetical, that the heat is generated by chemical change, seems to me less hypothetical and more natural than his own; and needs only to be carried a step further and applied to the formation of the chemical elements themselves, by pressure, to supply a far more complete solution of the great problem.

The rejection of uniformity of action through many millions of years is justified, I conceive, on many grounds. But instead of grounding it on the certain steady decrease of solar heat by exhaustion and dissipation, I think it may be based more reasonably on the opposite ground of its increase. For if the present amount has ensued after solar condensation, and the sun was once a diffused mass of low temperature, variation by increase for long ages must be one constituent element of the theory; but a reversal of the process, and a greater loss than gain of heat for many millions of years must be wholly improbable in the absence of any direct experimental evidence.

13. Those theorems of Fourier, on which the reckoning is based, all rest on the hypothesis that the heat transferred from a hot to a cool body is strictly as the difference of their temperatures, and that the temperature is the quotient of the amount of heat in any body, divided by the mass. This implies the hypothesis that heat is a specific fluid. For it reasons as if the total heat of the system, between the parts of which conduction takes place, were a fixed quantity, not capable of increase or diminution, by forces generating motion, or motion being extinguished by expansion. But the opposite view, the doctrine of Bacon and Rumford, that heat is simply atomic motion, is now fully established, and Sir William is one of those who have had no mean share in its confirmation. Hence the conditions of the problem of conduction, for long periods, must be wholly altered. There is no longer a fixed amount of heat, of which a small part is transferred by a definite law from a hot to a cooler body. It may be generated in the one by condensation, and conversely by expansion be destroyed in the other to an unknown extent. Potential may be turned into kinetic energy on one side, and on the other kinetic into

potential. There may thus be both an indefinite demand, and an equally unlimited supply. The real problem will depend mainly on these two elements, which are entirely absent in the solution Sir William has proposed. The calculation is really a partial survival from that fluid-caloric theory which is now universally abandoned.

14. The doctrine of uniformity, as held by Sir C. Lyell, rests on a confusion of two things wholly distinct,—the constancy of natural laws, such as gravitation and cohesive affinity, and the sameness of the conditions under which they operate at widely separated periods of time. But these conditions are changing hourly through the action of the laws themselves, and the difference in the course of ages becomes so great as wholly to falsify any conclusions which are based on the assumption of their near approach to identity. I fully agree, then, with Sir W. Thomson, in his protest against that theory; but I cannot accept, as reasonable or true, the special ground on which he bases his opposition. Mr. Croll sets the two doctrines in contrast in the following passage, which shows the immense scale of time adopted by uniformitarian theorists.

“It was the modern doctrine that the great changes undergone by the earth’s crust were produced not by convulsions of nature, but by the slow and almost imperceptible action of sun, rivers, snow, frost, ice, which impressed so strongly on the minds of geologists the vast duration of geological periods. When it was considered that the rocky face of our globe had been carved into hill and dale, and worn down to the sea-level by these apparently trifling agents, not once or twice but many times, in past ages, it was not surprising that the views entertained by geologists on the immense antiquity of our globe should not have harmonized with the deductions of physical science. It had been shown by Sir W. Thomson and others, from physical considerations of the sun’s heat and the secular cooling of our globe, that the history of the earth’s crust must be limited to a period of something like a hundred millions of years. But these speculations had little weight when pitted against the stern and undeniable fact of subaërial denudation. How were the two to be reconciled? Was it the physicist who had under-estimated geological time, or the geologist who had over-estimated it? Few familiar with modern physics, who have given attention to the subject, would admit that the sun could have been dissipating his heat at the present enormous rate for a period much beyond a hundred millions of years.”

15. In this conflict of the two theories, I believe that there

is an almost equal error on each side. Each theory is based on data wholly insufficient to establish its truth. The doctrine of uniformity, I believe, is untrue for many reasons, but not for the reason which Mr. Croll, following Sir W. Thomson, has assigned. There is no proof that the sun was much hotter a hundred or fifty millions of years ago than at present. If there be a difference, which is probable, I think it much more likely that it would be of an opposite kind, and that its heat has increased by condensation, more than it has lost by dissipation. In the "Theory of Helmholtz," which Sir William has latterly espoused, having abandoned Meyer's meteoric hypothesis, the heat of the sun is now thought to be supplied by condensation, which replaces the ceaseless waste from dissipation or radiation into space. Now if the sun has reached its present high state of heat and light from an earlier stage, when it was neither hot nor luminous, what proof can there be that the process has been reversed for the last million of years, and the waste exceeded the supply for so long? But this very idea, that all the heat radiated into space is dissipated and lost, is an assumption without solid reason. If it arose at first from a transformation of potential into kinetic energy, or attractive force into motion, by the condensation of the solar mass, it can only cease or be lost by a reconversion of this kinetic energy into potential energy of another kind; namely, the condensation of repulsive ether. Thus the energy which flows out from the sun as sensible heat and light, in the sector of space bordering on the sun's equator, will return to it invisibly and insensibly, in the neighbourhood of the poles, and the sun would thus be an immense magnet by virtue of its revolution.

16. The general climate of the earth, Sir W. Thomson further remarks, "cannot have been sensibly affected by conducted heat, at any time more than ten thousand years after the solidification of the surface." This may be true, if we take the phrase "conducted heat" in a rigorous sense, and exclude all liquefaction, convection, regelation, or fresh generation of heat by condensation from pressure or chemical change. But these omitted or excluded elements are those of chief importance in the actual problem. A solution which omits them may be true as an abstract dynamical theorem, but can have little bearing on the actual course of geological change.

17. The first volume of Sir W. Thomson's and Professor Tait's comprehensive "Treatise on Natural Philosophy" closes with these remarks on the once current hypothesis of the earth's fluidity below a thin superficial crust.

“These conclusions, drawn from a consideration of the necessary order of cooling and consolidation, according to Bischoff’s results on the relative specific gravity of solid and melted rocks, are in perfect accordance with §§ 832—849, on the present condition of the earth’s interior; that it is not, as commonly supposed, all liquid within a solid crust from thirty to one hundred miles thick, but is, on the whole, more rigid than a solid globe of glass of the same diameter, and probably than one of steel.”

The investigation here alluded to seems to me decisive against the doctrine of the earth’s central fluidity, and carries to a further point the conclusion of Mr. Hopkins, thirty years ago, from the phenomena of nutation and precession. It accords with my own inference from an hypothesis wholly distinct. But while I think that Sir William has disproved the notion of the central fluidity of the earth, and justly rejects the notion of geological uniformity for many hundred millions of years, I wholly dispute the soundness of his doctrine, that the date of the formation of the crust can be defined by “Fourier’s Theorems” on conducted heat, or that the waste of solar heat is in constant excess over the fresh supply. In fact, the doctrine of uniformity would be equally untrue, whether the light and heat of the sun have increased or diminished sensibly in the course of a million years.

#### IV.—THE TRANSLATION THEORY.

18. Another view has been suggested by Poisson, to account for past changes in the earth’s climate, and warm and glacial periods,—the earth’s translation through hotter and colder regions of space. This does not need to detain us long, as there seem to be very simple and decisive reasons against it. Mr. Croll has thus given them briefly and clearly in a few words.

“This is not a very satisfactory hypothesis. . . . Space is not a substance which can possibly be either hot or cold. If we adopt this hypothesis, we must assume that the earth, during hot periods, was in the vicinity of some other great source of heat and light beside the sun. But the proximity of a mass of such magnitude as would be able to affect to any great extent the earth’s climate, would, by its gravity, seriously disarrange the mechanism of the solar system. If it had ever, in a former period, come into the vicinity of such a mass, the orbits of the planets ought to afford evidence of it. But again, to account for a cold period, like the glacial epochs, we must assume the earth to have come near a cold body. And recent discoveries with regard to interglacial periods are wholly irreconcilable with this theory.”

19. But while this translation theory of Poisson is both vague and inadequate, and wanting in direct evidence, the fact of the movement of our system in space is a strong reason against the uniformity assumed by many geologists to have lasted through many millions of years. The rate of the sun's motion in space is held to be 150 millions of miles a year. This would carry it as far as *α Centauri*, the nearest star whose parallax is determined, in 140,000 years. The direction prolonged backward has its apex only  $25^\circ$  from *Sirius*, the brightest of all the stars, and of which the light has been reckoned to be 60 times greater than that of the sun. Its parallax is  $\frac{2.3}{100}$  of a second. It has been lately inferred from the spectroscope that we are receding from *Sirius* at the rate of 25 miles a second, or 800 millions in a year, so as to traverse the whole distance in 100,000 years. And since we cannot tell whether the earlier motion may not have varied so far in its direction, we can have no assurance that all the elements of our system may not have been altered by the proximity of *Sirius* only one hundred thousand years ago. All estimates of solar force and the earth's inclination and eccentricity which go back beyond this limit must remain highly uncertain on this ground alone, and are beyond the range of assured and certain science.

20. Two other theories may be also dismissed in few words. First, that of an altered axis of rotation, so that the north and south poles of the diurnal rotation were at places considerably remote from those which they now occupy. But this is rendered all but impossible by the spheroidal shape of the earth. At any time, after the crust had once hardened and taken a spheroidal form, revolution on any axis, not adjacent to the present one, must have been mechanically impossible. Any secondary change of surface by the uprising of a mountain-chain might produce an increased nutation and a kind of waddling motion around the true axis, but it could not alter the place of that axis, or produce any sensible effect on the climate of any main parts of the surface.

21. Another theory of the same kind is Sir C. Lyell's transposition theory. He supposes that the mean temperature would be raised if the land were mainly in the torrid zone, and be lowered if it were grouped around the poles. Mr. Croll argues that the effect would be diametrically opposite, and that the contour of the surface most favourable to the warmth of the earth is when the water is in all the middle part, and the land only at the poles. Now it is difficult to reason out all the consequences as to the mean temperature of the whole surface. The mere fact that two such opposite

views have been held suggests a doubt whether either can rest on sure scientific grounds. The one thing which seems clear and certain is, that a structure like that of our globe with two main oceans extending almost unbroken from the south to the north pole, over three-fourths of the whole surface, is the arrangement most favourable to a mitigation of fierce extremes, and to fit our world for human habitation. At the same time, since the glacial epoch belongs to a stage of geology when the outlines of land and water were nearly the same as now, it is perfectly clear that no difference in their relative arrangement can serve to account for a much lower or a much warmer temperature than has obtained in the known historical period of the world.

#### V.—VARIED INCLINATION THEORY.

22. Another theory of a more definite kind is advocated by Lieut.-Colonel Drayson, in his work entitled "The Cause, Date and Duration of the Last Glacial Epoch of Geology." He places the period of maximum glaciation 13,700 years before Christ, or 15,500 years ago. Such a view, if it were established, would plainly be much more reconcilable with the Bible chronology for the date of man's appearance on the earth than the opinions just examined. But I believe that it rests on a fundamental mistake which it is not difficult to place in a clear light. Mr. Croll remarks on it as follows:—

"The theory is beset by a twofold objection. First, it can be shown from celestial mechanics that the variations in the obliquity must always have been so small that they could not affect the climatic condition of the globe. Secondly, even admitting that the obliquity could change to an indefinite extent, it can be shown that no increase or decrease, however great, could possibly account for the glacial epoch, or a warm temperate condition in the polar regions."

23. This second objection, whether true or false, seems to me diametrically opposed to the reasoning of Mr. Croll in favour of his own hypothesis, when he would account for a glacial season by an increased excentricity, concurring with a northern winter solstice in aphelion. With regard to the total heat there is this slight difference, that a change of inclination leaves it quite unaltered, but an increased excentricity causes a small increase. So far the second is less suited than the first to account for a glacial period. But with regard to total winter temperature, the operation of the two causes is precisely of the same kind, and the relative effect in the ratio

of twice the excentricity to the sine of the inclination. Hence an increase of the inclination from  $23^{\circ} 28'$  to  $35^{\circ} 56'$  with the present excentricity would cause the same degree of inequality as an increase of the excentricity to  $\cdot 0747$ , its supposed amount 850,000 years ago. If glaciation would result, as Mr. Croll contends, from the latter combination, it must have done so from the other, and for the same reason. On the other hand, if a hotter summer undoes and reverses the effect of a colder winter with an increased obliquity, it must equally do so with an increased excentricity.

24. The real error of Lieut.-Col. Drayson's theory is its contradiction to the laws of physical astronomy. The pole of the equator, by precession, is receding  $50''$  in longitude annually at a right angle to the pole of the ecliptic. But the obliquity is also slowly lessening, and the poles are coming nearer together. Lieut.-Col. Drayson finds that the two phenonema will be reconciled, and the observations of precession and polar distance satisfied from Tycho down to the present day, if we assume the pole of the equator to revolve round a point at  $6^{\circ}$  distance from the pole of the ecliptic. In this case, the nearest approach would be about five centuries hence, the period of revolution 31,840 years, and B.C. 13,600 the obliquity would have its maximum value, or  $35^{\circ} 26'$ . The excentricity, by Mr. Croll's table, would then be  $\cdot 01875$ , and the effect to produce inequality of heat at midwinter and midsummer, the same as with the present obliquity and an excentricity of  $\cdot 1095$ , or half as great again as the maximum in Mr. Croll's table.

25. But the mistake is here. The precession or backward motion of the pole of the equator, and the diminished obliquity or the motion of the pole of the ecliptic nearer to that of the equator depend on two wholly distinct causes. One is due to the action of the sun on the equatorial protuberance, and must be at right angles to the line which joins the two poles at the moment and in no other direction. The other is due to the disturbing action of the other planets on the earth's annual orbit. It does not make the pole of the equator move with reference to that of the ecliptic, but the reverse, that is, the pole of the ecliptic approaches to or recedes from that of the equator. Thus the earth's pole does not revolve round a fixed centre  $6^{\circ}$  away from the pole of the ecliptic, but round a pole itself moving in a small self-returning curve of definite limits. It moves in fact in a sort of cycloid of a rather complex kind, and not in a circle. No doubt a circle may be found, as Lieut.-Col. Drayson has proved, to satisfy the observations, which range over only four centuries. But this is a striking example of the danger of trusting to a purely

empirical law beyond the limits of the observations from which it is deduced, even when it is much more scientific than a bare average. Lieut.-Col. Drayson's circle is an approximation of the second order, and will satisfy the observations of four centuries much better than a simple average, which is of the first order only. But it will wholly mislead when carried beyond those limits; for the true curve of the earth's pole projected on the celestial sphere is not an excentric circle, but a kind of cycloid, or a circle of which the centre is ever moving, though within narrow limits. The pole of the equator does not move towards that of the ecliptic, but at right angles to the joining line, while the latter does approach to and recede from the pole of the equator. If the hypothesis were true, there is no reasonable doubt that it would involve the consequence of fierce extremes of summer heat and winter cold, over a large part of each hemisphere of the earth.

#### VI.—THE THEORY OF INCREASED EXCENTRICITY.

26. The most popular theory, at present, which offers a kind of geological chronology, is that of Mr. Croll, in his work entitled, "Climate and Time in their Geological Relations." It has been adopted by Mr. Geikie in his "Great Ice Age," by Sir C. Lyell, and apparently by many others, and has been developed, in a volume of five hundred pages, with great labour, research, and ingenuity. It professes to account for a recurrence of extremely cold or glacial periods by the coincidence of two astronomical elements,—an increased excentricity of the earth's orbit at certain past dates, and the position of the northern winter solstice near the aphelion. It is held, further, that when the southern winter solstice was in the aphelion, there would be a similar period of glaciation of the southern hemisphere. Mr. Croll has calculated the excentricity, by Leverrier's formulæ, at intervals of 50,000 years, for three millions of years of past, and one million of future time, and every 10,000 years for the last million only. He discovers two maxima, 850 and 210 thousand years ago, and identifies them with a Miocene and a Post-Pliocene Ice Period, assumed to be proved by modern geology. The first signs of man's presence on the earth are usually held to be either soon after, or else just before, the Boulder Drift, the second of these periods. The effect, then, of Mr. Croll's theory would be to place the entrance of man on our planet above two hundred thousand years ago. During this vast interval, thirty times greater

than all the known period of human history, aboriginal men, who possessed no arts, and left no monuments, and lived in the dark with no message of light from heaven, must have continued to wander, homeless and hopeless, in deserts and mountains, and in dens and caves of the earth.

The moral and religious difficulties of such a creed are plainly immense. I wish now to examine it simply on the side of physical science. Mr. Croll's theory is certainly elaborated with great pains and care, and includes a wide collection of materials, and a large amount of patient thought and ingenuity. It has received the highest praise from a writer in the *Quarterly Review*, as beautiful, simple, and complete. I need, therefore, to offer strong reasons for my own conviction, expressed before in the Annual Address, that it is based on a complete fallacy, and is wholly wanting in solidity and truth.

27. A first objection, made by Professor C. Martens, and more recently by Mr. Callard, is of a very simple and striking kind. The planet Mars is forty millions of miles further from the sun than our earth. Its excentricity is  $\cdot 0933$  instead of  $\cdot 01678$ , or  $5\frac{1}{2}$  times greater, and its absolute amount 26 millions of miles, or nine times greater than the present excentricity of the earth; three times greater than that at Mr. Croll's second, and twice as great as at his first, supposed glacial period. Yet the snows of this planet, while they increase in winter, and decrease in summer, are never seen to extend more than six or seven degrees from either pole. The spectroscope and telescope conspire to prove that Mars is not now suffering under an ice age. How, then, could the increase of the earth's excentricity from 3 to  $10\frac{1}{2}$  millions of miles produce the glaciation of more than half the hemisphere, when one of 26 millions has no such effect in a planet half as far again from the sun?

Mr. Croll observes that little is known of the climatic condition of Mars, and that its atmosphere may perhaps be wholly different from our own, and that other physical conditions, besides greater excentricity, may be needed to secure a glacial epoch. This may doubtless be true; but since we have only to guess at such causes of difference, the negative evidence, though not decisive, is strongly adverse to the notion that glaciation, in the case of our earth, is due mainly to a greater excentricity than now exists. For in Mars the aphelion distance is about 148 millions, while in Mr. Croll's ice era, our own would be 97 millions, and still the imaginary result from increased excentricity does not seem to follow.

28. A second objection has some weight. The total heat received by the earth in a year from the sun is inversely as

the minor axis, when the periodic time and the major axis are constant and do not change. This was stated by Sir J. Herschel in a paper read to the Geological Society in 1830. It admits of easy demonstration, and Mr. Croll quotes the paper in his Appendix, and admits the scientific truth. He thinks, however, the difference is so slight that it may be safely neglected, and treated as of no account. But this is not so plain. It would be very strange, if a period in which the earth receives the most heat from the sun were that in which, on the whole, it suffers the most from extreme cold. With an excentricity of  $\cdot 0575$ , or  $10\frac{1}{2}$  millions excess of aphelion over perihelion distance, the excess above the present would be three-twentieths per cent., or  $1\frac{1}{2}$  part in a thousand. Let us take 5,000 years on each side of Mr. Croll's date, or the interval from 205 to 215 thousand years ago. If a northern winter aphelion lay midway between, this would include half one whole circuit, in which the aphelion lies within the northern winter season. The excess of heat received from the sun in those 10,000 years above its mean amount will be, in Mr. Croll's mode of reckoning, about 27 billions of billions of foot pounds. This agrees ill with the hypothesis that the period is one marked by extreme and excessive cold.

29. A third and more decisive objection follows. The season which the theory singles out to account for extreme glaciation, is that in which the northern hemisphere receives the greatest excess of solar heat above the mean value.

The proof is simple. The total heat received by the earth from the sun in its annual orbit is equal for equal angles. The swiftness and the nearness, the remoteness and the length of time, compensate each other, varying by the same law of the inverse square of the distance. But this is not true for the separate hemispheres. If the orbit were circular, each would receive more in the summer, and less in the winter half of the year. But from the excentricity, when the perihelion and aphelion are at the two solstices, the summer heat is increased and the winter heat diminished, or conversely, in the same ratio. But since the summer heat is greater than that of the winter, the total for the hemisphere whose summer is in the perihelion must exceed the other.

30. To make this plainer, let us take approximate values. Let the earth's distance from the sun be 90 millions, the excentricity, as in the supposed glacial epoch, one-ninth, or the greatest and least distances, 95 and 85 millions. The quantity of heat at perihelion and aphelion will vary in a duplicate ratio; or if 9 be taken for the mean quantity, 8 and 10. The ratios at midsummer and midwinter are as  $1 + \sin. \iota$  to  $1 - \sin. \iota$ , nearly

as 7 to 3, and for the whole half-year as 5 to 3. There is an excess or defect of about one-fourth of the mean value. Hence  $\frac{5}{4}$  of 10 +  $\frac{3}{4}$  of 8 = 18·5, will be the total heat for the northern, and  $\frac{3}{4}$  of 10 +  $\frac{5}{4}$  of 8 = 17·5, for the southern hemisphere, when the northern summer solstice is in the perihelion, and the northern winter solstice in the aphelion. Thus the northern half of our globe will receive from the sun one thirty-sixth, or nearly 3 per cent. of heat in excess of the mean value. Thus the period selected as the Ice Age is one in which the northern hemisphere receives from the sun an amount of heat exceeding by almost 3 per cent. its mean value, and greater than at any other period in the long course of 700,000 years.

31. Thus the result cannot depend on a lessened total amount of solar heat incident on the earth at the eras in question, for the total is increased. Sir J. Herschel, Arago, and other leading men of science, have failed to see that increase of excentricity within the actual limits could produce an ice age in either hemisphere. Mr. Croll admits that it could not, directly, be the cause of such a change; but he argues that, indirectly, it may be the cause, by bringing other causes into operation.

His reasoning is as follows. From the values of the excentricity at past periods he deduces the ratio of the direct solar heat at midwinter to its present amount. One column of his table gives the excentricity, from Leverrier's formulæ, at intervals of 50,000 years for three millions of years backwards, and one forward, and of 10,000 years for one million backward. Another column gives the ratio of the midwinter solar heat at each period to what it is now. The temperature of space is assumed to be  $-239^{\circ}$  F. The excess above this limit is assumed to depend on the midwinter solar radiation, and to be strictly proportional to it. The midwinter heat of our country is taken at  $39^{\circ}$  F., or the excess as  $278^{\circ}$ . The ratios for the two selected eras, 850,000 and 210,000 years ago, are ·837 and ·864; hence the deficit at the two eras would be  $45^{\circ}\cdot3$ , and  $37^{\circ}\cdot7$ , and the results  $-6^{\circ}\cdot3$  and  $+1^{\circ}\cdot3$  F. for the midwinter heat of our country at those two eras. With such a degree of cold, ice and snow would rapidly form. The heat of the summer, Mr. Croll argues, would be unable to melt the winter ice, and it would go on accumulating through many successive years, till the orbit and aphelion place were changed, and the main condition was thus reversed, after 10,000 years.

Here Mr. Croll reverses his argument against Poisson's theory, that space is not a body, and can have no temperature,

No. 18, 1. 9. A temperature of space about two hundred and forty degrees below the zero of Fahrenheit is the basis of all his calculations.

In these calculations there are several serious defects, which disprove the conclusion, and require us to look further for an adequate explanation of the general prevalence of cold in the northern hemisphere during the Drift or Glacial period. The amount of the excentricity, the law of radiation, the proper point of the orbit for estimating the balance of solar heat, and loss by radiation, the law of midsummer heat, and the effect of aërial and oceanic currents, are all of them elements which seem to me to have been incorrectly assumed or left out of view. The combined result of the corrections thus required will be practically to set aside the whole theory.

32. First, the excentricity is calculated by M. Leverrier's formulæ. It might seem beforehand very doubtful whether these can be relied on for a date three millions of years ago, or even for 850,000, or 210,000 years. But there is here a special reason for distrust. The present excentricity is  $\cdot 0167836$  (Hersch. Ast.), and those at the two eras in debate,  $\cdot 0747$  and  $\cdot 0575$ . Now the maximum for the earth, according to Lagrange, is  $\cdot 07641$ , and according to Leverrier  $\cdot 077747$ , and the value at 850,000 years ago is thus very near the limit. But these calculations were made before the discovery of Neptune. Fresh calculations have been made by Mr. Stockwell, since that discovery, and the corrected maxima for the planets from Venus to Saturn are all diminished. Those of Leverrier are M.  $\cdot 225646$ , V.  $\cdot 086716$ , E.  $\cdot 077747$ , M.  $\cdot 142243$ , J.  $\cdot 061548$ , S.  $\cdot 084919$ , U.  $\cdot 064666$ . But the later values are M.  $\cdot 2317185$ , V.  $\cdot 0706329$ , E.  $\cdot 0693888$ , M.  $\cdot 139655$ , J.  $\cdot 0608274$ , S.  $\cdot 0843289$ , U.  $\cdot 0779652$ , N.  $\cdot 0145066$ . Thus the value accepted by Mr. Croll for his earlier date is one which exceeds the corrected maximum by  $\cdot 0053$ , or nearly a million miles. If Mr. Stockwell's calculation is correct, it is an impossible value.

An exact correction would, of course, involve a prodigious amount of fresh labour; but a reasonable approach to it may be gained by diminishing the excess over the present excentricity in the ratio of the excesses of the two maxima. These are  $\cdot 0609634$  and  $\cdot 0526052$ . The values  $\cdot 0747$  and  $\cdot 0575$  will thus become  $\cdot 06676$  and  $\cdot 05192$ , or about nine-tenths of those on which the actual calculation has been based. This first correction will lessen the decrease of mid-winter temperature three or four degrees.

33. But the method of deducing the midwinter heat from

the ratio of the heat received from the sun at the winter solstice is also defective. The excess of that winter temperature over the temperature of space is held to be strictly proportional to the amount of solstitial heat received. But this combines a mere hypothesis with a defective law of dispersion or loss by radiation. A simpler rule may be deduced, in a less hypothetical way, from the experiments of MM. Dulong and Petit. According to these, when heat radiates from a hotter to a cooler body, and the difference of their temperatures is constant, the radiation increases or diminishes in the ratio of 1.165 to 1 for a rise or fall of 20° C. or 36° F. in their two temperatures. Of course, if the lower body has a fixed temperature, and the hotter alone varies, the ratio should be slightly greater. To establish an equilibrium between the heat received from the sun and that radiated into space, the midwinter heat must thus be lowered till the radiation is lessened in the same proportion as the solar heat received.

Adopting this rule, and retaining Mr. Croll's values for the excentricity .0747 and .0575, and the answering ratios of midwinter heat, the lowering of temperature will not be 45°·3 and 37°·7 F., but 41°·94 and 34°·34 only, a difference of more than three degrees. But with the corrected values .06676 and .05192 they will be 38°·45 and 31°·84 only; or the winter heat at the later period, Mr. Croll's proper ice age, will be 7°·2 F. instead of 1°·3, a difference of six degrees.

34. But a further correction is plainly required. The equilibrium between the heat received and lost is clearly not at the solstice itself. The greatest heat in summer and cold in winter is well known to be about a month later, that is, at a distance of about 30° from the solstice. Thus the distances, on which the solar heat, when the solstice is in the perihelion or aphelion, depends, will not be  $1 - e$  and  $1 + e$ , but  $1 - \frac{1}{2}e\sqrt{3}$  and  $1 + \frac{1}{2}e\sqrt{3}$ .

Introducing this correction, the lowering of the heat with the two uncorrected values of the excentricity will be 35°·45 and 29°·81, but with the corrected or reduced values .06676 and .05192, it will be 33°·18 and 28°·73; so that, instead of -6°·3 and +1°·3 F. for the extreme or midwinter temperatures, the corrected values would be +5°·8 and +10°·3, or in the earlier period twelve, and in the later period nine degrees higher, than the value Mr. Croll has given.

35. The summer heat, in Mr. Croll's theory, is supposed to depend on wholly different principles from the winter cold. He speaks of it as follows.

“ There is no relation, at the periods in question, between the intensity of the sun's heat and the temperature of the summer. One is apt to suppose, without due consideration, that the summers ought then to be as much warmer than at present as the winters are colder. Sir C. Lyell in his ‘Principles’ has given a column of summer temperature calculated from my table on this principle. Astronomically this is correct, but physically, as shown in ch. iv., it is wholly erroneous, and would convey a wrong impression on the whole subject of geological climate. The summers of that period, instead of being much warmer than at present, would in reality be much colder, notwithstanding the great increase in the sun's heat from her diminished distance.”

36. I think there is not the least solid ground for the contrast here affirmed, and that the want of due consideration is on the other side.

First, let us inquire what will be the summer temperature, if the principle in the previous calculations of midwinter heat is maintained. The contrast will then be between the present heat, when the sun is near the aphelion, and the perihelion heat with the increased excentricity. Adopting the three corrections already introduced, first, of the value of the excentricity, secondly, of the law of radiation, and thirdly, of the maximum heat or cold a month after the solstice, the increase of summer heat would be  $34^{\circ}\cdot88$  and  $28^{\circ}\cdot5$  at the two eras proposed. Thus, instead of  $39^{\circ}$  and  $64^{\circ}$ , the present midwinter and midsummer heat in our island, the temperatures would be, by the corrected rule,  $5^{\circ}\cdot8$  and  $98^{\circ}\cdot88$  for the earlier, and  $10^{\circ}\cdot3$  and  $92^{\circ}\cdot5$  for the later date.

37. The reasons assigned, why glaciation should have resulted indirectly from the increased excentricity about 200,000 years ago are these: First, the midwinter temperature would be lowered to an enormous extent. I have just shown that this is not correct. The decrease would be only  $28^{\circ}\cdot7$  instead of  $37^{\circ}\cdot7$ , and the resulting temperature  $10^{\circ}\cdot3$ . This is nearly the same as that of Canada, near Quebec, while the summer temperature, by the previous estimate, would be almost  $30^{\circ}$  higher. This is wholly different from the conditions of a glacial period.

The winters, it is said, would be longer as well as colder. Instead of being 8 days shorter than the summer, as now, the excess would be 36 days. But for the period mainly in question the difference is 26 days, or 13 days is the excess of the winter over half a year. The mean rainfall of our island is 32 inches. Without some unproved change in the physical conditions, the rainfall of the winter months would be less than 20 inches, or if snow be reckoned six times lighter than water, this would amount to a depth of 10 feet only. But the latent cold of ice is  $140^{\circ}$ , and water has four or five times the specific heat of

most solids. The formation of ice is thus a most powerful means of arresting a decline of temperature, as evaporation is the great natural remedy for excessive heat. The heat required to melt 20 inches depth of frozen water over the whole surface of any portion of land is equal to that of 37 hours of vertical sunshine, if we adopt the datum of Sir J. Herschel, that vertical solar heat on a square foot in one second would raise one pound about one-ninth of a degree. The total summer heat, reckoned roughly, would be equal to 1,300 hours of vertical heat at the equator, or lat.  $54^{\circ}$ , and 900 hours at the pole: hence, if the whole winter rainfall were deposited in snow or ice, the heat needed to melt the whole would be that of four days only nearest to the summer solstice, or one-thirtieth of the whole summer heat in our latitude.

38. The reasoning in "Climate and Time," pp. 58, 59, seems to assume that ice and snow are the cause and not the effect of a cold climate, and tend to aggravate not to mitigate its severity. But the exact opposite is true. As ocean currents tend to equalize the temperature of different parts of the earth, so the formation and melting or evaporation of ice and snow are the chief natural means of lessening the difference of sensible heat in different seasons of the year. When the radiation is in excess of the supply of solar heat, the freezing of water sets free  $140^{\circ}$  of heat to repair the loss; and when the summer returns, all the ice and snow must be melted before the temperature can have a sensible rise above the freezing-point. A pound of water, with a sensible difference of  $180^{\circ}$  only from its frozen state to its evaporation at the boiling-point requires  $1,320^{\circ}$  of heat, and this will be equivalent to  $5,280^{\circ}$  or  $6,600^{\circ}$  degrees for a pound of rock or of earth, the specific heat being one-fourth or one-fifth of that of water. Or, taking the interval from zero to  $70^{\circ}$ , a pound of water, in virtue of the process of freezing and its great specific heat, serves to reduce the sensible change of temperature from twelve to fifteen times.

39. There are three ways in which snow and ice are said to lower the summer temperatures. First by direct radiation. Whatever the heat of the sun, the snow and ice can never rise above  $32^{\circ}$ , and their radiation lowers all surrounding bodies to that level. Next, the rays which fall on them are to a great extent reflected into space, and those which are not reflected, but absorbed, disappear in the mechanical work of melting the ice. Thirdly, they chill the air, and condense the moisture into fogs, and these prevent the sun's rays from reaching the earth; thus the snow, in these aphelion winters, would remain unmelted the whole summer.

Now of these causes the first and third exclude each other. If fogs hinder the sun's rays from reaching the earth, they must also prevent the ice and snow from radiating heat away into empty space. The dull, cloudy surface above must receive and absorb all the heat of the summer sun, and can allow little heat to radiate into space, except at night; even then much less than under a clear sky. Of course, till the ice and snow are all nearly melted, they effectually hinder a sensible rise of heat above  $32^{\circ}$ ; but this is only the converse of their previous effect, in their formation, to hinder a lowering of the temperature till the whole has been frozen. All the heat of the sun which falls on the earth must produce its full effect, either in raising the ice, snow, and the ground itself, up to the freezing-point, or in melting them, and turning them into water or aqueous vapour. The same amount of cold which would depress a stratum of chalk ten feet deep to the zero of Fahrenheit would spend itself in turning  $7\frac{1}{2}$  inches of rainfall into ice and snow. Thus the presence of moisture, whether in the air or the soil, or lakes and rivers, is the most effectual hinderance to excessive lowering of the winter temperatures, so long as the total annual heat received from the sun is not diminished. But in the imagined glacial epoch, this total amount is increased  $\frac{3}{20}$  per cent. for the whole globe, and 3 per cent. for the northern hemisphere.

40. Even with the corrections before named, the calculation cannot lead to a precise result, but shows at the most a limit towards which the temperature would tend, if the solar heat and radiation into space maintained the given proportions for an indefinite period of time. If the rule were sound, some very unnatural conclusions would follow. Each pole, during its winter of half a year, when it receives no heat at all from the sun, would sink to the temperature of space, or  $-239^{\circ}$  F. Again the heat which the pole receives from the sun at midsummer, exceeds that received by an equal surface at the equator in the ratio of  $\pi \cdot \sin. \iota$  to  $\cos. \iota$ , or 1.3638 to 1. But since the summer heat of the equator is  $79^{\circ}$ , or  $318^{\circ}$  above that of space, the midsummer heat of the pole, by Mr. Croll's mode of reckoning, should be  $115^{\circ}$  higher, or  $194^{\circ}$ , little short of the heat of boiling water. Each conclusion is plainly very wide of the truth.

41. Again, Mr. Croll insists forcibly on the vast amount of heat transferred northward by the Gulf Stream. He reckons it equal to one-fourth part of the whole amount received from the sun by the Atlantic area or basin, from  $25^{\circ}$  N. up to the Arctic Circle. The consequent increase of the mean temperature of Great Britain is not less, he thinks, than  $30^{\circ}$ ; but in

estimating the temperature for his glacial epoch this element is omitted altogether. It is plain, however, that it must then have been not much less than it is now. The contour of land and sea was nearly the same as at present in the Boulder Drift period, and the Atlantic basin had nearly its actual outline, and reached as far to the north. The strength of the current must depend on the contrast between the heat of the southern summer and the cold of the northern winter, so far as these were directly dependent on the sun. This would be only 8 per cent. less than it is now. On the other hand the current would be greater in the summer half of the year, and serve more fully to blot out the traces of the cold of the previous winter. The general result would be an increase of summer heat and winter cold, each about  $28^{\circ}$  at the most, but probably much diminished by the equalizing effects of aerial and ocean currents.

42. Another element has still to be considered. In Mr. Croll's Table, p. 320, vol. iii., the longitude of the perihelion at the date B.C. 210,000 is stated to be  $144^{\circ} 55'$ . From the last entries it seems plain that this amount has reference to a fixed and not a movable solstice or equinox, and is the change resulting from the progression of the apsides alone. The change from precession for this same period, at the present rate, would be eight complete circuits and  $46^{\circ} 56'$ . Hence the true longitude of the perihelion, on this view, would be  $144^{\circ} 55' - 46^{\circ} 56'$ , or just  $98^{\circ}$ . Thus the northern summer solstice, as it is now, would be nearly in aphelion. This is precisely the opposite condition to that which forms the basis of Mr. Croll's theory. We need to go backward or forward 10,000 years, to have the winter solstice in aphelion, when the excentricity is  $\cdot 0497$  or  $\cdot 0569$ . In the former case the midwinter increase of cold would be only five-sixths of Mr. Croll's estimate, when his other data are retained, or the decrease, which has been reduced from  $37^{\circ} \cdot 7$  to  $28^{\circ} \cdot 7$ , would be further reduced to  $23^{\circ} \cdot 9$ , or the midwinter temperature by the rule be  $15^{\circ} \cdot 1$ , which is higher than the temperature of Canada.

43. The main principle involved in Mr. Croll's theory is that the cold or hot state of each hemisphere is determined chiefly by its midwinter temperature, and this in turn by the simple ratio of the direct solar heat then received, the excess over the mean temperature of space, or  $-239^{\circ}$  F., being determined by a simple rule-of-three calculation. And since the winter northern solstice is now very near the perihelion, the present excess above the average value, when combined with the deficit at other periods, results in a very considerable disproportion. The ratio, according to Mr. Croll, 850,000

years ago, is about five-sixths; and hence, one-sixth of  $278^{\circ}$ , or  $45^{\circ}$ , will be the aggravation at that date of the winter cold. But if this mode of reckoning were sound, it ought to apply to the northern and southern hemispheres with the present excentricity. In this case the southern winter should be colder than the northern in the amount answering to the ratio  $\cdot 93507$ , or  $18^{\circ}$  F. But in fact there is no such inequality, and it would almost appear that the climate, in answering latitudes, is slightly warmer than in the northern hemisphere, except in the immediate neighbourhood of the pole.

44. The following extract from Mr. Croll's table gives his conclusions with regard to his two proposed glacial periods, and the midwinter temperature of Great Britain at the answering periods:—

Date.	Excentricity.	Perihelion.	Excess of Winter, In days.	Sun's Heat.	Depression.	G. B. mid-winter.
880,000	$\cdot 0456$	$152^{\circ} 33'$	21.2	$\cdot 884$	$32^{\circ} 2$	$6^{\circ} 8$
870,000	$\cdot 0607$	$180^{\circ} 23'$	28.2	$\cdot 859$	$39^{\circ} 0$	$0^{\circ} 0$
860,000	$\cdot 0708$	$209^{\circ} 41'$	32.9	$\cdot 843$	$43^{\circ} 6$	$-4^{\circ} 6$
850,000	$\cdot 0747$	$239^{\circ} 28'$	34.7	$\cdot 837$	$45^{\circ} 3$	$-6^{\circ} 3$
840,000	$\cdot 0698$	$269^{\circ} 14'$	32.4	$\cdot 845$	$43^{\circ} 2$	$-4^{\circ} 2$
830,000	$\cdot 0623$	$298^{\circ} 28'$	29.0	$\cdot 857$	$40^{\circ} 0$	$-1^{\circ} 0$
820,000	$\cdot 0476$	$326^{\circ} 4'$	22.1	$\cdot 881$	$33^{\circ} 1$	$5^{\circ} 9$
240,000	$\cdot 0374$	$74^{\circ} 58'$	17.4	$\cdot 898$	$28^{\circ} 3$	$10^{\circ} 7$
230,000	$\cdot 0477$	$102^{\circ} 49'$	22.2	$\cdot 885$	$33^{\circ} 2$	$5^{\circ} 8$
220,000	$\cdot 0497$	$124^{\circ} 33'$	23.2	$\cdot 877$	$34^{\circ} 1$	$4^{\circ} 9$
210,000	$\cdot 0575$	$144^{\circ} 55'$	26.7	$\cdot 864$	$37^{\circ} 7$	$1^{\circ} 3$
200,000	$\cdot 0569$	$168^{\circ} 18'$	26.5	$\cdot 865$	$37^{\circ} 4$	$1^{\circ} 6$
190,000	$\cdot 0539$	$190^{\circ} 4'$	24.7	$\cdot 871$	$35^{\circ} 7$	$3^{\circ} 3$
180,000	$\cdot 0476$	$209^{\circ} 22'$	22.1	$\cdot 881$	$33^{\circ} 1$	$5^{\circ} 9$
170,000	$\cdot 0437$	$228^{\circ} 7'$	20.3	$\cdot 887$	$31^{\circ} 3$	$7^{\circ} 7$
160,000	$\cdot 0364$	$236^{\circ} 38'$	16.9	$\cdot 900$	$27^{\circ} 8$	$11^{\circ} 2$

45. In the following table, the excentricity is reduced by the formula  $e' = \frac{e}{2} + \cdot 024$  to correspond with Mr. Stockwell's corrected maximum,  $\cdot 0693888$ , instead of Leverrier's  $\cdot 077747$ . The equilibrium of solar heat and radiation is assumed to be  $30^{\circ}$  after the winter solstice, and the law of radiation is taken from Dulong and Petit's experiments. The ratio  $1 \cdot 165 \log. = \cdot 0663259$  answers to a change of  $36^{\circ}$  F., or a change of solar distance to that amount to  $72^{\circ}$ . Hence  $\log. \text{radius vector} + \frac{1}{11} - \frac{1}{200}$  will give the answering change in degrees. The precession at the rate of  $50'' \cdot 3405$  a year, or  $139^{\circ} 50'$  for 10,000 years, is combined with the perihelion places of Mr. Croll's table, to give the anomalies at  $30^{\circ}$  after the solstice. The columns are the date (in 10,000's of years B.C.), excentricities, anomalies,

logarithms of radius vector, change of midwinter heat compared with a mean distance, and results for Great Britain.

88	·0415	302° 47'	·01564	16°·18	48°·41
87	·0544	135° 7'	-·01572	-17°·07	15°·16
86	·0631	326° 0'	·01679	18°·23	50°·46
85	·0664	156° 22'	-·00980	-10°·64	21°·59
84	·0622	346° 46'	·00782	8°·49	40°·72
83	·0558	177° 42'	-·00039	-0°·42	31°·81
82	·0432	10° 16'	-·01058	-11°·49	20°·74
24	·0345	71° 2'	-·01393	-15°·12	17°·11
23	·0433	264° 21'	·01914	20°·78	53°·01
22	·0450	101° 47'	-·01868	-20°·28	11°·95
21	·0517	301° 35'	·01988	21°·58	53°·81
20	·0512	138°·22'	-·01389	-15°·08	17°·15
19	·0480	336°·46'	·00815	8°·85	41°·08
18	·0432	177°·38'	+·00004	°·04	32°·27
17	·0399	19° ·3'	-·00499	-5°·42	26°·81
16	·0366				

In A.D. 1800 the excentricity is ·01678, the anomaly 98°, log. of radius vector in midwinter ·00623, the increase +6°·77 and 39°-6°·77=32°·23 is the midwinter heat of Great Britain, in a circular orbit, to be added to the degrees in col. 5, to obtain the midwinter heat on Mr. Croll's hypothesis, after due corrections.

46. Thus it appears, when the principle of Mr. Croll's calculation is admitted, and necessary corrections are introduced, the midwinter depression, or increase of cold in Great Britain, at his earlier date, B.C. 850,000, would not be 45°·3, but only 10°·6; and that in B.C. 210,000 there would not be a decline of 37°·7, but a rise of 21°·6. At B.C. 220,000 there would be a decline of 20°·3; and this is fourteen degrees less than the amount in his theory. And when we observe, further, that the same principle would involve the consequence, that southern winters should now be 13° colder than at the same latitudes in the northern hemisphere, while there is actually only a very slight difference, the disproof of the hypothesis seems tolerably complete.

47. The way to restore some semblance of truth to the theory is to apply it, not to the periods in round numbers in the table, but to intermediate dates, when the solstice was really in the aphelion. This is nearly fulfilled for the date B.C. 220,000, but neither for B.C. 850,000 nor B.C. 210,000. Indeed at the latter date the winter solstice is almost exactly in the perihelion, and by the hypothesis the midwinter heat would be 21° higher than now, instead of 38° lower. In the other case the solstice has the anomaly 126°·22', by the approximate reckoning. The rate of change is 139°·50 + 29°·47=

169°·37 for 10,000 years. To bring it to 60°, which is nearly the position of maximum effect, would require an interval of 3,900 years, or a date from A.D. 1800 backward, of 846,100 years. The corrected exentricity would then be about ·06476, instead of ·0664 or ·0747. The depression, by the corrected rule, at this the most favourable moment, since the logarithm of the radius vector at the aphelion would be ·02725, will represent a diminished heat, compared with a circular orbit, of 29°·58, or 2°·7 F., and this will be counteracted by a summer heat, exceeding the present by 24°·8 F., or an average of 89°.

48. The other periods most favourable to the effect of depressing the northern winters will be, reckoning backward from A.D. 1800 as before.

823,000	diminution from present winter heat	28°·4	result	10·6
217,400	”	”	”	10·7
195,100	”	”	”	9·4

Now, when we remember that the approach to the maximum would last only one or two thousand years; that the summer, in each case, would be hotter than at present by all the contrast between the present aphelion and the past perihelion distance; that the heat annually received by the northern hemisphere at these periods is 3 or 4 per cent. above the mean amount; and that the actual difference of the northern and southern winters, which by the same scale should be 13°·7, or nearly half the whole amount, is in reality hardly sensible, I think the presumptive evidence is irresistible in favour of the view of Sir J. Herschel, Arago, and others, which Mr. Croll reverses as erroneous; that the differences of exentricity, within their actual limits, will by no means account for the occurrence of glacial periods.

49. There is another hypothesis, wholly distinct from that of Mr. Croll, which seems to me to admit of being confirmed by very strong presumptions. It is that which refers the main stages of geological change to marked eras of chemical transmutation, in the latest stages of terrestrial condensation. But this cannot be unfolded at the close of a paper which has already reached rather an undue length.

I think I have sufficiently shown that the chief definite grounds, of astronomical science, upon which the doctrine of man's extreme antiquity has been assumed to rest, are wholly fallacious and unsound.

The CHAIRMAN (C. Brooke, Esq., M.D., F.R.S.).—I am sure that we all unite in returning our best thanks to Professor Birks for the very able paper which he has read.\* It is now open for those present to make observations thereon.

Rev. Prebendary CURREY, D.D.—I feel incompetent to enter upon the details of the arguments which have just been presented to us with reference to the special theories which Professor Birks has discussed; in fact, the accumulation of scientific research and of learning in his paper has been so great as wholly to bewilder me. But what I want to point out is this, that the question before us is “modern cosmogonies examined in their bearing upon the antiquity of man,” and I confess that to me it is very difficult to understand what bearing a great deal of this paper has upon the subject of the antiquity of man. Let us suppose for a moment that all the conclusions which Professor Birks seeks to set up are clearly established, and that all the theories which he attacks are completely overthrown, still, in my opinion, that would not affect the question of the antiquity of man. All that it would do would be to show us that certain theories put forward by particular philosophers are liable to exception, and are, perhaps, unsound; but it would not necessarily follow that other theories may not be quite sound. The destruction of each theory can only affect such others as proceed upon similar lines; and even those only so far as they concern the subject in hand. Professor Birks’s arguments have to do with the antiquity of the earth, rather than with that of man. Now if you can prove that certain strata, containing the remains of man, are not so old as has been represented, you may make it probable that man has not been so old an inhabitant of the earth as some suppose. The paper does not refer to any special antiquity of man,

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\* Since the meeting Mr. Brooke has sent the following observations, which he intended to have made towards the close of the discussion:—

“I wished to have made a remark, had time permitted, on § 13 of Professor Birks’s paper. I cannot see that, ‘the hypothesis that the heat transferred from a hot to a cool body is strictly as the difference of their temperatures, and that the temperature is the quotient of the heat in any body divided by the mass,’ implies the corpuscular theory of heat. Speaking logically, it must be borne in mind that heat has no *objective* existence; it is a *subjective* impression on the organs of sensation produced by certain molecular wave-motions. If we now suppose two contiguous particles of different bodies to be affected by different amounts of wave-motion, and that the whole motion be then shared between them, it is clear that one must have gained, and the other lost half the difference; which is the same thing as saying that the amount of heat transferred is as the difference of the temperatures of two bodies. It also appears to me equally clear that if a given amount of heat wave-motion, distributed through a given number of particles, be shared with an equal number previously at rest, each particle of the whole will have half the wave-motion that previously affected each of the first-mentioned particles: this amounts to the same thing as saying that the temperature is the quotient of the heat in any body divided by the mass. It therefore appears to me that the matter-theory of heat is *not* involved, as stated by Professor Birks.”

but it considers different theories of great antiquity assigned by philosophers, not to man, but to the surface of the earth and its formation. But, even supposing that to be unsound, and suppose the conclusion is that the earth is not by any means so old as it has been represented to be, and that therefore man, whose remains have been found in it, is not so ancient as has been represented—suppose all that to be established, surely that does not show that there is not still an immense antiquity to fall back upon. Suppose you reduce the past ages of the world's existence from 120,000,000 years to 50,000,000 years, you will still find 50,000,000 years quite enough to deal with. (Laughter.) From the alluvial deposits of the Mississippi the ages assigned by Lyell may have been reduced to not more than 94,000 years; but though Lyell's first calculation may not be maintained, still a period of 94,000 years would carry the antiquity of man back to a time far more remote than any one has as yet asserted. Suppose, then, that all these statements of the antiquity of the earth are greatly exaggerated and overdrawn, does Professor Birks deny that the Glacial period is removed from the present time by a very large number of years—perhaps hundreds of thousands? It seems to me to have been indubitably established and maintained by every geologist of repute, that the period during which the earth's surface has existed is sufficient for us to trace a number of years immensely greater than those periods which we have been accustomed to consider as belonging to the duration of man; and, if that be so, I do not see that we gain anything except a reduction from 250,000,000 to 50,000,000 years; and even though the strata in which the remains of man are found may have their age reduced to tens or hundreds of thousands of years, instead of to millions, still that gives us an antiquity far beyond anything we have been accustomed to assign to the existence of man upon the earth. Therefore I do not see that this very elaborate, scientific, and learned paper helps us much with regard to the antiquity of man in relation to the date here assigned to it. We must remember that the paper sets out by determining very absolutely the number of years to which we must limit the existence of man, which we are not permitted to set down at more than 7,000 or 8,000 years. That is laid down as an absolute proposition; and, more than that, we are told that if we should assume or arrive at a conclusion which places it 10,000 years back, we are not only scientifically wrong, but we have abandoned the very foundation of faith, and we can maintain neither the Bible nor the truths of Christianity. That, I must say, surprised me beyond measure. To be told that if we venture to assume that man has been upon the earth longer than 7,000 or 8,000 years, we are not only wrong, but we contradict the statements of the Bible, and at least implicitly deny the doctrine of the redemption of mankind;—that, I think, is a most dangerous argument. If you lay down certain propositions with regard to facts which are greatly in dispute, or which, at all events, are not generally accepted, and say that any man who differs from you in regard to them is abandoning the doctrines of Christianity, then

I say you are using an argument of the most dangerous character, and one of a kind which I think this, above all other societies, is bound to cry out against, and to disown. The principle of this Society is to reconcile science with Christianity, and to find out, as far as we can, how far the truths of Christianity may be harmonized with the discoveries of modern science; and we find a number of scientific men, including nearly all of the greatest eminence, holding the view that man's age upon the earth is considerably longer than 7,000 years. We must not, even though they may be wrong in their opinions, turn round and tell them that they are infidels, that they are abandoning the principles of Christianity, and that they cannot possibly hold the doctrine of redemption. Our purpose in this Society is, as I have just said, to endeavour to find out how far we can reconcile science and Christianity, and not to place them directly in opposition, as it certainly seems to me that this paper does, from the statements which it makes at its commencement. That is the reason why I cannot help speaking perhaps rather strongly in reference to these propositions. As to the arguments and theories, I am by no means competent to enter upon them, even if I desired to do so; but I do not think they affect the question. But do not let us lay down principles of the kind involved in saying that those who do not agree with you do not hold the doctrines of Christianity. It is the fact that many clergymen do hold views of the kind which Professor Birks condemns, and he seems to condemn them for doing so; but I must say that this is not the manner in which I like to see scientific questions dealt with, holding it out as matter of reproach to any one who dares to hold a contrary opinion. This question of the antiquity of man is an open one, and may be held as an open one by clergymen as well as by other people; and often those clergymen who examine it will find themselves forced to come to conclusions to which Professor Birks is opposed. I am not pretending to discuss this question scientifically, but, like other men, I have read the ordinary works on the subject. Look at this matter historically, look at the monuments to be found in Egypt. Some of those monuments certainly go as far back as the time of Abraham; and you will find that even those old monuments represent the different races of man as existing at present; the negro with all his peculiar characteristics, and various other peoples also. All these variations arising in the few hundred years that elapsed between the date of the Flood and the time of Abraham; is not this a most striking proof that you must carry your date farther back? (A voice: "No," and laughter.) Well, I do not say that my opinion is to be taken dogmatically. I only state it as it presents itself to my own mind. In maintaining my own views I bring forward strong arguments, as they appear to me, for the great antiquity of man; I will not say how great, but certainly much greater than those dates which are said to be deduced from the Bible. We must not forget, however, that the Bible has no chronology, that what we accept as the chronology of the Bible was formed by the ingenious calculations of Archbishop Ussher; and we know that many people, quite independent of the scientific question, hold views of Biblical chronology

which are widely different from those of Ussher. They differ most materially. Hales's system of chronology is certainly not the same as Ussher's. Ussher's was an ingenious calculation, but it is not to be accepted as part of the Bible. We have been so accustomed to see those figures 4004 put opposite to the first chapter of Genesis, in the account of the Creation, that we are considered to be almost abandoning our Bible if we do not accept them. A religious society, in publishing the "Commentary on the Bible," was bold enough to say that the early dates of the Bible did not seem to be sufficiently clearly established to warrant their insertion; and some remonstrances came from earnest men, who said, with alarm, "You are attacking the Bible." This is the way in which a great amount of injury may be done to the cause of truth and of religion. We assume certain interpretations of the Bible with which we have been familiar, and we tell people "if you do not accept these, you cannot accept the doctrines of Redemption." That is a line of argument against which I must emphatically protest. I have referred to the monuments of Egypt as bearing upon the question of dates, and from these I cannot come to any other conclusion than that they afford a much greater antiquity for man's existence than 7,000 years. Then look at language.\* Trace it in all its families and their connections as far as you can; and does not the form of those various tongues, with their peculiar characteristics and differences, require a longer time for growth than these few thousand years? To my mind a very much longer time is required. It may be said that we have a dispersion of tongues at the building of the Tower of Babel, but all I can say is, we cannot suppose that in that dispersion of tongues languages were divided out as we now have them, for they all show the marks of gradual progress and gradual formation. If we argue at all, we must argue upon things as we see them; and if we see traces of the progress and improvement of language by gradual stages, we are not to go back and say, all these could have been done miraculously at the building of the Tower of Babel. God does not work with His creatures in that way; He does not invent these things in order to cheat us, and give us historical evidence of what is not historical. Whether we examine the crust of the earth, or the history of language, or the monuments of Egypt, all we can do is to take them on the principle that we are to read their history and their progress in the same manner as we read the history and progress of what is before us. We need not maintain the strict uniformitarian system, that exactly the same rate of deposit was to be laid down every year. A great accumulation of worthless conjecture has been obtained by calculating the geological deposits that we have, and saying they must have taken 200,000 or 250,000 years to produce. All that is extremely vague conjecture, but it does not destroy the main evidence of the great broad facts; and I say look

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\* These two points are treated on in the *Transactions*, Vol. III. p. 464, *et seq.*

at the great broad facts of the Mississippi again. You say that the Mississippi deposits did not occupy vast numbers of years; but I would ask, where is the theory which will account for these deposits, except by the assumption of a great number of years? I do not say any particular number of hundreds of thousands, but certainly a very large number. Let any one bring forward a counter theory if he can. I do not want to express the least disrespect to Professor Birks. He forms his own conclusions, and everybody knows that he is a great master of mathematics, and a vast accumulator of knowledge, but I would point out the importance, in a society of this kind, of refraining from putting forward such an argument as that no one is to hold a particular view on such a question as the antiquity of man, without being liable to the suspicion of denying the doctrines of redemption, and giving up the possibility of maintaining the truths of Christianity.

Rev. A. G. PEMBERTON.—I have listened with great interest to the reading of this paper, but I have drawn conclusions very opposite to those expressed by Dr. Currey. I thought it most valuable that so great an authority as Professor Birks, with great scientific knowledge, should grapple with these scientific questions. I did not gather from the paper that he contended for the accuracy of Archbishop Ussher's chronology, and I quite agree that we need not defend any such calculations. My Hebrew Bible has no chronological calculations at all. Hales's valuable work is simply a compilation of various systems of chronology. There can be no question that the range of knowledge which is knowable is, as that great intellect Newton pointed out, extremely limited, and man's ignorance is immense when compared with his knowledge. As Jeremy Taylor has said, the most learned pundit would find, if he came to compare his ignorance with his knowledge, that the ignorance immensely outweighed the knowledge. Then we must also remember that geology at present is only in its infancy, and I feel sure that as it grows and increases, our knowledge of the past, we shall find that there is no real antagonism between science and the Bible.\* Now so far as natural religion goes, we know that it does not reveal a single syllable about redemption through Christ. The whole of that sublime economy, which is as beautiful as it is sublime, entirely depends on the authenticity, genuineness, and inspiration of the Scriptures. Every man, therefore, who would grapple with the subject fairly, should inquire whether the Bible be an authentic document, whether it be genuine, and whether it be inspired, and if he do this, he will come to the conclusion which the great Grotius, a man as illustrious for the splendour of his genius as for the extent of his attainments, came to, when he wrote his remarkable book *De Veritate*. The acute-minded Le Clerc too, who, from being an unbeliever, became a believer, made objections to the Pentateuch: he was answered, and, being an honest man, he went and studied the subject more deeply, and then wrote a refutation of his own objections; but Voltaire has copied the objections

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\* See Professor Dawson's remarks, Preface to Vol. XI.—Ed.

without the answers into his *Philosophical Dictionary*. The infidelity which has arisen in the present day is peculiarly injurious to the young, because it assumes what is false,—that there is an antagonism between true science and religion, whereas there is really none. I myself have not the leisure or the opportunity to go deeply into all the questions which are raised by the paper of Professor Birks, but I am glad to find so able an advocate coming forward, with learning, great powers of mind, and accuracy of thought, to go into the depths of the subject, and to show that those men who differ from the Scriptures as to inspiration and as to the doctrines of our redemption through our Lord Jesus Christ, are in the wrong, and ground all their objections upon mere supposition and conjecture, without a line of history or an atom of real proof to support them.

Rev. J. J. COXHEAD.—The existence of an ice age, of which we find many traces, being acknowledged, it appears to me that we are bound to accept Mr. Croll's hypothesis, which seems probable, until a more satisfactory one is substituted for it. (Dissent.) I think that the existence of an ice age and the finding of supposed human implements in the Drift are arguments in favour of the antiquity of man.

A MEMBER.—But the periods of the Ice age and of the Drift have to be ascertained.

Mr. T. K. CALLARD.—Dr. Currey has told us that he could not see what bearing the learned paper we have listened to has upon the question of Man's Antiquity. It might be that Dr. Currey expected more than was proposed by the author. I do not think that Professor Birks supposed that, after reading his paper, we should leave to-night, certain that there did not exist a great antiquity of man, but if he has succeeded in removing one of the strongest arguments that has hitherto been used for assigning to man such great antiquity, I think he has done all that could be expected from him in one evening (Hear, hear), and I think he has very successfully done this. It has been accepted by most of our leading geologists, that man first appeared on the globe some 200,000 or 210,000 years ago. But how was that period arrived at? It was by accepting that as the time of the Glacial epoch; for, as Professor Birks says in his second paragraph, "Human deposits are thought to occur in quaternary strata or drift, directly after the close of a great ice period." If that great ice period, then, was 200,000 years back, and the human deposits occur immediately after its close, you have the case proven that man lived 200,000 years ago. But there is nothing whatever, either in astronomy or geology, to fix that as the date of the Glacial epoch, except the excentricity of the earth's orbit, which was so great at that period. Now, if Professor Birks has made it clear to your minds, in answer to Mr. James Croll's hypothesis, that neither the excentricity of the earth's orbit, nor the changes produced by the precession of the equinoxes, nor the altered obliquity of the ecliptic; that none of these astronomical changes, nor all of them put together, would have produced an ice age; if he has made that clear, we then must give up the

200,000 years as the date of the Ice age, and also as the date of the men who left the "human deposit" referred to in the gravel drift. I think a great step has been taken to-night if Professor Birks has established this one point. I reached the same conclusion as the author of the paper has done, when the hypothesis of Mr. James Croll was first published, and feel honoured by Professor Birks' reference to my pamphlet, and I scarcely need say that the conclusion I then reached has been greatly strengthened by to-night's paper. There may be, as stated by Dr. Currey, other reasons for believing in the great antiquity of man, most of which reasons will be no doubt brought under consideration when Professor McKenny Hughes (Woodwardian Professor of Geology) reads his paper upon the subject; but there are no other reasons that can be produced, except those to which Professor Birks has replied, that will fix 200,000 years as the period of man's introduction to the earth. I would like now to offer a remark or two upon the "human deposits" of the drift; they are described by Professor Birks as flints, which "are affirmed to have been plainly fashioned into tools, spears, or hatchets by the hands of savage men." If the affirmation is correct, the antiquity of the savage men who fashioned them is not proven, unless the age of the drift in which they are found is also proven: but if, on the other hand, there should be reasonable doubt about the human fashioning of these flints into tools, spears, or hatchets, the evidence for man's antiquity will be considerably reduced. I will confine my remarks to the affirmed implements, &c., of the gravel drift; those from Brixham Cave were, in my judgment, satisfactorily disposed of in a paper read by Mr. Whitley before this Institute. But the implements of the gravel drift demand more careful consideration. I have seen that beautiful collection in Blackmore Museum, Salisbury; and some of the still finer specimens in the possession of Mr. John Evans, the President of the Anthropological Society. I have looked at them until I have been hardly able to doubt the human origin claimed for them. But then I have to bear in mind that these are very choice specimens, virtually selected from some thousands of other broken flints that bear more or less resemblance to these chosen ones. I have seen about a thousand together at the residence of the late M. Boucher de Perthes, at Abbeville; they were collected from the implement-bearing gravel in that neighbourhood, but I do not think that there is any one present who would not at once dismiss two-thirds of them as simply flints that had met with accidental fracture, yet all bearing a certain resemblance to the better forms. Here is a very fine specimen of the spearhead type [Mr. Callard produced a specimen, which was handed round the room for inspection]; it was found in the gravel-bed of Moulin Quignon, and no believer in drift implements would question the human fashioning of this specimen. But here is a broken flint which I took out of the same gravel-pit [the specimen was shown] which I do not think that any member of this Institute would claim for a human implement; but when the other side of the flint is presented to you, it exhibits the same *outline* as the accepted spear-

head. I also, from the same gravels, obtained this specimen [another specimen shown], which bears not the faintest resemblance to spear-head, hatchet, or to any other implement, but you will observe that the surface is covered with the minute chipping and flaking, that, had it occurred on the other specimen with a spear-head outline, it would certainly have been received as one of the implements fashioned by the hands of Palæolithic man. I will now show you a flint which I obtained in the neighbourhood of Marlborough Downs [specimen exhibited]; it has not yet been out of its matrix, therefore could not have received its form from the hand of man; it is incased in silicious sandstone, and it has so happened that the blow given to the stone by the mason has split the flint longitudinally, which affords a good opportunity of examining its natural form, and if you compare it with the accepted implement from the gravel-bed of Moulin Quignon, you will observe that both in size and shape they are identical; in addition to which, the exposed part of the flint is covered with facets. As there is no *collateral* evidence whatever to support the claim of these chipped flints being the work of man, the evidence of their being such resting exclusively upon their form and chipping, and seeing that nature does produce similar forms, which by natural causes can get similarly chipped, I think we may be justified in some hesitation in accepting these flints, however remarkable they may appear, as the workmanship of Palæolithic man. To say the least, they appear too doubtful to be made the basis to support the theory of man's great antiquity.\*

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\* The greater or lesser antiquity of the *earth* in no respect affects the question of the antiquity of *man*. No scientific man has thought of placing man farther back than the Miocene period, and but few would claim for man a greater antiquity than that of the Gravel Drift. The reasons which would lead to claiming a great antiquity for the former are totally different to those that are adduced for the antiquity of the latter.—(T. K. C.)

With respect to certain well-known theories requiring vast epochs for geological changes. In a work just published, *Recent Researches in Physical Science*, Professor P. G. Tait says that the Uniformitarian theories of geologists are "totally inconsistent with modern physical knowledge as to the dissipation of energy"; he then speaks of "the Law of the Dissipation of Energy, discovered by Sir W. Thomson," and remarks, "It enables us distinctly to say, that the present order of things has not been evolved through infinite past time by the agency of laws now at work, but must have had a distinct beginning—a state beyond which we are totally unable to penetrate, a state which must have been produced by other than the now (visibly) acting causes." And, arguing from our present knowledge of radiation, against the claims of "Lyell and others, especially of Darwin, who tell us that even for a comparatively brief portion of recent geological history three hundred millions of years will not suffice," Professor Tait quotes Sir W. Thomson's three lines of argument, and urges, "Ten million years is the utmost we can give to geologists for their speculations as to the history even of the lowest orders of fossils" and "for all the changes that have taken place on the earth's surface since vegetable life of the lowest known form was capable of existing there." Of course, it remains to be seen how far future researches may induce others to modify the above statements (vol. x. p. ii.).—Ed.

Rev. T. M. GORMAN.—I must dissent from one portion of Professor Birks' statements, for in the text of the earlier chapters of *Genesis* I cannot discover sufficient data for an exact chronology; but we may be sure that the true chronology would harmonize with the *facts* of science.

Captain F. PETRIE (Hon. Sec.).—Without offering any opinion upon the special question raised in the paper, I venture to refer to two remarks made by Dr. Currey: the first is that in which he alluded to Sir C. Lyell's calculation as to the antiquity of man in the Mississippi valley. Sir C. Lyell, in the fourth edition of his *Antiquity of Man* (1873), refers to only two instances of fossil human remains having been found in the Mississippi valley; the first being that of the skeleton of a Red Indian, the cranium in good preservation, found 16 feet below the surface when excavating for some gas-works: Dr. Dowler considered it to be 57,600 years old. Sir C. Lyell cites his opinion with apparent approval (p. 46), and gives his reasons, founded upon a calculation as to the rate of deposit of the mud; but Messrs. Humphreys and Abbot, quoted by Sir C. Lyell in the later edition of his work as reliable authorities, have calculated that the whole ground on which New Orleans stands, down to a depth of 40 feet, has been deposited in forty-four centuries. In regard to the second instance of fossil human remains, Sir C. Lyell says, "It is necessary to suspend our judgment as to the high antiquity of the fossil" (p. 239). To show the rapid rate of deposit in the valley, M. Fontaine mentions that near Tamaulipas Street, New Orleans, the whole area to the depth of over 100 feet has been deposited within the last sixty years; and that since the construction of the gas-works, some deep excavations at Port Jackson, at a considerable distance from the river, and at a depth of from 15 to 20 feet below the surface, a piece of wood shaped by human art had been found, which on examination proved to be a portion of a modern boat. In a work entitled *The Recent Origin of Man* it is mentioned (p. 472) that the body of a man, which had been buried between two stumps of trees, had been covered by the deposit of the river to a much greater extent in four years than even 16 feet. With respect to the discovery of fossil human remains, many have been found, in regard to every one of which some controversy has taken place: a skeleton in the British Museum is a curious example; it is that of an Indian, killed in battle only two centuries ago; it is embedded in solid rock, and came from the North-west coast of Guadeloupe, where "the rock is a limestone, harder than statuary marble, and is forming daily: it contains minute fragments of shells and coral, encrusted with a calcareous cement resembling travertine, by which the particles are bound together: the skeleton still contains some of its animal matter and all the phosphate of lime." (*Recent Origin of Man*, p. 78.) The foregoing remarks may show some of the difficulties with which we have to cope in our search for geological facts which will throw light upon "the antiquity of man." At the recent conference, held on May 22, 1877, the President, Mr. John Evans, F.R.S., "pointed

out the extreme caution which was necessary in dealing with the subject, as it lay within the domain of the archæologist, the anthropologist, and the geologist; neither of whom was sufficient, alone by himself, to offer a very strong opinion on the subject. Great care was also necessary with regard to the facts of the discoveries themselves, as the objects discovered were liable to get mixed with other objects below them; and this was important in the case of cave-deposits, in which there might be interments of a later date than the human skeletons deposited in the caves. The question was now very much within the province of the geologist, whose business it was to determine the antiquity of the deposits in which the discoveries may have been made. After alluding to several recent discoveries in France, Spain, and Switzerland, the President remarked that each successive discovery, or presumed discovery, must be received in a cautious but candid spirit; and, looking to the many sources of doubt and error which attached to isolated discoveries, their watchword must for the present be "caution, caution, caution." With regard to the physiognomy of the negro, as delineated upon ancient monuments being the same as that existing in the present day, a well-known fact should not be forgotten, namely, that a special type will develop rapidly, and then remain to all appearance permanent; the writings and investigations of Dawson, Parker, and others have shown this.\* Finally, I do not think we can, in any of our scientific investigations in regard to these subjects, have a better watchword than Mr. Evans's, the more we investigate and the more we know, the more will this appear; and I hope our faith is not held so lightly as for us to allow its safety to be compromised by the lights and shadows which may fall upon it during our labours.

Professor BIRKS.—I think it should hardly have been expected that I could, in one paper, treat the whole of the large question which my subject involves. I have only dealt with one specific point on which the theory now in vogue, for insisting on the high antiquity of man, mainly rests as a definite result of science. I should be sorry to have it supposed that I say that any one who does not accept my view of the antiquity of man is an infidel. I only say that so far as that point is concerned he departs from the Bible testimony. I do not mean to say that any one who does not believe in the one point of the 7,000 or 8,000 years does not believe in 19-20ths of the Bible absolutely and in the New Testament, but he seems to me to have surrendered one integral part of the whole message, and in so doing he impairs his faith in the rest. I do not deny an ice age, but I have a view of my own which is quite consistent with the narrative of the Bible.†

The meeting was then adjourned.

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\* Vol. X. p. 384.

† Professor Andrews and other Americans have argued that the Ice age ended scarce 8,000 years ago; Sir C. Lyell and Mr. Geikie admit that the Glacial period in Scotland may be brought down to the "Polished Stone age," or 6,000 years ago. (*Recent Origin of Man.*)

## CONCLUSION OF PROFESSOR BIRKS' REPLY (COMMUNICATED).

My second paper, like my first; in which I have sought to repel the charge that the Bible is inaccurate, and opposed to the certain and proved conclusions of science, has brought upon me a strong censure from Dr. Currey. He thinks my defence mischievous and unsound, though he does not profess to understand it as a scientific argument. He thinks it lost labour to show that five or six different theories, upon which the dogma of man's high antiquity has been based, are erroneous, and exclude each other, unless I can prove the same, in this one paper, of every possible hypothesis or presumption of the same kind. I am astonished at such a test of valid argument in defence of the thorough truth of the Bible being laid down by any one. I must strive to clear away the mist which would make my labour almost fruitless unless it be removed. The basis of my argument is that the Bible does not merely contain the "Word of God" somewhere within it, but is itself "God's word written," or a series of messages which the Holy Spirit spake by the prophets; that it is truth, "the true sayings of God," and not an imperfect mixture, in unknown proportions, of God's truth with numerous errors; that hence it is not lawful for any Christian "so to expound one part of Scripture as to be repugnant to another": this could only be true if it contains no real self-contradiction. If the Scripture, then, is God's word, and all self-consistent, it cannot contradict genuine science. Two kinds of contradiction are possible, and very frequent. False constructions of Scripture may be opposed to true and sound conclusions of science; and false conjectures, hypotheses, and inferences of students of science may contradict alike the real truths of science and unambiguous statements of the word of God. Wherever there is a seeming collision, the duty of every honest Christian is to inquire, first, what is its real source,—a false interpretation of the Bible, or of the works of God, and the facts of science. Now, I cannot defend the Bible from infidel assaults under these two unfair conditions—unlimited scientific credulity, and an unlimited license of non-natural interpretation of the Bible, so as to impute to it the almost entire absence of any definite meaning. In the present paper I am said to have charged all with being infidels who do not accept "Ussher's" chronology, and to have made this one essential part of Christian orthodoxy. I am astonished at the charge, when I have done the exact reverse. I named a limit for the Bible date of man's entrance on the earth, which includes the highest estimates of those who do not altogether discard the Scriptural testimony concerning it. There may be Christians who, in deference to the inferences or guesses of modern geologists, can accept some such paraphrase as this of the earliest link in St. Luke's genealogy of Christ. Having climbed some four or five thousand years to Seth in seventy ascents, then, in order to complete a hundred thousand years, they must proceed: Who was the son of Adam;

who was the son in a thousandth descent, of some pre-Adamite man, who was the son, in the ten thousandth generation, of some ape, chimpanzee, or gorilla, which was the son, or creature at least, of God. But those, if such there be, who can stretch the words of God so far, to make them fit the supposed exigencies of modern thought, will never persuade infidels that they are honest in this process of accommodation. The author of *Supernatural Religion* speaks with contempt of "the profoundly illogical zeal of distinguished men within the Church," who endeavour "to arrest for a moment the pursuing wolves of doubt and unbelief by throwing to them scrap by scrap every element which does not quite accord with current opinion." The nature of my own argument is clear as the day. If distinct and repeated statements of the Bible, linked with the very foundations of the faith, are to be rejected, something more than a "perhaps" or "peradventure," or loose notions about what we think was the probable lapse of time from Adam to the first negro, can alone warrant their rejection. Now the one definite argument I find amidst a sea of conjectures and loose guesswork is this, that traces of man's presence are first found soon after what is called the Glacial age or Boulder Drift period. Next, Mr. Croll, in an elaborate and ingenious theory, very widely accepted, ascribes this to a definite astronomical cause, and places it just about 200,000 years ago. I have shown, on the grounds of pure science, that this theory, however great the labour and skill bestowed upon it, is radically defective, and that at the period in question the more correct and scientific conclusion is, that the winter in Great Britain would be just as cold as the winter in Canada, but the summer heat 30 deg. higher than the summer of Canada or our own. To complete the defence of the Bible from its assailants under this head, it would be needful to propose a different explanation of the facts, in harmony with the statements of Scripture. This I think that I see clearly, and I shall hope to unfold it at some future time.

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#### REMARKS BY C. R. BREE, M.D., F.Z.S.

Human remains have not been found in any well-marked geological stratum. Certain implements, said to have been of human manufacture, have been found in caves, gravel, and kitchen-middens of doubtful age, though evidently much older than the time allotted to man's existence on earth. But, as Dr. Currey remarks, we have no definite human chronology mentioned in Scripture; so there is no contradiction. There can be no doubt but that man lived on the earth much before 7,000 years ago, but we have no proof in the records of geology that his life began in any well-known geological epoch. The real fact of value is that no remains of man or his antecedent, "the hairy cocked-eared wild man" of Darwin, have hitherto been found in any geological stratum. The paper certainly does not deserve the charge brought against it in Dr. Currey's concluding remarks.