TRANSACTIONS

OF THE

GEOLOGICAL SOCIETY OF GLASGOW.

XIII. *Of* Geological Dynamics. By Sir William Thomson, LL.D., F.R.S.

(Read April 5, 1869.)

Part I.—*Reply to Professor Huxley's Address to the Geological Society of London, of February 19, 1869.*

Part II.—*Origin and Total Amount of Plutonic Energy.*

Part III.—*Note on the Meteoric Theory of the Sun's Heat.*

PART I.

1. In a recent address (February 19th, 1869) to the Geological Society of London, from the Presidential Chair, Professor Huxley directs attention to the two following sentences, which he quotes from my lecture on "Geological Time," delivered to this Society on the 27th February, 1868:—

"A great reform in geological speculation seems now to have "become necessary. * * * * * *
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"It is quite certain that a great mistake has been made—that "British popular geology, at the present time, is in direct opposi- "tion to the principles of natural philosophy."

2. Professor Huxley attempts to answer these charges, and appeals to "that higher court of educated scientific opinion to which "we are all amenable," for a verdict of "not guilty." He prefaces "his pleading" with the following remarkable statement:—"As "your attorney-general for the time being, I thought I could not do

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"better than get up the case with a view of advising you. It is "true that the charges brought forward by the other side involve "the consideration of matters quite foreign to the pursuits with "which I am ordinarily occupied; but in that respect I am only "in a position which, nine times out of ten, occupied by counsel, "who, nevertheless, contrive to gain their causes, mainly by force "of mother-wit and common sense, aided by some training in other "intellectual exercises."

I must, therefore, in the beginning, be permitted to say that the very root of the evil to which I object is that so many geologists are contented to regard the general principles of natural philosophy, and their application to terrestrial physics, as matters quite foreign to their ordinary pursuits. I must also say, that though a clever counsel may, by force of mother-wit and common sense, aided by his very peculiar intellectual training, readily carry a jury with him to either side, when a scientific question is before the court, or may even succeed in perplexing the mind of a judge; I do not think that the high court of educated scientific opinion will ever be satisfied by pleadings conducted on such precedents. But jury and judge may be somewhat perplexed as to what it is on which they are asked to give verdict and sentence, when they learn that Professor Huxley himself makes the gravest of the accusations which he repels as made by me. In the course of his address he describes Kant's Cosmogony; and, pointing out anticipations in it of some of the "great principles" taught in the *Theory of the Earth*, somewhat later, by Hutton, he says, "on the other hand, Kant is true "to science. He knows no bounds to geological speculation, but "those of intellect. He reasons back to a beginning of the present "state of things; he admits the possibility of an end." Professor Huxley does not use words without a meaning: and these mean that Hutton was *not* true to science, when he said, "The result, "therefore, of this physical inquiry is, that we find no vestige of a "beginning, no prospect of an end." The chief complaint on which I am now brought into court is, that I have extended the same accusation to modern followers of Hutton who have used this dictum as a fundamental maxim of their geology.

3. In opening his case, Professor Huxley asks "What is it to "which Sir W. Thomson refers when he speaks of 'geological "'speculation' and 'British Popular Geology?'" then enters on a highly interesting and instructive discussion of various schools of

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geological philosophy, which constitutes the chief substance of his address, and recurs to the question, "Which of these is it that Sir "William Thomson calls upon us to reform." But instead of answering this question he says, "It is obviously Uniformitarian-"ism" which Sir W. Thomson "takes to be the representative of "geological speculation in general." I have given no ground for this statement. Not merely "obviously," but avowedly and explicitly, I attacked Uniformitarianism; but I did not attack geological speculation in general. On the contrary, I anxiously and carefully guarded every expression of my complaint from applicability to other speculations than those involving more or less fundamentally the particular fallacies against which my objections were directed; and the very phrases I used to limit my accusations showed that I had not taken Uniformitarianism to be the representative of geological speculation in general. The geology which I learned thirty years ago in the University of Glasgow embodied the fundamental theory now described and approved by Professor Huxley as Evolutionism. This I have always considered to be the substantial and irrefragable part of geological speculation; and I have looked on the ultra-uniformitarianism of the last 20 years as a temporary aberration worthy of being energetically protested against.

4. In the course of his lecture, Professor Huxley says:—"I "do not suppose that at the present day any geologist would "be found to maintain absolute uniformitarianism, to deny that "the rapidity of the rotation of the earth *may* be diminishing, "that the sun *may* be waxing dim, or that the earth itself *may* be "cooling. Most of us, I suspect, are Gallios, 'who care for none "'of these things,' being of opinion that, true or fictitious, they have "made no practical difference to the earth, during the period of "which a record is preserved in stratified deposits."

It is precisely because so many geologists "have cared for none of these things," which (though not matters of words merely) do certainly belong to the law of Nature, that they have brought so much of British popular geology into direct opposition to the principles of Natural Philosophy. Professor Huxley tells us that they have been of opinion that the secular cooling of the earth has made no practical difference to it during the period of which a record is preserved in stratified deposits. On what calculation is this opinion founded? One considerable part of the reform in

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geological speculation for which I ask is, that evidence adduced in favour of the opposite opinion should be thoroughly sifted, and not merely disposed of as matters of opinion, or of faith beyond the realm of reason.

5. It was, however, in reference to the special subject of my paper, "Geological Time," that I chiefly urged the necessity of reform, and it is satisfactory now to see that in this respect considerable progress must have been made, when, on the 19th February, 1869, Professor Huxley ventured before the Geological Society of London to suggest that "the limitation of the period during which "living beings have inhabited this planet to one, two, or three "hundred million years, may be admitted, without a complete "revolution in geological speculation." When he says that on me rests the *onus probandi* of my assertion in January, 1868, "that a "great reform seemed to have become necessary," as I had brought "forward not a shadow of evidence" in support of that assertion, I cannot complain that he puts a heavy burden on me. No moderately well read or well instructed student of modern British popular geology wants evidence from me, in addition to that supplied by his reminiscences of books and lectures, that the admission of such a limit as even worthy of attention, is a sweeping reform. Here, however, is some of it, if desired. [The italics are mine in each case.]

6. "So[1] that, in all probability, a far longer period than 300 "million years has elapsed *since the latter part of the secondary* "*period*."

7. "Again,[2] where the FORCE seems unequal to the result, the "student should never lose sight of the element TIME: *an element* "*to which we can set no bounds in the past*, any more than we know "of its limit in the future."

"It will be seen from this hasty indication that there are two "great schools of geological causation—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 also for the most part over wider areas. *The* "*former belief is certainly more in accordance with the spirit of right*

[1] Darwin's "Origin of Species," Edition 1859, page 287.

[2] Page's Advanced Text Book of Geology, 1859. Page 338.

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"*philosophy*, though it must be confessed that many problems in "geology seem to find their solution only through the admission of "the latter hypothesis."

8. "Any [1] person who has paid even the slightest attention to "the science of geology must be aware of the fact that the whole of "our knowledge in regard to age in this science is confined to "relative age, and that with respect to absolute age we have little "or no real information; and in this absence of positive knowledge "as to the absolute age of rocks, geologists have sometimes indulged "in the wildest and most extraordinary statements and speculations. "*They speak of the enormous lapse of time requisite for the forma-* "*tion of exceedingly small quantities of rock, in a manner that* "*would almost make us suppose that some miraculous agency was* "*at work to retard the progress of the formation of these rocks.* "Indeed it has been well observed that the mantle of the preachers "has fallen on the geologists, and that the figures and images by "which the former paint to their terrified audience the duration of "eternity, *a parte post* have been seized on, and adopted by the "geologists in endeavouring to describe eternity *a parte ante*. The "infinite time of the geologists is in the past; and *most of their* "*speculations regarding this subject seem to imply the absolute infinity* "*of this time*, as if the human imagination was unable to grasp the "period of time requisite for the formation of a few inches of sand "or feet of mud, and its subsequent consolidation into rock."

"Professor Thomson [2] has made an attempt to calculate the "length of time during which the sun can have gone on burning "at the present rate, and has come to the following conclusion:— "'It seems, therefore, on the whole, most probable that the sun "'has not illuminated the earth for 100,000,000 years, and almost "'certain that he has not done so for 500,000,000 years. As for "'the future, we may say with equal certainty that the inhabitants "'of the earth cannot continue to enjoy the light and heat essen- "'tial to their life for many million years longer, unless new sources "'now unknown to us, are prepared in the great storehouse of "'creation.'"

"*This result* of Professor Thomson's, *although very liberal in the* "*allowance of time, has offended geologists, because, having been*

[1] Manual of Geology. By the Rev. S. Haughton, F.R.S. Edition 1865, p. 79.

[2] Ibid. Page 82.

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"*accustomed to deal with time as an infinite quantity at their dis-*
"*posal, they feel naturally embarrassment and alarm at any attempt*
"*of the science of Physics to place a limit upon their speculations.*
"It is quite possible that even a hundred million of years may be
"greatly in excess of the actual time during which the sun's heat
"has remained constant."

"Although[1] I have spoken somewhat disrespectfully of the
"geological calculus in my lecture, *yet I believe that the time during*
"*which organic life has existed on the earth is practically infinite,*
"*because it can be shown to be so great as to be inconceivable by*
"*beings of our limited intelligence.*"

9. "The[2] only agent to which we can reasonably attribute the
"destruction and removal of masses of rock, notwithstanding that
"they were many thousands of feet in thickness, and many hundred
"thousand square miles in extent, is the slow and gradual gnawing
"of the sea breakers upon coasts, an action always tending to plane
"down land to a little below the level of the upper surface of the
"ocean."

"The time required for such a slow process to effect such enor-
"mous results *must of course be taken to be inconceivably great.*
"The word 'inconceivably' is not here used in a vague, but in a
"literal sense, to indicate that the lapse of time required for the
"denudation that has produced the present surfaces of some of the
"older rocks, is vast beyond any idea of time which the human
"mind is capable of conceiving."

"Mr. Darwin, in his admirably-reasoned book on the origin of
"species, so full of information and suggestion on all geological
"subjects, estimates the time required for the denudation of the
"rocks of the weald of Kent, or the erosion of space between the
"ranges of chalk hills, known as the North and South Downs, at
"*three hundred millions of years.*[3] The grounds for forming this
"estimate are of course of the vaguest description. It may be
"possible, perhaps, that the estimate is a hundred times too great,
"and that the real time elapsed did not exceed three million

[1] Ibid, page 99.

[2] Students' Manual of Geology. By J. B. Jukes, M.A., F.R.S. 1862.

[3] Prof. Phillips refers to this estimate of Mr. Darwin's; prefers one inch per annum to one inch per century as the rate of erosion; and says that most observers would consider even the one inch per annum too small for all but the most invincible coasts! He thus, on purely geological grounds, reduces Mr. Darwin's estimate of the time to less than one one-hundredth.—PHILLIPS' "Life on the Earth." Cambridge, 1860 (Rede Lecture).

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"years; but, on the other hand, *it is just as likely that the time "which actually elapsed since the first commencement of the erosion "till it was nearly as complete as it now is, was really a hundred "times greater than his estimate, or thirty thousand millions of years.*"

10. It is to be presumed that Professor Huxley repudiates these figures when he says, "if we accept the limitation of time placed "before us by Sir William Thomson, it is not obvious on the face "of the matter that we shall have to alter or reform our ways in "any appreciable degree:" but I am at a loss to understand how he can ask, "has it ever been denied that this period *may* be "enough for the purpose of geology."

11. In marked contrast to them, is Professor Phillips' careful analysis of "the geological scale of time."[1] By reckoning the actual thicknesses of different strata, and allowing $\frac{1}{111}$ of an inch per annum as a not improbable mean rate at which they have been deposited, he finds ninety-six million years as a possible estimate for the antiquity of the base of the stratified rocks: but he gives reasons for supposing that this may be an over estimate, and finds that from stratigraphical evidence alone, we may regard the antiquity of life on the earth as being possibly between thirty-eight millions and ninety-six millions of years. How many orthodox geologists accepted these estimates fourteen months ago? Now, indeed, we have a precisely similar estimate from Professor Huxley himself. And just twelve months ago at a meeting of this Society, Mr. Geikie, declaring his secession from the prevailing orthodoxy, maintained that all the erosion of which we have monumental evidence in stratified rocks, and in the shapes of hills and valleys over the world, could have taken place several times over in the period of a hundred million years.

12. Professor Huxley, immediately after his statement (quoted in § 10 above), "If we accept the limitation of time placed before us by "Sir William Thomson, it is not obvious on the face of the matter "that we shall have to alter or reform our ways in any appreciable "degree;" says, "we may therefore proceed with much calmness, "and, indeed, *much indifference to the result*, to enquire whether "that limitation is justified by the arguments employed in its "support." (The italics are mine.) This method of treating my "case" is perfectly fair, according to the judicial precedents upon which Professor Huxley professedly founds his pleading. I make no

[1] Ibid, p. 119.

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comment or reply, but simply ask permission to put in the following evidence (the italics again are mine) :—"He who can read Sir "Charles Lyell's grand work on the Principles of Geology, which "the future historian will recognise as having produced a revolu- "tion in natural science, yet does not admit how *incomprehensibly* "*vast* have been the past periods of time, *may at once close this* "*volume.*" (Darwin's "Origin of Species by means of Natural Selection.")[1]

13. In the discussion in this Society which followed my lecture on Geological Time, the necessity for much longer periods in geological history than 100 million years was very strongly urged on biological grounds. I answered that Geologists, by estimates of very great numbers of millions of years, had misled biologists into hypotheses which could not now be justly adduced to support such estimates when physical geology declares against them. I am glad to find this view supported by the high authority of Professor Huxley himself, who says, "But it "may be said that it is biology and not geology which asks for so "much time—that the succession of life demands vast intervals; "but this appears to me to be reasoning in a circle. Biology "takes her time from geology. The only reason we have for "believing in the slow rate of the change in living forms is the "fact that they persist through a series of deposits which geology "informs us have taken a long while to make. If the geological "clock is wrong, all the naturalist will have to do is to modify his "notions of the rapidity of change accordingly." But I may be permitted to remark that a correction of this kind cannot be said to be unimportant in reference to biological speculation. The limitation of geological periods, imposed by physical science, cannot, of course, disprove the hypothesis of transmutation of species; but it does seem sufficient to disprove the doctrine that transmutation has taken place through "descent with modification by natural selection."

14. And now as to Prof. Huxley's examination of my arguments. (I.) Referring to my estimate of the retardation of the earth's rotational velocity due to an imagined melting of ice from the polar regions, he remarks that a certain accumulation of polar ice since the miocene epoch, and not more than he imagines may in reality have taken place, would produce five times as much acceleration, as the amount of the retardation which we have estimated from

[1] Edition, 1859; page 282.

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the tides; and he supposes that this would "leave $\frac{4}{5}$ of a second "per annum in the way of acceleration." But *the observed result is retardation*, and Professor Huxley's hypothesis as to ice, if it were valid, would therefore prove retardation by the tides six times as much as that which we have ventured to estimate! I am much obliged to him for this suggestion, and also to Mr. Croll for a suggestion which he has made to me that the erosion of equatorial mountains and deposition of detached matter at considerable distances from the equator, in either north or south latitude, may be exerting, at the present time, an accelerating influence of a sensible amount upon the earth's rotational velocity, and rendering the observed retardation less than that due to the tides. For, as shewn in my lecture on Geological Time (§ 12 and Appendix), the dynamical theory of the tides, and known facts regarding the interval between "full and change of the moon," and the times of spring tides, render it difficult to see how tidal retardation of the earth's rotation can be so little as to make the integral of lost time in a century amount to only twenty-two seconds. It is probable that something of this accumulation of ice suggested by Professor Huxley, or erosion of matter from the equator suggested by Mr. Croll, may, to a considerable extent, have counteracted the tidal retardation.

15. Now Professor Huxley asks, "If tidal retardation can be "thus checked and overthrown by other[1] temporary conditions, what "becomes of the confident assertion, based upon the assumed uni- "formity of tidal retardation, that ten thousand million years ago "the earth must have been rotating more than twice as fast as at "present, and, therefore, that we geologists are 'in direct opposition "'to the principles of natural philosophy' if we spread geological "history over that time." I answer that tidal retardation cannot be permanently overthrown by temporary conditions; that its true amount may be considerably greater than that which we have estimated from the theory of the moon's motion; and that from million of years to million of years it must always be a positive retardation: whereas the integral effect of the others in millions of years must be zero. Professor Huxley's remarks, instead of making my assertion less worthy of confidence, give us a probability that we may repeat it with equal confidence *for a smaller limit than ten thousand million years*, when in the course of a few years

[1] I presume the presence of the word "other" here is to be regarded as an undetected "erratum."

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the committee of the British Association on tides gives us, for all seas, more knowledge of the times of spring tides relatively to the changes of the moon; of the times of daily high water relatively to the moon's transits; and of the amount of rise and fall, than we have at present.

16. But since Professor Huxley has raised the definite question —What interchange of water and ice would keep the rotation of the earth constant from the miocene period? I must point out that it can be answered only when we know how many centuries have elapsed, supposing we assume (as he does with me, for the sake of argument,) a uniform datum of tidal retardation; and must remark that he has omitted to multiply his estimated thickness of ice by this unknown number of centuries. The subject is certainly somewhat perplexing, owing to the ambiguity of the words commonly used in expressing such matters; of which we have a familiar instance in the statement, "clock too "fast," or "clock too slow," meaning *clock before*, or *clock behind*. Our estimate of tidal retardation is such as to make the earth, regarded as a clock, come to be twenty-two seconds of time behind at the end of the century,[1] after just beginning at the beginning of the century to go slow, and going gradually slower and slower, at a uniform rate of retardation during the century. Thus to get behind by twenty-two seconds at the end of the century implies going slower by ·22 of a second per annum at the middle of the century and ·44 of a second per annum at the end, than at the beginning of the century. This, therefore, gives a retardation of ·44 of a second per annum per century, or of ·0044 of a second per annum per annum; an effect equal in amount to what would be produced by the melting of ·044 of a foot of ice per annum from ice caps of twenty degrees round each pole. Thus to produce an amount of retardation equal to that which we estimate as due to the tides, ice must melt at the rate of ·044 of a foot per annum, or 4·4 feet per century from the polar ice caps.[2] But if the actual retardation were not due to the tides, its amount would be ten instead of twenty-two, by observation and dynamical theory

[1] 25^2 times 22^s, that is, 13750^s, or $3^h\ 49^m\ 10^s$, at the end of 25 centuries.

[2] The attraction of the polar ice upon the ocean referred to by M. Adhemar and Mr. Croll, was not taken into account in my calculations in the "Rede Lecture" of 1866, from which these figures are quoted. Its effect is to render a somewhat less thickness of ice, but greater depression of water in the equatorial regions, necessary to produce the same increase of rotational velocity.

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of the moon's motion. Two feet of ice per century, therefore, melted from the supposed polar ice caps would be required to account for it by the melting of ice, or fifty feet in the twenty-five centuries during which it has taken place. If, then, Professor Huxley can show that it is probable that ice to any such extent as *that* has melted from polar regions, giving a gradual rise of the average level of the sea to the extent of three feet, in the last twenty-five centuries, he would establish the probability of another solution than tidal retardation, to the astronomical question put before us by Adams. But the very fact that dynamical theory of the tides leads me to look for rather a greater than a less amount of retardation than the twenty-two seconds which we have estimated, makes it probable that no such considerable rising of the sea level, if any rising at all, will be found to have taken place. On this question we may, however, fairly look for some positive evidence from the investigations of geologists and archæologists combined.

17. My expectations from tidal dynamics now weigh with me very decidedly against M. Dufour's meteoric hypothesis;—much more than they did at the time I first referred to it in the Rede Lecture of 1866. And although the establishment of this hypothesis would be almost as fatal as the retardation by tides to the uniformitarian geologists, I cannot view the solution of the question with indifference. I look forward with much interest to see it tested by chemical analysis of the dust which has accumulated over Egyptian, Greek, and Assyrian monuments for the last two or three thousand years.

18. (II.) The only answer which Professor Huxley gives to my argument from the sun's heat is, that as lately as fifteen years ago I "entertained a totally different view of the origin of the sun's "heat, and believed that the energy radiated from year to year "was supplied from year to year, a doctrine which would have "suited Hutton perfectly." So far from this being the case, if Professor Huxley will "Hansardize" me by looking to my original paper on "The Mechanical Energies of the Solar System," he will see that my contribution to the meteoric theory of solar heat was to prove the insufficiency of any chemical theory, and to point out that *meteoric supply cannot be perennial in even approximate uniformity with the existing order of things.*[1] I think he

[1] Farther information on this point is to be found in an extract from the Proceedings of the Glasgow Philosophical Society, March 24, 1859, appended (Part III., below).

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will find nothing in that paper which "justly entitles" him to "disregard" my present estimates, but, on the contrary, much to enforce them. In a note to that paper, dated May 4th, 1854, is to be found an indication of my subsequent correction of the untenable part of my first views, and, obstructing it, a difficulty which I then felt as to the sun's capacity for heat. In my article on the Age of the Sun's Heat,[1] to which Professor Huxley refers, a resolution of that difficulty is pointed out, according to which it is shown that the sun's capacity for heat is probably more than ten times, and less than 10,000 times that of an equal mass of water under ordinary pressure. A British jury could not, I think, be easily persuaded to disregard my present estimate by being told that I have learned something in fifteen years.

19. (III.) Referring to my third line of argument founded on a consideration of terrestrial temperature, Professor Huxley asks the question, "But is the earth nothing but a cooling mass, 'like a "'hot-water jar, such as is used in carriages,' or 'a globe of sand-"'stone,' and has its cooling been uniform?" and says, "An "affirmative answer to both these questions seems to be necessary "to the validity of the calculations on which Sir W. Thomson lays "so much stress." I reply that I have carefully considered the first question, and referred to it in my paper on the Secular Cooling of the Earth § 9,[2] or Thomson and Tait's Natural Philosophy, Appendix D, § i.; and that the main purport of that paper constitutes a *negative answer to the second question.* I have distinguished the results calculated from conduction at only the present rate, giving a limit of twenty or thirty thousand million years, in a short article of more recent date entitled, "The "Doctrine of Uniformity in Geology Briefly Refuted," from those of the analytical investigation of the "antecedents" of the present condition of underground heat, contained in my former paper ("Secular Cooling"). The analytical investigation shows the law of the *greater* rate of conduction outwards in past times, and demonstrates a much closer limit for the whole time during which the earth has been solid and continuously cool enough at its surface to be habitable without break of continuity to life, than can be estimated without taking into account the *deviation from uniformity* which I assert.

[1] Macmillan's Magazine, March, 1862.

[2] Secular Cooling, § 18; Transactions of the Royal Society of Edinburgh, 1862; Phil. Mag., 1862; or, Thomson and Tait, Appendix D, § 5.

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20. Referring partly to my views and partly to his own inadvertent misstatement of them, Professor Huxley continues:—

"Nevertheless it may be urged that such affirmative answers "are purely hypothetical, and that other suppositions have an "equal right to consideration. For example, is it not possible "that the prodigious[1] temperature which would seem to exist "at 100 miles below the surface, all the metallic bases may behave "as mercury does at a red heat, when it refuses to combine with "oxygen; while, nearer the surface, and therefore at a lower tem-"perature, they may enter into combination (as mercury does "with oxygen a few degrees below its boiling point) and so give "rise to a heat which is totally distinct[2] from that which they "possess as cooling bodies? And has it not also been proved by "recent researches that the quality of the atmosphere may "immensely affect its permeability to heat, and consequently pro-"foundly modify the rate of cooling of the globe as a whole?

"I do not think it can be denied that such conditions may exist, "and may so greatly affect the supply and the loss of terrestrial "heat as to destroy the value of any calculations which leave "them out of sight."

I reply that I admit the first, and emphatically deny the second, proposition of the last sentence. Heat of combination of elements, present together in a mixed mass and devoid of chemical affinity at a high temperature, but acquiring chemical affinity and consequently combining as the temperature sinks, constitutes merely an addition to the sum of the thermal capacities of the several elements separately reckoned, to give the effective thermal capacity of the composite mass. And the value of "calculations" which leave this possibility "out of sight" is not "destroyed" though an altered figure in the result might be necessitated by an altered estimate of specific heat. But in my calculations I have left a wide enough margin to give due weight on Professor Huxley's side to the smallness of our knowledge regarding specific heats, thermal conductivities, and temperatures of fusion, of the earth's material. And as to the cloudiness or clearness of the atmosphere, I say that the secular cooling of the earth is not affected by it. The

[1] Does this imply internal fluidity? If so, it is to be rejected. "Prodigious" seems too strong a word for any temperature below the melting point of the material.

[2] By no means so: but, on the contrary, an essential part of the heat emitted by the composite mass in cooling.

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one question relevant to atmospheric effect on the secular cooling of the earth is, what has been the resulting temperature of the upper surface of land and sea? My calculations depend only on the assumption that through geological history this temperature has been suitable for such life as now exists on the earth.

21. Criticising the calculations I had adduced regarding the earth's rotation, Professor Huxley makes the following remarks, which have equal bearing upon those regarding the sun's heat and light and the earth's interior temperature: "I desire to point out "that this seems to be one of the many cases in which the admit- "ted accuracy of mathematical processes is allowed to throw a "wholly inadmissible appearance of authority over the results "obtained by them. Mathematics may be compared to a mill of "exquisite workmanship which grinds you stuff of any degree of "fineness; but, nevertheless, what you get out depends on what "you put in; and as the grandest mill in the world will not "extract wheat flour from peascods, so pages of formulae will not "get a definite result out of loose data." To the second of these sentences I assent, but certainly not to the first. I have not presented definite results; I have amply indicated how "loose" my data are; and I have taken care to make my results looser. Professor Huxley himself in *other* parts of his address has *complained of their vagueness* "as greatly embarassing the discussion." If I had presumed to limit the past duration of life on the earth to one million years, or to ten million years, by calculations, founded on such data as I have used, so ill drawn an inference could scarcely "embarass" those who are still disposed to trust to "a practically "unlimited bank of time ready to discount any amount of hypo- "thetical paper." But it is obvious that they must be seriously embarassed by even a superior limit of four hundred million years: especially when the declaration of it is coupled with the assertion of a *very strong probability* that "all geological history showing "continuity of life," is in reality to be condensed into a period not exceeding *one* hundred million years.

22. Before concluding, I may be permitted to make a few remarks on the practical bearing of the limitations which I have adduced upon some points of geological theory, which, when the boundary between mineralogy and geology is once passed, cannot be evaded even by those most averse to speculation.

23. Fourier's theory of the conduction of heat renders it almost

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impossible to escape the conclusion, that if the earth has been solid and habitable continuously during the last 50 million years, its rate of increase of underground temperature per metre downwards must have been very sensibly more rapid 50 million years ago than now. The more recently discovered laws of thermodynamics render it certain that the sun must have been something very different 50 million years ago from what he is now; and almost certain that he must have been then very much hotter. And we find Sir Roderick Murchison[1] writing as follows, on purely stratigraphical grounds:—"I could here cite the works of many eminent writers "for numerous evidences of the grander intensity of causation in "former epochs, by which gigantic stratified masses were some- "times inverted, or so wrenched, broken, and twisted, as to pass "under the very rocks out of which they were formed. Among "those who have passed away I may mention de Saussure, Von "Buch, Humboldt, Cuvier, Brongniart, Buckland, Conybeare, "De la Beche, and W. Hopkins. Of those who hold the same "views, and are now living, I may enumerate Elie de Beaumont, "D'Archiac, De Verneuil, Studer, Sedgwick, J. Forbes, Phillips, "Dana, Logan, and many others. The traveller amid the Alps, "and other mountain chains will there see clear and unmistakable "signs of such former catastrophes, each of which resulted from "fractures utterly inexplicable by reference to any of those puny "oscillations of the earth, which can be appealed to during historical "times." * * * * "Again,[2] I see in existing nature no "cause of sufficient intensity to account for ordinary sediments "(once charged with organic remains) having been changed into "crystalline masses occupying whole regions. The theorist in "vain endeavours to explain such operations by processes so slow "in their action, as to be almost imperceptible. If it be argued "that the strata constituting lofty mountains were metamorphosed "in parts by such a slow process, let any one who sustains that "view explain how it is that every stratum in a lofty range of "mountains, composed of carbonate of lime, should, in some cases, "all at once change into sulphate of lime, and in others into "dolomite."

24. Sir Charles Lyell himself admits a warmer climate in the

[1] "Siluria." 1867 Edition, page 489.

[2] Ibid. Page 495.

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earliest geological periods. Thus considering "a general[1] refri-"geration of climate;" (from the more ancient times understood) "and several oscillations of temperature during the glacial epoch;" to be proved by palæontological evidence; he endeavours to explain those past changes chiefly if not solely by hypothetical alterations in the distribution of land and sea over the globe. Every reader of the "Principles of Geology" must admire the ingenuity, and admit the importance, of the chapter in which this hypothesis is set forth. But I earnestly beg Professor Huxley, and those in whose name he speaks, to reconsider their opinion, (§ 4 above) that the secular cooling of the earth and of the sun "has made no practical "difference to the earth during the period of which a record is "preserved in stratified deposits." There is, surely, good ground for Sir Roderick Murchison's opinion that metamorphic causes have been more active in ancient times than at present, because of more rapid augmentation of temperature downwards below the earth's surface; and it cannot be reasonably urged that a hotter sun is not a probable explanation of the supposed warmer climate of the palæozoic ages.

25. The "grave charge of opposition to the principles of Natural "Philosophy," which Professor Huxley so earnestly repudiates, was carefully limited by the words in which I expressed it, to certain clearly specified points; and it was only because of the prominent and fundamental position given to those points in many of our standard works, that I brought that charge against "British "Popular Geology." I have no wish to press the charge, merely for the sake of proving myself to have been in the right at the time I made it; and if it rested solely on the question of geological time, I would willingly avoid repeating it. But in some of the most recent geological writings of the highest character I still find the same tendency to overlook essential principles of thermo-dynamics, as that to which I called the attention[2] of the geological section of the British Association, at Manchester in 1861.

26. In the last edition of "The Principles of Geology," 1868, vol. 2, page 242, we find the following statement:—"The existence "of electrical currents in the earth's crust, and the changes in "direction which they may undergo after great geological revolu-

[1] Principles of Geology. Vol. I., page 387. 1867 edition.

[2] In a communication published afterwards, under the title, "Secular Cooling "of the Earth," in the Transactions of the Royal Society of Edinburgh, 1862, and in Thomson & Tait's Natural Philosophy, Appendix D (1867).

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"tions in the position of mountain chains, and of land and sea, the "connection also of solar and terrestrial magnetism, and of this "last with electricity and chemical action, may help us to conceive "*such a cycle of change as may restore to the planet the heat supposed* "*to be lost by radiation into space.*" And again, at page 213— "It is a favourite dogma of some physicists, that not only the "earth, but the sun itself, is continually losing a portion of its "heat, and that, as there is no known source by which it can be "restored, we can foresee the time when all life will cease to exist "upon this planet; and, on the other hand, we can look back to the "period when the heat was so intense as to be incompatible with "the existence of any organic beings such as are known to us in "the living or fossil world."

"When we consider the discoveries recently made, of the con- "vertibility of one kind of force into another, and how light, heat, "magnetism, electricity, and chemical affinity are intimately "connected, we may well hesitate before we accept this theory of "the constant diminution from age to age of a great source of "dynamical and vital power." These statements are directly opposed to the general principle of the dissipation of energy: and the hypothesis which they suggest is very inconsistent with our special knowledge of the conduction and radiation of heat, of thermo-electric currents, of chemical action, and of physical astronomy.

Kant's hypothesis of the restoration of a new chaos, like the old one, with potential energy for a repetition of cosmogony, described by Prof. Huxley, was not a more violent contravention of thermo-dynamic law; but the synthesis of its fallaciousness is more obvious.

27. Professor Huxley's own statement as to catastrophism and uniformitarianism is open to the objection of violating the principle of the conservation of energy. "Catastrophism has "insisted upon the existence of a practically unlimited bank of "force, on which the theorist might draw; and it has cherished "the idea of the development of the earth from a state in which "its form, and the forces which it exerted, were very different from "those we now know." * * * *

"Uniformitarianism, on the other hand, has, with equal justice, "insisted upon a practically unlimited bank of time, ready to dis- "count any quantity of hypothetical paper."

In the Catastrophism of Leibnitz, Newton, Sedgwick, Phillips,

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Hopkins, Forbes, Murchison, and many other true geologists, which is in no respect different as a geological doctrine from that now described by Professor Huxley under the new name "evolutionism," there has been no "unlimited bank of force." And it is because the whole amount of energy existing in the earth has always been essentially finite, that physical science supports their theory, and rejects, as radically opposed to the principles of natural philosophy, the uniformitarianism described by Professor Huxley in the passage just quoted.

28. Professor Huxley concludes thus: "My functions, as your "advocate, are at an end. I speak with more than the sincerity of "a mere advocate when I express the belief that the case against "us has entirely broken down. The cry for reform, which has been "raised from without, is superfluous, inasmuch as we have long been "reforming from within with all needful speed; and the critical "examination of the grounds upon which the very grave charge of "opposition to the principles of Natural Philosophy has been "brought against us, rather shows that we have exercised a wise "discrimination in declining to meddle with our foundations at the "bidding of the first passer-by, who fancies our house is not so well "built as it might be."

The quotations which I have given above prove that my call for reform was very far indeed from being superfluous, and that what Professor Huxley describes as a "reforming from within," has been for the last ten or fifteen years in the wrong direction, so far as the estimation of geological time is concerned: and they bear out my statement, that modern British popular geology, "as "represented by a very large, very influential, and in many respects "philosophical and sound body of geological investigators, consti-"tuting perhaps a majority of British Geologists," is, on some very important points, in "direct opposition" to the principles of Natural Philosophy, and of Physical Astronomy.

29. I cannot pass from Professor Huxley's last sentence without asking, Who are the occupants of "our house," and who is the "passer-by?" Is geology not a branch of physical science? Are investigations, experimental and mathematical, of underground temperature, not to be regarded as an integral part of geology? Are suggestions from astronomy and thermo-dynamics, when adverse to a tendency in geological speculation recently become extensively popular in England through the brilliancy and elo-

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quence of its chief promoters, to be treated by geologists as an invitation to meddle with their foundations, which a "wise discrimination" declines? For myself, I am anxious to be regarded by geologists, not as a mere passer-by, but as one constantly interested in their grand subject, and anxious, in any way, however slight, to assist them in their search for truth.

Part II.—On the Origin and Total Amount of Plutonic Energy.

30. By Plutonic action, I mean any disturbance of underground equilibrium. Volcanoes, earthquakes, and subsidences are the phenomena most commonly understood when plutonic activity is spoken of. The store of energy to which these phenomena are due is properly called plutonic energy, and according to the clear and simple, but thoroughly rigourous, language of modern dynamics, plutonic energy is to be distinguished from plutonic activity.

31. The *action* of a dynamical agent was defined by Newton, as something to be measured numerically, by the number measuring simple force or pressure, multiplied into the number measuring the velocity with which the matter experiencing it yields in the direction of the force. In the nineteenth century dynamical vocabulary, Newton's "action of an agent" is simply a *performing of work*, and we distinguish between action, or rate of action, as defined by Newton, and the integral amount of action or integral amount of work done after any operation of force is completed. Again, in modern physical dynamics we have learned that every performance of work consists in merely a transformation or intertransposition of materials, or a stopping of some motion and generating of other instead, and that when work is performed in one locality, another locality must on that account be left with so much less of the wherewithal for the farther performance of work. This "wherewithal" is called energy; and thus the performance of work is simply the drawing of energy from one store and laying it out elsewhere. Any irreversible transformation of energy is called a dissipation of energy; of which the most prominent examples are the conduction of heat from warmer to colder parts of a body, or of the matter occupying any portion of space, and the generation of heat by friction or collision.

32. Plutonic action is, therefore, to be defined as any transformation of energy going on within the earth. No natural operation

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is thoroughly reversible, and therefore, every plutonic action involves something in the way of dissipation of energy. But the grand and awful phenomena of volcanoes and earthquakes, results of abnormal plutonic activity, give rise probably to much less dissipation of energy, summed for all parts of the earth from age to age, than the continual silent action of the conduction of heat outwards, the amount of which we are able to estimate in a thoroughly definite manner. Thus we find that from year to year the earth, at the present time, is parting with heat at the rate of 92 horse-power[1] per square kilometre.[2] That is to say, from a square metre of surface the loss of energy is at an average rate of seven metre-tons per million seconds, or 220 metre-tons per annum. The whole area of the earth is 510,000,000 square kilometres; and therefore the loss from the whole earth is 3600 millions of metre-tons per second, or 112×10^{15} metre-tons per annum. This statement is not hypothetical in any respect. But the numerical data assumed in it, being ·005 gramme-water-units per centimetre per second for conductivity, and 1^0 cent. per 30 metres for the rate of increase of underground temperature downwards, are what Professor Huxley would justly call loose, because we do not know the true average conductivity of the upper strata for the whole earth, nor the true average value of the rate of augmentation of temperature per metre downwards; and a very large margin of probable error must be allowed for any estimate that can yet be made of the true rate at which energy is being lost from the earth. This, however, does not at all affect the principles in illustration of which I adduce the numbers, or the importance of these principles for the success of geology as a science.

33. The store of energy, transformations of which constitute plutonic action, consists certainly at the present time in a great measure, if not altogether of terrestrial heat. This indeed is the only description of energy *proved* to exist in any considerable quantity within the earth; but it is possible that there may be great

[1] "One horse-power" is a rate of performing work equal to (33000 foot pounds, or) 4·563 metre-tons per minute; the French ton of 1000 kilogrammes understood, being ·9842 of the British ton.

[2] The kilometre is ·62138 of that very inconvenient measure, the British statute mile. The square kilometre is 247·11 of that, if possible worse measure, the acre. Experts can tell how many square yards are in an acre; but of all the men in England accustomed to reckon their land in acres, and to state, or read, or hear reckonings of political statistics in square miles, very few could readily answer the question, how many acres are there in a square mile?

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masses of uncombined chemical elements, and that the potential energy of their mutual affinities may constitute a considerable portion of the plutonic energy in store, whether for the generation of future underground heat, or for immediate application to some of the more violent manifestations of plutonic activity. Now, there are two ways of estimating the possible total amount of plutonic energy; one by taking the earth as it is, and not reasoning from antecedent conditions, but simply estimating from known properties of matter; how much heat it is conceivable may exist in it in its present condition; the other by tracing the history of the earth backwards.

34. From experiments such as have not yet been made, but could be made with very great ease, on the total heats of fusion of ordinary rocks and metals,[1] we shall probably soon be able to estimate, without any very unsatisfactory degree of vagueness, a limit to the possible amount of heat in the earth. With a view to putting together the data required for this estimate, it is important to notice that we have strong reason to believe the earth is not a mere thin shell filled with melted material of rock or metal, or both, as many French and a few English geologists assume it to be; but is solid from surface to centre with the exception of comparatively small spaces still occupied by fluid lava, or subjected occasionally to melting in volcanic action.[2] We may therefore say it is not at all probable that there is now within the earth a hundred times as much heat as that which would raise a quantity of average surface rock equal in mass to the whole earth, from zero to 200° cent., since this would be certainly many times more than enough to melt that amount of any kind of surface rock under any moderate pressure. But merely from consideration of thermal capacities, and possible temperatures of the earth at great depths, we are not at present able to make any much less vague estimate than that, of the possible total amount of heat.

35. Inasmuch as energy is being continually lost from the earth by conduction through the upper strata, the whole quantity of plutonic energy must have been greater in past times than at present, and the question forces itself upon us, how was it first

[1] A very simple plan would be to pour small quantities of melted rock into hollows in blocks of cast iron, massive enough not to rise more than a few degrees of temperature by the communication of heat from the melted rocks.

[2] "Rigidity of the earth," (W. Thomson) Trans. R. S., 1862; and Thomson & Tait's Natural Philosophy, §§ 832-849.

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acquired? As the earth, being finite, cannot ever have had an infinite store of energy within it, there must have been a time when it was not a warm body, parting with energy, as it is now. If the matter of the earth existed before that time, it must have been under conditions which led to its being warm, and to its commencing to part with energy. It may have gained its heat by communication from other matter, or by work performed upon it by matter not now forming part of itself. But the only probable hypothesis is, that it has become warm by the conversion of mutual potential energy, whether of gravitational, or gravitational and chemical, attraction between its parts, into heat.

36. It may be said, why not admit previous kinetic energy without limit, as we have no reason to believe that the antecedent condition of the matter now constituting the earth was a condition of rest rather than a condition of motion? I answer that we know nothing of absolute motion or rest in the universe, and that any great degree of *relative* motion of different portions of matter through space, renders the chances of their hitting one another very small. I therefore say it is not probable that the portions of matter now constituting the earth had in their antecedent condition any great amount of relative motion; and it is probable that the kinetic energy which was converted into heat in their coalition was the equivalent of kinetic energy acquired by mutual gravitation. It seems, indeed, that Kant's "attempt to "account for the constitution and mechanical origin of the "universe, on Newtonian principles," only wanted the knowledge of thermo-dynamics, which the subsequent experiments of Davy, Rumford, and Joule supplied, to lead to a thoroughly definite explanation of all that is known regarding the present actions and temperatures of the earth, and of the sun, and other heavenly bodies. And if Carnot's theory had been before him, he assuredly would not have forestalled Hutton in the chimera of "a reproduc- "tive operation, by which a ruined constitution may be repaired."[1]

37. Now the whole amount of potential energy exhausted in the coming together of the earth's materials, from infinite mutual distances (that is to say, from distances many times greater than the present diameter of the earth) to their present relative positions, is easily estimated with great accuracy with the knowledge we possess

[1] See the account of Kant's Cosmogony given by Professor Huxley, in his "Address" of *Feb.* 19, 1869, to the London Geological Society.

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of the earth's average density. If the density were uniform from surface to centre, the amount of potential energy in question would be equal to the work required to lift a body equal to $\frac{3}{5}$ of the earth's mass from the present surface to an infinite distance. But observation proves the mean density of the earth to be 5·5, which is about twice the average surface density; and if we use Laplace's probable law of interior density,[1] we find more exhaustion of energy in coalition, by about 10 per cent., than if the density were uniform, the result for the whole being, as nearly as may be, a mass equal to $\frac{2}{3}$ of the earth's, raised from the surface to an infinite distance. This second estimate we may adopt with great confidence, as probably very close to the truth, considering how little it differs from the first. Now, the work required to lift a mass from the earth's surface to an infinite distance, against the diminishing force of gravity, is the same as that which would be required to lift an equal mass through a space equal to the earth's radius, against a force everywhere equal to the actual force of gravity at the surface. Hence, as the earth's radius is 6370 kilometres, the whole amount of potential energy exhausted in the coalition of its parts amounts to $\frac{2}{3} \times 6370000$ or 4250000 metre-tons per ton of its whole mass: the metre-ton (an ordinary gravitation unit of work) being the amount of work required to overcome, through a space of one metre, a force equal to the weight of a ton at the earth's surface; the difference of the force of gravity at different parts of the earth's surface neglected. But unless, which is very improbable, the conglomeration took place quite suddenly by the simultaneous collision of materials falling in from all sides, a large part of this energy must have been dissipated away by radiation of heat consequent on partial collisions. We must therefore look on the definite estimate 4250000 metre-tons per ton of the earth's mass, which expresses somewhat accurately the whole potential energy exhausted during the conglomeration, as being considerably above the greatest amount of plutonic energy due to gravitation, that can ever have existed in the earth at any one time.

38. To estimate the potential energy of chemical affinity already exhausted, or yet to be exhausted, by the combination of the materials constituting the earth, we may remark first, that the upper crust consists chiefly of metallic oxides, but contains also a large quantity of carbonic acid and water. Now we have the

[1] Thomson and Tait. § 824.

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following results, from two very accurate observers, regarding heat of combination—reduced so as to show the amount of heat generated per unit mass of the compound substance formed:—

HEAT OF COMBINATION OF VARIOUS ELEMENTS WITH OXYGEN.

Substance.	Product.	Quantity of Substance.	Quantity of Oxygen.	Units of Heat Evolved.	Observer.
Potassium,	K O	$\frac{39}{55}$	$\frac{16}{55}$	1682	Joule.
Iron, - -	Fe_3O_4	$\frac{21}{29}$	$\frac{8}{29}$	1141	Andrews.
Carbon, -	C O^2	$\frac{3}{11}$	$\frac{8}{11}$	2155	Do.
Hydrogen,	H_2 O	$\frac{1}{9}$	$\frac{8}{9}$	3756	Do.
Zinc, - -	Zn O	$\frac{65\cdot2}{81\cdot2}$	$\frac{16}{81\cdot2}$	1045	Do.
Tin, - -	Sn O_2	$\frac{118}{150}$	$\frac{32}{150}$	969	Do.
Copper, -	Cu O	$\frac{127}{159}$	$\frac{32}{159}$	481	Do.

These numbers make it, I think, very certain that the heat of combination per ton of the average materials of the earth would be over-estimated at 3000 units centigrade—that is, 3000 times the quantity of heat required to raise the temperature of a ton of water by 1° cent., or, according to Joule's equivalent, 1,270,000 metre-tons of energy.

39. The number 4,250,000 previously found (§ 37) for the amount of potential energy of gravitation exhausted in the coali tion of the earth's mass, is $3\frac{1}{3}$ times this estimate of the potential energy of the chemical affinity of its elements. The whole amount of energy due to the two causes together is about $5\frac{1}{2}$ million metre-tons, or 13,000 thermal units centigrade, per ton of the earth's mass. This, being about 700 times as much heat as would raise the temperature of an equal mass of surface rock from 0° to 100° cent., is three and a-half times the amount stated in § 34, as an over-estimate of the whole amount of heat at present in the earth. But considering, as in § 37, how much heat must have been dissipated during the conglomeration of the materials which now constitute the earth, we are rather compelled to contract than permitted to enlarge our ideas of the possible total of plutonic energy at present in the earth, by tracing its history backwards to its probable origin.

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PART III.—NOTE ON THE METEORIC THEORY OF THE SUN'S HEAT.

(From Report of Meeting of the Glasgow Philosophical Society's Meeting of March 24, 1869.)

40. Sir WM. THOMSON, in reply to a question from the President, Dr. Bryce, said that his contribution to the meteoric theory of solar heat had been to point out that the meteoric supply could not be perennial. In his paper "On the Mechanical Energies of the Solar System" (Transactions of the Royal Society of Edinburgh, April, 1854,) he had shown that meteors falling from extra-planetary space in sufficient abundance to generate the heat emitted from the sun for the last 2000 years, must, by the augmentation they must have brought to the central mass, have caused a gradual shortening of the year of which the accumulated effect during that period must have dislocated the seasons to the extent of a month and a half. But observation proves that there has been a dislocation of the seasons only to the extent of about an hour and three-quarters, since a certain eclipse of the moon was seen on March 19th, 721 B.C., in Babylon. It is quite certain, therefore, that meteoric supply for sun heat has not within historical periods come from distant space outside the earth's orbit. He therefore found it necessary to modify the meteoric hypothesis of sun heat—a hypothesis which he had learned from a communication by Mr. Waterston to the British Association at Hull in 1853, but which he has since found had been previously proposed by Mayer. If it is true that the heat emitted by the sun is compensated from year to year by meteors, he proved that instead of a certain quantity of meteors falling in a certain time from distant extra-planetary space, as supposed by Mayer and Waterston, a double quantity in the same time must fall from orbits inside that of Mercury. But at the same time he pointed out that observation and dynamical theory of the motions of the planets must be had recourse to, to test whether or not there can be a sufficient amount of matter circulating as meteors inside the orbit of Mercury to provide sun-heat for a few hundred years to come. Since that time Leverrier's fine researches on the motions of the planet Mercury give evidence of matter circulating as a great number of small planets within his orbit round the sun. But the amount of matter thus indicated is very small, probably not enough for a few hundred years' heat. It is therefore highly improbable that the heat of the sun depends at all for its continuation upon a continued meteoric supply. In the present state of

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science what appears most probable is Helmholtz's view, that the sun originally acquired his heat in being built up out of smaller masses falling together and generating heat by their collision, but that at present he is simply an incandescent mass cooling. In an article in *Macmillan's Magazine*, March, 1862, "On the Age of the Sun's Heat," he (Sir W. Thomson) had shown that the sun may have been several million future years giving out heat and light from the vast initial supply generated in that manner; but that, without supposing the sun to be a miraculous body, continually violating the laws of matter, we cannot believe that from first to last he could illuminate the earth for several times one hundred million years, if even for so long a period as that. Since he had been asked to explain his views regarding the theory of sun heat, he took the opportunity of adverting to a statement which Professor Huxley had recently made in his inaugural address to the Geological Society of London, to the effect that he (Sir W. Thomson) had only 15 years ago entertained a view of the origin of the sun's heat which would have "suited Hutton perfectly," inasmuch as, according to that view, the energy radiating from year to year is supplied from year to year. But Professor Huxley had not noticed that the very limited supply which could possibly exist in store, according to that view, could not upon any estimate amount to three hundred thousand years' expenditure, at present rate even without taking into account the astronomical observations published since 1854. And, in fact, no view except Hegel's —"the motion of the heavenly bodies is not a being pulled "this way and that, as is imagined (by the Newtonians); they go "along, as the ancients said, like blessed gods,"—could satisfy a "thorough-going Huttonian uniformitarian," or could fulfil the conditions imagined by Lyell as a foundation for a theory of under-ground heat. As to the sun, we can now go both backwards and forwards in his history, upon the principles of Newton and Joule. A large proportion of British popular geologists of the present day have been longer contented than other scientific men, to look upon the sun as Fontenelle's roses looked upon their gardener.[1] "Our gardener," say they, "must be a very old man: "within the memory of roses he is the same as he has always been; "it is impossible he can ever die, or be other than he is."

[1] Kant's "Physische Geographie" (Collected Works, vol. vi., Leipzig 1839.)

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