## Art. IV.—Geological Time.

1. *On the Secular Cooling of the Earth.* By Sir W. Thomson. Trans. R.S.E., 1862, and Phil. Mag., 1863, ii.; Thomson and Tait's Natural Philosophy, vol. i. App. D.
2. *The Uniformitarian Theory of Geology briefly refuted.* By Sir W. Thomson. Proc. R.S.E., 1865.
3. *On Geological Time.* By Sir W. Thomson. Trans. of the Geological Society of Glasgow, 1868.
4. *President's Address to the Geological Society of London, February* 1869. By Professor Huxley.
5. *Of Geological Dynamics.* Part I. *Reply to Professor Huxley's Address to the Geological Society of London.* Part II. *Origin and Total Amount of Plutonic Energy.* Part III. *Note on the Meteoric Theory of the Sun's Heat.* By Sir W. Thomson. Trans. of the Geological Society of Glasgow, 1869.
6. *Mathematics* versus *Geology.* Pall Mall Gazette, May 3, 1869.
7. *The Origin of Species.* North British Review, 1867.
8. *Presidential Address to the British Association at Norwich,* 1868. By Dr. Hooker.
9. *On the Age of the Sun's Heat.* By Sir W. Thomson. Macmillan's Magazine, 1862.

The papers above mentioned have a more or less direct bearing upon what is assuredly one of the most important as well as the most interesting scientific discussions of the present century. Well might it be said, considering not merely the importance of the questions at issue, but also the qualifications of the principal champion on either side—

> " expectation stood
> In horror: from each hand with speed retired,
> Where erst was thickest fight, the angelic throng,
> And left large field, unsafe within the wind
> Of such commotion; such as, to set forth
> Great things by small, if, nature's concord broke,
> Among the constellations war were sprung,
> Two planets rushing from aspect malign
> Of fiercest opposition, in mid-sky
> Should combat, and their jarring spheres confound."

Nothing short of such a classic extract can fitly describe the controversy carried on in the journals above named: the antagonists being undoubtedly each the foremost man in Britain in his subject—Sir William Thomson in Applied Mathematics and Natural Philosophy, Professor Huxley in Physiology and

Natural History. Why and about what should such authorities differ? If they *have* anywhere common ground, can their methods give inconsistent results? Is not truth single? These and like questions rise before us with breath-taking rapidity. The answer is unfortunately but too easily given; we find it proclaimed without any attempt at disguise in the (spoken) words of a great living geologist. (We quote from memory, but believe we express the exact *sense* of his remark.) "I should certainly not accept any mathematical result connected with Geology if it were inconsistent with the results obtained by *our* mode of treating *our* subject. I would not accept a thousand, or even a hundred thousand, millions of years, or any limit whatever imposed by physical science. I am just as incompetent to judge of the evidence on which *you* go as you are to judge of *ours*." Which is equivalent to telling mathematicians and natural philosophers, in common slang, to "mind their own business, and let other folk be."

Something there is, in this, very much resembling those most objectionable theories and practices of the Trades-Unionists which have recently been held up to public execration. The unfortunate "knobstick" is, relatively to his delicacy of feeling, which requires at least a brickbat to make any impression upon it, not treated worse than the mathematician who presumes to undertake a part, however small, of the work arrogated to himself by a non-mathematical savant. We may compare it also to the senseless outcry against machinery which has disgraced almost every age of the world. That educated scientific men should thus fall into the wretched fallacies of handloom-weavers, boot-closers, and (*pudet dicere*) even of Irish reapers, is surely a very singular psychological phenomenon, worthy the attention of sensational writers on obscure diseases of the mind and brain. Even Professor Huxley says, in his Address (above mentioned, the capitals are ours),—"We have exercised a wise discrimination in declining to meddle with our foundations at the bidding of the first PASSER-BY who fancies that our house is not so well built as it might be:"—which looks like an unintentional parody of one of Victor Hugo's latest ironical queries, "Où en serait-on si le premier venu avait des droits?" In Sir W. Thomson's "*Reply*," this boast of Professor Huxley is met in the mildest and meekest spirit:—calculated, we think, as Geologists at present are, merely to produce fresh and more uncalled-for attacks upon him. For the *moment*, we fear he weakens, not his cause but, his chance of a hearing by not sufficiently showing his teeth:—

"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 eloquence of its chief promoters, to be treated by geologists an 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."

In connexion with Professor Huxley's metaphor, Dr. Hooker's remark about Lyell may be read with profit. The contrast is at least curious :—

"Well may he be proud of a superstructure raised on the foundations of an insecure doctrine when he finds that he can underpin it, substitute a new foundation, and after all is finished, survey his edifice, not only more secure, but more harmonious in its proportions than it was before."

This of course means that a *Geologist* is perfectly at liberty to retain his "superstructure" while entirely altering his foundations; but we shall see presently that even Dr. Hooker (who has allowed this much) is quite as indignant at the *Mathematician* who proffers assistance, as any geologist can be.

This sort of thing won't do in Science, and the sooner scientific men of every species recognise the fact the better. Although there is often something almost ludicrous and contemptible about the *mere* mathematician, whose *ratio existendi* it is difficult to conjecture, yet mathematics are indispensable to the complete development of every real science: and he who discourages their application simply repeats, in perhaps a more telling form, the bigoted blunder of the otherwise great astronomer, who persistently refused the aid of the telescope in his observations, and thus immeasurably diminished the usefulness of his long and important labours. The same foolish bigotry is even now-a-days not uncommon with a certain class of Physiologists and Anatomists, who cling to what they call "real old Anatomy," and look with scorn upon their brethren who avail themselves of the wonderful powers of the microscope.

Every scientific man ought to be, as far as he can, a mathematician: just as every literary man ought to be more or less of a classical scholar. In a certain, usually somewhat pedantic, sense this is the case in Germany and France; but certainly in no sense in Britain, for here few even of our Natural Philosophers, with a mere unit or two among our Chemists, and none of our Physiologists, can lay claim to more than the most

beggarly elements of mathematical knowledge. Such a man as Helmholtz, Physiologist and yet Mathematician and Natural Philosopher (and in the very front rank in all three), would be a *monster* in this country. It is mainly to this that we must ascribe the fact that there is little such hostility between different groups of genuine scientific men abroad as we find everywhere at home; few of those petty rivalries of subjects, which are the disgrace of all science. In saying this we are aware that even abroad the METAPHYSICIANS claim to have a word on every subject, as they have long done in this country; but there, as here, few really scientific men now-a-days pay much attention to them: mere soap-bubbles, they are uninjured by the keen thrust of the scientific rapier, but collapse into a drop of water before the blown bladder of the jester: and they are considerately left to form a sect *per se*, wherein complacent vanity and self-sufficiency are almost as rife as mutual recrimination.

According to Professor Huxley, "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 formulæ will not get a definite result out of loose data."

According to Common Sense (which, though it is not obvious in the preceding extract, Professor Huxley claims to wield as one of his most formidable weapons; and which we are therefore surprised to find taking the field against him), Mathematics cannot pretend to deduce from any data results not therein involved, nor can it pretend to improve observations which are known to have been loosely made, or in which good approximations were unattainable: but it has the special advantage (possessed by no other method) of being able to estimate numerically the *weight* or *value* of every conclusion it furnishes. And no mathematician, worthy of the name, would state, without indicating (as well as his information enabled him) the limits of error, a result derived from "loose data:" much less would he employ "pages of formulæ" for the purpose.

The fact is that, although many scientific men (*in Britain*) may attempt to ignore it, Mathematics is as essential an element of progress in every real science as language itself; but it cannot be usefully introduced until we have arrived at something a little beyond what may be called the mere "beetle-hunting" or "crab-catching" stage. If Professor Huxley is inclined to admit that Geology is still in this very imperfect state, all we can say is that, with Sir W. Thomson, we think otherwise, and so thinking feel that mathematical knowledge

ought to be brought to the aid of men of real merit and genius, who are now hopelessly floundering about for want of it.

It *is* the business of every real mathematician to make, as far as in him lies, useful applications of his grand instrument; if he do not, and yet is active, he too often works, not at improving his instrument (work which would of course be of value) but, at applying it to imaginary and in general ridiculous "Problems" in whose data the facts of physical science are ignored, or at quips and puzzles for the *Lady's and Gentleman's Diary*. Such a fate is worse than oblivion, it is a perennial self-gibbeting.

Let us then hear no more nonsense about the interference of mathematicians in matters with which they have no concern; rather let them be lauded for condescending from their proud pre-eminence to help out of a rut the too ponderous waggon of some scientific brother.

It is only within the last two or three years that a few logicians have been able so far to get over this abominably miscalled *esprit de corps* as to think the late Dr. Boole excusable for having published his magnificent work on *The Laws of Thought*; a work which, look at it from what side we may, is one of the grandest scientific monuments of the present century. But in Geology, as in Logic, Mathematics is now advancing to play the part of Henry VIII., and,

> "We hear the sacrilegious cry,
> 'Down with the nests and the rooks will fly.'"

We should not have associated, at the head of this article, with the acknowledged writings of men of such deserved reputation as Sir W. Thomson and Professor Huxley, the critical remarks of an anonymous journalist (we happen to know well the high qualifications of the former writer in this *Review*), were it not that these remarks have unfortunately obtained far more extensive publicity than the writings they refer to. This person may do considerable mischief by his assuming to speak with authority, and "not as the scribes." Who, where, or what, he is we have not the slightest notion; and we can therefore freely examine his production. It is one of a class which is now-a-days becoming far too common, and which every man of true scientific feeling ought to do his best to discourage, a critique (?) of the most one-sided character, made entirely without knowledge of the merits and defects of either side. Its tone, too, is throughout *studiedly* insolent and offensive to Sir W. Thomson, and such as justly to deprive the writer of all claim to be treated with the courtesy ever due to an honourable opponent. Perhaps, before we have done with it, we may be able to show that its author has in this outburst effected nothing

but a complete and humiliating demonstration of his own ignorance and prejudice. Though a Hercules or a Briareus is usually required for any effective intervention in a war of the gods, we have legendary authority for believing that commoner mortals may occasionally be of some service; but Thersites makes his appearance only to be ignominiously exposed and sent howling to the hulks:—

> *μηκέτ' ἔπειτ' Ὀδυσῆϊ κάρη ὤμοισιν ἐπείη,*
> *μηδ' ἔτι Τηλεμάχοιο πατὴρ κεκλημένος εἴην,*
> *εἰ μὴ ἐγώ σε λαβὼν ἀπὸ μὲν φίλα εἵματα δύσω,*
> *χλαῖνάν τ' ἠδὲ χιτῶνα, τά τ' αἰδῶ ἀμφικαλύπτει,*
> *αὐτὸν δὲ κλαίοντα θοὰς ἐπὶ νῆας ἀφήσω*
> *πεπληγὼς ἀγορῆθεν ἀεικέσσι πληγῇσιν.*

By far the grandest question in Geology proper, though one which Hutton expressly declines to deal with, is that of the original formation and early history of the Earth, for in its answer are included, to a great extent, the present and the future. For our present purpose it is not necessary to consider any of the theories, some of them very plausible, which have of late been propounded as to the origin of Suns and Planets by the falling together of discrete masses originally scattered about in space. What we wish to consider is how far observation of those strata with which alone the geologist can ever be acquainted, assisted by such astronomical and physical information as we can gather from the earth's figure, internal heat, rate of rotation, etc., is fitted to guide us in reckoning back to what must have occurred in earlier ages of the world. Have we any means of forming an opinion as to the state of our globe so much as one, ten, or one hundred, million years ago? Let us first consider what the geologists can fairly attempt by data derived from their own science. If we find their methods at best extremely inadequate to the solution of such grand questions, we must next inquire whether physical science has not other resources at least a little superior to theirs.

Geologists may argue the point from various sides, most easily from the important action of water in modifying the earth's surface. For instance, given the thickness of a bed of stratified rock, whose appearance at once proves that it has been deposited at the bottom of an ocean or of an immense lake, and assuming from what we see going on at present around us the most probable rate at which such deposits are formed, we can at once calculate the most probable requisite time. Or we may consider the disintegrating and wearing effects of water, instead of its constructive effects, and seek how long time has been required for the erosion of portions which we see have been by

its agency removed from a rock or deposit whose character is known from the fragments which remain. Still there is a possible fallacy, for deposition and denudation may have alternated many times during the formation or destruction of such beds of rock. All deductions of these kinds are therefore necessarily of extreme vagueness, and they can at best only supply an exceedingly rough approximation to an *inferior* limit of the time required, leaving the superior limit capable of any magnitude whatever. There are various other conceivable methods, open to the geologist as such, but they all have the same utterly unsatisfactory character, and yield an inferior limit only. Now what is wanted is a *superior* limit, and the veritable origin of the present discussion is the fact that, when methods capable of giving a superior limit of time have been applied, they are found to show that even the inferior limits usually assigned by geologists are of totally inadmissible duration. Such periods were really first introduced by the so-called *Uniformitarian* school of geologists, of whom Hutton and Playfair in former days, and Lyell in the present, may be taken as types. Their ruling notion is that all changes are essentially periodic, and thus that the earth has a sort of normal state, from which it can never differ more than a little, and about which it continually oscillates. To deny this, was, according to Playfair, virtually to assert that "the Author of nature has given laws to the universe, which, like the institutions of men, carry in themselves the elements of their own destruction." The whole passage from which this extract is taken is given by Sir W. Thomson, and he has summarily pointed out its outrageous fallacies. It has been quoted over and over again with approval by Teleologists and authors of Systems of Natural Religion, but it is simply a confusion of two perfectly distinct things, the *permanence of physical laws* (an idea whose correctness we have no reason to doubt), and the *permanence of the present state of things* on the globe (which no one acquainted with modern science can for a moment believe in). A better observer, though not a less pious or less orthodox man than any of the Teleologists, says

"Change and *Decay* in all around I see;"

yet in the eyes of many of the unenlightened a denial of Playfair's assumption is even now little better than atheism. But these gigantic periods, introduced by the Uniformitarians without any physical proof of their admissibility, have been even farther extended by more recent theorists; such as Darwin, for instance, who requires them for his *Development of Species*.

As we have just seen, the ordinary geological methods are quite incapable of setting any superior limit to such periods:

and, before proceeding farther, it may help us a little, as regards the strange revelations presently to come, if we look for a moment at the way in which even a President of the British Association speaks of a branch of science of which there can be no offence in saying he is certainly not a master. The attack is directed against conclusions of physical science, with regard to geology, which have been expressed in our pages, and its fallacies *must* therefore be at least noticed here.

A brief *résumé* of a few of Sir W. Thomson's views on Geological Time was given in this *Review* in 1867, in an article on *The Origin of Species*, and the only attempt at an answer to them, as there stated, which we have yet seen, was that made by Dr. Hooker in his Presidential Address to the British Association at Norwich. We cannot now enter into a complete examination of his reasoning, but we may take a single very curious specimen—

"While fully admitting that Astronomy is the most certain in its methods and results of all sciences, that she (*sic*) has called forth some of the highest efforts of the intellect, and that her results far transcend in grandeur those of any other science, I think we may hesitate before we admit her queenship, her perfection, or her sole claims to interpretation and to prophecy. Her methods are mathematics, she may call geometry and algebra her handmaidens, but she is none the less their slave. No science is really perfect; certainly not that which lately erred 2,000,000 miles in so fundamental a datum as the earth's distance from the sun."

There is here a most unaccountable confusion between the results deduced *directly* from measurements of a quantity which requires some telescopic power to observe it at all, and those deduced from rigorous mathematical processes. That an *Observer* should make an error of a few *hundredths of a second* of arc (each corresponding to about *a hundred thousand miles* in the thence computed distance of the sun), in a quantity whose utmost value is some eight or nine seconds, surely need excite no surprise. Rather is it remarkable that such a close approximation has already been reached in a determination of such extreme delicacy. He who would deny this must have a very singular idea of what a second of arc is, and what limit of accuracy is attainable in the most perfect of astronomical observations. The coming transits of Venus will show what amount of improvement instruments and modes of observation have received within the last century, but few astronomers will say that there may not still remain an uncertainty of some hundreds of thousands of miles in the sun's distance. It is well worthy of notice, however, that experimental determinations of the velocity of light demonstrated the inexactness of the former

estimate of the sun's distance, and that such physical methods may possibly prove as efficient as more direct astronomical ones. The nature of the difficulty here considered has been well compared to that of determining the distance of a steeple some ten or twenty miles off; the observer being limited to the length of base-line afforded by an ordinary window-sill. But that unavoidable instrumental imperfections, and necessarily inadequate conditions of observing, should be regarded by *any* genuine scientific man as a defect in the *Science* of Physical Astronomy altogether passes belief.

To get a superior limit to the possible duration of something not very different from the present state of things on the earth other sciences than Geology must be appealed to; and here, because, and *only because*, our scientific men are usually mere specialists, the Natural Philosopher is required. What can a geologist, as such, tell about the nature, origin, and duration of the Sun's heat? Yet, suppose it could be shown that ten million years ago the sun was very much hotter than it now is, would not that fact have an important bearing on the length of time during which plants and animals have inhabited the earth? What can he tell us about the internal heat of the earth, and the rate at which it is at present being lost? Yet if it could be shown, on strict physical principles, that ten million years ago the underground temperature was at least that of red heat at a depth of one thousand feet below the surface, would not that materially influence his speculations? He may tell the mathematician to "mind his own business," but the mathematician must reply, "My business is in this case to save you from ignorantly committing egregious blunders, which not only retard the progress of your own science, but tend to render all science a laughing-stock to the uninitiated."

Having thus pointed out the nature of the questions involved in the present discussion, we shall examine, in order, the more specially combative of the various articles enumerated above.

Sir W. Thomson's paper, *On Geological Time*, was read to the Glasgow Geological Society last year. The main point referred to in it is the tidal retardation of the earth's motion, but the questions of the loss of energy from the sun and earth by radiation are also considered. He takes as his text the oft-quoted passage from Playfair, already alluded to, in which it is asserted that, however far we look into the past or the future, with reference either to the solar system or to the animal and vegetable kingdoms, "we discover no mark either of the commencement or the termination of the present order." As regards the

solar system, he founds his statement upon the celebrated result, then just obtained by Lagrange and Laplace, that the dimensions, inclinations, and eccentricities of the orbits of the planets could not be permanently altered by their mutual action, but must fluctuate in value between certain very narrow limits, though the periods of these fluctuations were shown to be in general very long. This was no doubt a most remarkable conclusion, one which still remains worthy of our highest admiration, but unfortunately *it is not true;* and with it falls the main prop of Playfair's statement. In obtaining the result, the French mathematicians used methods of approximation only (the solution of the problem in its generality appears even now to be hopeless), equivalent on the whole to omitting squares of the disturbing forces, *i.e.*, they virtually assumed, in calculating the effect of one planet on another, that the position of the first had not been affected by the second, besides formally neglecting terms of the third and higher orders of small quantities such as the eccentricities and inclinations. No doubt the quantities thus left out of account are exceedingly small, and negligible with perfect propriety, so long as the results of a few thousand years', or even a few tens of thousands of years', perturbations are considered; but it remains to be shown that, small as they are, they do not involve as surely the destruction of the solar system as the infinitesimal effect of each passing footstep renders in time new pavement necessary on a frequented street. In all probability this cannot be done; but even if it could, there is something more which at once decides the question. The investigations of Lagrange and Laplace took no account of the *resistance*, which physical science has shown is called into play by *every* motion of matter, and of which Newton was well aware, for he distinctly says,— "Majora autem planetarum et cometarum corpora motus suos, et progressivos et circulares, in spatiis minus resistentibus factos, conservant diutius." This implies that he knew that all motions of the planets and comets are resisted, but that in virtue of the masses of these bodies and the rarity of interplanetary matter, the effects of such forces of resistance would take a long time to accumulate sufficiently to become discoverable. But a "long time" is one thing, and "however far we look into the past or the future" is another and a very different thing (containing, in fact, the point originally at issue in this discussion.) Taking this into account, the proposition of Lagrange and Laplace retains merely its present mathematical and astronomical value; properly estimated, it turns *against* Playfair, and upsets his conclusion. To show how fixed was this notion of permanence in Playfair's mind,

and to what astounding limits of extravagance he was prepared to go, in spite of his better reason, whenever it was by possibility involved, take the following extract from his critique on *Vince's Gravitation* in the *Edinburgh Review* (1808-9). He is speaking of a very ridiculous hypothesis, put forward by John Bernoulli (who was no physicist, and as inferior to his brother in mathematics as he was in temper and in honesty), as to the cause of gravitation:—

"One circumstance in the favour of a hypothesis which has so little in other respects to recommend it, we must not omit to mention. It is, that the formation of the particles issuing from the sun into little balls which return to the sun again, presents us with something like a circulation, by which light is made to return to the luminary from which it was originally emitted. That light does so return in reality, by some means or other, is extremely probable, and conformable to the maxim, that nature nowhere admits of unlimited and progressive change. Such change, however slow, must destroy the order of which it makes a part, and is therefore very unlike the economy observed in the other phenomena of the heavens. Bernoulli's theory, therefore, includes at least one particular, in which the wisdom and simplicity of nature appears to have been consulted."

Sir W. Thomson proceeds to give an exceedingly clear and simple statement of the effects and *modus operandi* of one very interesting case of resistance—that offered by the tides to the earth's rotation. The celebrated Kant, who was a mathematician and a naturalist before he took up the study of metaphysics, and whose conclusions (like those of Sir W. R. Hamilton) are therefore usually of real value, or at least such as in general to merit serious consideration, long ago pointed out that the tidal wave, held back as it were by the moon and sun while the earth revolves underneath it, must act as a sort of friction-break, gradually diminishing the velocity of the earth's rotation. But Kant had no means of ascertaining, even roughly, what may be the amount of this effect; nor does he seem to have pointed out any other consequences of this action: such, for instance, as change of the moon's distance from the earth, or change of the earth's distance from the sun, and consequent change of length of the year. Now-a-days, with the principles of Energy to guide us, we know that in all friction heat is produced, and that this heat corresponds to so much energy of visible motion irrecoverably transformed, and therefore degraded. This degradation must last so long as there is relative motion of the earth and the tide-wave; and thus the final tendency (so far as the moon alone is concerned) is to diminish the earth's velocity of rotation until it shall turn always the same side to the moon, *i.e.*, to make the day of the same length as the lunar month and

lunar day. What an admirable verification of this physical prediction is afforded by the moon herself! The present condition of her surface shows that at some former period her whole crust, if not her whole substance, must have been in a molten state. Thinking of the enormous tides which must have been produced by the earth in this viscous mass of molten rock, we can easily understand how quickly its rate of rotation, whether originally greater or less than that of its rate of revolution, must have been compelled by friction to become identical with it; as we know it to be (*pace* Jellinger Symmonds, and his followers) by the simple fact that only one side of the moon has ever, within historical time, been visible to us. The following extract from Thomson's paper gives some additional particulars, and is well worthy of note as a most luminous verbal explanation of a subject which one might be inclined to fancy could hardly be raised from the domain of symbolic calculation:—

"But we may go further, and say that tidal action on the earth disturbs, by re-action, the moon. The tidal deformation of the water exercises the same influence on the moon as if she were attracted, not precisely in the line towards the earth's centre but, in a line slanting very slightly, relatively to her motion, in the direction forwards. The moon, then, continually experiences a force forward in her orbit by re-action from the waters of the sea. Now, it might be supposed for a moment that a force acting forwards would quicken the moon's motion; but, on the contrary, the action of that force is to retard her motion. It is a curious fact easily explained, that a force continually acting forward with the moon's motion will tend, in the long run, to make the moon's motion slower, and increase her distance from the earth. On the other hand, the effect of a resisting force on, for instance, the earth would undoubtedly be, in the course of ages, to make the earth go faster and faster round the sun. The reason is, that the resistance allows the earth to fall in a spiral path towards the sun, whose attraction generates more velocity than frictional resistance destroys. The tidal deformation of the water on the earth tends, on the whole, therefore, to retard the moon's angular motion in her orbit; but (by the accompanying augmentation of her distance from the earth) to increase the *moment of her motion* round the earth's centre. And the ultimate tendency—so far as the earth's rotation is concerned—must be to make the earth keep always the same face to the moon.

"It may be remarked, in passing, that the corresponding tendency has probably already had effect on the moon itself. The moon always turns the same face to the earth. If the moon were now a liquid mass, there would be enormous tides in it. The friction in that fluid would cause the moon to tend to turn the same face towards the earth: and we find the moon turns the same face always to the earth. It seems almost inevitable to our minds, constituted as they are, to connect possible cause and real effect, and say that a possible cause is a real

cause; and thus to believe the reason why the moon turns always the same side to us is because it was once a liquid mass which experienced tides and viscous resistance against the tidal motion. The only other view we can have—the only other hypothesis we can make—is, that the moon was created with such an angular velocity as to turn always the same face to the earth. But the course of speculative and physical science is absolutely irresistible as regards the relation between cause and effect. Whenever we can find a possible antecedent condition of matter, we cannot help inferring that that possible antecedent did really exist as a preceding condition—a condition, it may be, preceding any historical information we can have—but preceding and being a condition from which the present condition of things has originated by force acting according to laws controlling all matter."

But, it may be asked, how can even so beautiful a physical deduction as this be brought to bear upon the speculations of geologists? We answer, in many ways; but of these we need mention but one, our object at present being to show the nature rather than the extent of the argument. We again quote Thomson:—

"Now, if the earth is losing angular velocity at that great rate, at what rate might it have been rotating a thousand million years ago? It must have been rotating faster by one-seventh part than at present, and the centrifugal force must have been greater in the ratio of the square of 8 to the square of 7, that is, in the ratio of 64 to 49. There must have then been more centrifugal force at the equator due to rotation than now, in the proportion of 64 to 49. What does the theory of geologists say to that? There is just now at the equator one two-hundred-and-eighty-ninth part of the force of gravity relieved by centrifugal force. If the earth rotated seventeen times faster bodies would fly off at the equator. The present figure of the earth agrees closely with the supposition of its having been all fluid not many million years ago.

"The centrifugal force a hundred million years ago would be greater by about 3 per cent. than it is now, according to the preceding estimate of tidal retardation; and nothing we know regarding the figure of the earth, and the disposition of land and water, would justify us in saying that a body consolidated when there was more centrifugal force by 3 per cent. than now might not now be in all respects like the earth, so far as we know it at present. But if you go back to ten thousand million years ago—which, I believe, will not satisfy some geologists—the earth must have been rotating more than twice as fast as at present—and if it had been solid then, it must be (*sic*) now something totally different from what it is. Now, here is direct opposition between physical astronomy, and modern geology as represented by a very large, very influential, and, I may also add, in many respects, philosophical and sound body of geological investigators, constituting perhaps a majority of British geologists. It is quite certain that a great mistake has been made—that British popular geology at the present time

is in direct opposition to the principles of natural philosophy. Without going into details, I may say it is no matter whether the earth's lost time is 22 seconds, or considerably more or less than 22 seconds, in a century, the principle is the same. There cannot be uniformity. The earth is filled with evidences that it has not been going on for ever in the present state, and that there is a progress of events towards a state infinitely different from the present."

Surely the dullest of unprejudiced readers can hardly fail to see the gist of this passage; but, lest there should be any difficulty, we may summarize it thus: The figure of the earth, while still fluid, depended on its rate of rotation, being the more flattened the greater its velocity. The loss of velocity by tidal action is known well enough to show that had the earth become solid ten thousand million years ago, its shape could not have been that which it now presents. Why we have thus given again, in the roughest and coarsest form, one small part of the above extract, the reader will soon see.

As an amusing but painful contrast, let us turn to the remarks made on this in the *Pall Mall Gazette*. Here we find Thomson's reasoning about the *figure* of the earth transformed into something absolutely astounding:—"The first argument is based on the fact that the tides tend to retard the rate of the earth's rotation on its axis, and that, therefore, there was a time when the earth ROTATED TOO SWIFTLY FOR THE EXISTENCE OF LIFE." (The capitals are ours.) "Call you that, backing of your friends? A plague upon such backing, give me them that will face me." We can well fancy Professor Huxley's disgust at the "backing" of the *Pall Mall Gazette*.

Thomson proceeds to consider, as irrefragable disproofs of the Uniformitarian hypothesis, the rates at which both Sun and Earth are even now cooling. A hot body, cooling, has just before been somewhat hotter, and was then in all probability cooling more rapidly. This argument may be extended backwards for any required amount of time, without the least risk of physical error, and it must finally lead us, and within a very moderate number of millions of years, to a period when the earth, in consequence partly of its internal heat and partly of solar radiation, had at its surface a temperature quite inconsistent with the existence of organic life. The details of the requisite calculation, so far as internal heat is concerned, are very simple, and will be found appended to the paper (above mentioned) in the Proc. R.S.E., 1865, which we are tempted to quote in full:—

"The 'Doctrine of Uniformity' in Geology, as held by many of the most eminent of British geologists, assumes that the earth's surface and upper crust have been nearly as they are at present in temperature,

and other physical qualities, during millions of millions of years. But the heat which we know, by observation, to be now conducted out of the earth yearly is so great, that if this action had been going on with any approach to uniformity for 20,000 million years, the amount of heat lost out of the earth would have been about as much as would heat, by 100° Cent., a quantity of ordinary surface rock of 100 times the earth's bulk. [The calculation is appended.] This would be more than enough to melt a mass of surface-rock equal in bulk to the *whole earth*. No hypothesis as to chemical action, internal fluidity, effects of pressure at great depth, or possible character of substances in the interior of the earth, possessing the smallest vestige of probability, can justify the supposition that the earth's upper crust has remained nearly as it is, while from the whole, or from any part, of the earth, so great a quantity of heat has been lost."

That the reader may feel the full force of this argument, it is only necessary to point out to him that Sir W. Thomson expressly gives uniformitarianism the best possible conditions—conditions in fact really inadmissible, though (even when allowed) found utterly inadequate to the defence of the theory. For nothing in physics can be more certain than that the hotter a body is (*cæteris paribus*), the faster it loses its heat. Hence Thomson might have carried his argument (with perfect accuracy and propriety) a great deal farther than he has done in this paper. Here, however, he was dealing professedly with the geologists, and had to consult their exceeding weakness in matters pertaining, however slightly, to mathematics; while, three years before, in the first paper cited above, he had treated the question in a masterly way, and with the help of some of Fourier's beautiful formulæ, taking account of the greater rate of dissipation when the temperature of the globe was higher. This, of course, led him to results (as to the possible limit of time which can be allowed) considerably more restricted than those advanced in the paper we are now considering; and the geologists at once seize upon this *palpable inconsistency* (!) and declare that it shows that none of his results are worthy of acceptance. Their reasoning, if we can call it such, is not less absurd than would be that of a man who could say that there is *inconsistency* between such statements as the following—In order that two individuals who have been taxed, the one at ten per cent. on his capital, the other by an annual fine of £10, may now have each £100, twenty years ago the one must have had £822 and the other only £300:—neither being supposed to gain from any external source during the process. Or, from another point of view, if £300 be the greatest capital either could have had at starting—the process may have lasted *twenty* years with the annual fine of £10, while it could not

have lasted so much as *eleven* years at the annual ten per cent. of simple interest.

Thus Uniformitarianism has received its death-blow, and the operation has been performed as a duty, cheerfully but considerately, without malignity or ostentation. No one, in fact, except our Thersites, who seems neither to have got up the case made for the side he advocates, nor even apparently to be capable of distinguishing between Don Quixote and his squire,[1] has attempted a word in its defence. For, when we look to the Address of Professor Huxley, we find that, far from defending Uniformitarianism, he does his best to drop it entirely as an awkward witness, or rather as a discreditable acquaintance. In passing, for the time, from the consideration of Sir W. Thomson's first paper, we would say of it that, while it brings forward a formidable array of well-put objections, completely subversive of Uniformitarianism, it is obviously not meant as a complete sketch of the subject, fitted to answer, by anticipation, ingenious criticisms which may be, and have (since it was published) been brought forward by men of the calibre and determination of Professor Huxley. And it is therefore very satisfactory that such a man, qualified not merely by knowledge, but by acuteness of intellect, should have done his best (as we presume he has done from the circumstances under which his Address was delivered) to point out a possible flaw here and there in the argument, if not entirely to upset it. We do not, of course, assume that Professor Huxley has condensed into this brief Address all that he could say in answer to Sir W. Thomson; for the rest we must probably wait a little; but we may take for granted that he has seized upon what appeared to him to be the most inadmissible of Sir W. Thomson's statements.

Also, we may be allowed to remark, that it is unfortunate for the cause of progress that these statements should have appeared in a journal as yet comparatively obscure: though that journal, if it often contain contributions of such value, will soon, as regards circulation, stand on a par with any of its now more favoured rivals. No matter should they be lightly treated for the present, such articles will be dug up and admired by another generation:—when geologists have at length been brought to see that there can be no incompatibility between genuine scientific methods; and when the really good work which Huxley has done is alone remembered—this phase of opposition to rigorous physics being mercifully forgotten.

[1] "We entirely agree with Sir W. Thomson, that 'it is quite certain that a great mistake has been made;' but it is one similar in kind to Sancho Panza's (*sic*) attack on the windmill, and it has not been made by the British popular geologists."—*Pall Mall Gazette*, ut suprà.

Let us here record that the writer in the *Pall Mall Gazette* introduces Professor Huxley's Address as a "crushing refutation of Sir W. Thomson's conclusion." This will prepare the reader for the next scene of the tragedy.

The Address, which we now proceed to examine, is certainly clever, dashing, and plausible; but when perused with attention it is found to be seriously illogical. Professor Huxley several times changes front, and at least twice attacks Sir W. Thomson for saying what he has in effect himself conceded a page or two before.

He prefaces his Address by the following quotations from Sir W. Thomson's paper:—

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

The first of these is perhaps, if taken alone, rather vague, and therefore somewhat sensational. But the second completely explains the sense and bearing of the whole paper. What do we understand by British *popular geology?* Obviously not the views which may be held by a very few of the leading geologists, who are therefor in a sense looked on as heretics by the rest, but those views which are now being disseminated in all directions in Popular Lectures and Popular Text-books. It is mighty well for Professor Huxley to come forward and show that, so far as his own notions are concerned, a comparatively few millions of years will suffice for the observed development of organic life on the earth; but if in this respect he has by his own methods (possibly assisted by the conclusions of the first paper on our list, published about six years ago) arrived at nearly the same conclusions as Sir W. Thomson, why cry out against the Natural Philosopher? This is, to say the least, disingenuous, as is his oblivion of the very title of Thomson's second paper (above mentioned), which shows at once against which school the remarks were directed. But still more so is his affected ignorance of the patent fact that *popular* geologists (who in this country form the great majority of the geologists, and to whom Sir W. Thomson pointedly refers), with no less authorities than Lyell, Ramsay, Darwin, and Jukes at their head, still talk with the wildest looseness about thousands and tens of thousands of millions of years as the very least periods they can accept. Seeing that he is at one with Sir W. Thomson, inasmuch as the period which he considers to be required is nearly that which Thomson shows may be admitted, why does he not hail the coincidence as greatly strengthening his own independent conclusions? We

fear the true answer must be what we indicated above: Sir W. Thomson is not a professional geologist, and therefore must perforce be snubbed—*coûte que coûte.* As we have but too lately seen, when two Irish mobs are engaged in the sweet pastime of murdering one another, the interference of the police at once reconciles the hostile factions into one great brotherhood, which proceeds incontinently to sacrifice the police, as a votive offering on the altar of Peace.

The style of Professor Huxley's Address is well exhibited by the following extract from its opening sentences:—

"It surely is a matter of paramount importance for the British geologists (some of them very popular geologists too), here in solemn annual session assembled, to inquire whether the severe judgment thus passed upon them by so high an authority as Sir W. Thomson is one to which they must plead guilty *sans phrase*, or whether they are prepared to say 'not guilty,' and appeal for a reversal of the sentence to that higher court of educated scientific opinion to which we are all amenable."

"As your attorney-general for the time being, I thought I could not do better than get up the case with a view of (*sic*) 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 the position which is, 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."

Three things are very noticeable here:—*First* and *least*, there is satiety of what we are usually inclined to look upon as mere exuberant superfluities of metaphor: "attorney-general," "getting up the case," "not guilty," and so on—which have their climax, later in the Address, when Sir W. Thomson is "Hansardized," final causes are called the "*hetairæ* (*sic*) of philosophy," "Uniformitarianism insisted upon a practically unlimited bank of time, ready to discount any quantity of hypothetical paper," etc. etc. We are sorry to see that, in his reply, Sir W. Thomson has to a certain extent fallen in with this fooling, for we can give it no other name. Professor Huxley is far too acute and sensible a man to use such language except when it is required to mask defects in his case, and, it may be, to tickle the ears of some not particularly scientific audience. *Second*, "The higher court of educated scientific opinion" is certainly the true tribunal to decide on such a question,—but, unfortunately for Professor Huxley, there are many more educated scientific men who are mathematicians and natural philosophers, and to whom, in consequence, Sir W. Thomson's arguments bring the full force of intellectual conviction, than there are geologists of the same

high scientific training. And *Third*, How can a counsel hope to gain his cause (before such a court) who produces mere "mother-wit and common-sense," and an exercised intellect, if he has to discuss matters quite "foreign to his ordinary pursuits"? To humbug an every-day British jury is not, except in some very special cases, by any means a difficult, or even a very creditable, undertaking,—for now-a-days a British jury is in many respects nearly as effete and laughable (and very often also as dangerous) an institution as a British municipal corporation,—but the court of educated scientific opinion (understood as limited to those who are really scientific men) is, and always has been, quite capable of appretiating the merits of a case, and of detecting and exposing hollowness and unreality whenever they are present.

Professor Huxley begins with a most interesting semi-historical sketch and classification of the three systems of geological thought which have, in his opinion, alternately held sway. We cannot do better than quote some of his very clear descriptions:—[In all that follows the italics are ours, and the capitals are Professor Huxley's.]

"By CATASTROPHISM I mean any form of geological speculation which, in order to account for the phenomena of geology, supposes the operation of forces different in their nature, or immeasurably different in power, from those which we at present see in action in the universe."

"The Mosaic cosmogony is, in this sense, catastrophic, because it assumes the operation of extra-natural power. . . . *There was a time when catastrophism might pre-eminently have claimed the title of 'British popular geology'*; and assuredly it has yet many adherents, and reckons among its supporters some of the most honoured members of this Society."

"By UNIFORMITARIANISM I mean pre-eminently the teaching of Hutton and of Lyell." . . . .

"No one can doubt that the influence of uniformitarian views has been enormous, and, in the main, most beneficial and favourable to the progress of sound geology."

"*Nor can it be questioned that uniformitarianism has even a stronger title than catastrophism to call itself the geological speculation of Britain, or, if you will, British popular geology.* For it is eminently a British doctrine, and has even now made comparatively little progress on the continent of Europe. Nevertheless it seems to me to be open to serious criticism upon one of its aspects." . . .

"To my mind there appears to be no sort of necessary theoretical antagonism between Catastrophism and Uniformitarianism. On the contrary, it is very conceivable that catastrophes may be part and parcel of uniformity. Let me illustrate my case by analogy. The working of a clock is a model of uniform action; good time-keeping means uniformity of action. But the striking of the clock is essentially

a catastrophe; the hammer might be made to blow up a barrel of gunpowder, or turn on a deluge of water; and, by proper arrangement, the clock, instead of marking the hours, might strike at all sorts of irregular intervals, never twice alike in the intervals, force, or number of its blows. Nevertheless, all these irregular and apparently lawless catastrophes would be the results of an absolutely uniformitarian action; and we might have two schools of clock-theorists, one studying the hammer and the other the pendulum."

"Still less is there any necessary antagonism between either of these doctrines and that of EVOLUTION, which embraces all that is sound in both Catastrophism and Uniformitarianism, while it rejects the arbitrary assumptions of the one and the, as arbitrary, limitations of the other. Nor is the value of the doctrine of evolution to the philosophic thinker diminished by the fact that it applies the same method to the living and the not-living world, and embraces in one stupendous analogy the growth of a solar system from molecular chaos, the shaping of the earth from the nebulous cubhood of its youth, through innumerable changes and *immeasurable* ages, to its present form, and the development of a living being from the shapeless mass of protoplasm we term a germ."

"*I do not know whether Evolutionism can claim that amount of currency which would entitle it to be called British popular geology;* but, *more or less vaguely*, it is assuredly present in the minds of most geologists."

We must have one more extract, but it is of a really astonishing character:—[Here, however, the capitals are ours, the italics Professor Huxley's.]

"I do not suppose that, at the present day, any geologist would (*sic*) 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."

"The accusation that we have been running counter to the *principles* of natural philosophy, therefore, is devoid of foundation."

If the reader will take the trouble to look back again to these quotations, and especially to the portions which we have italicised in the earlier ones and put in capitals in the last, he will see that Professor Huxley says in effect: There *was* a time when Catastrophism was British popular geology, Evolutionism can but vaguely claim that amount of currency which would entitle it to be called British popular geology, but Uniformitarianism has the stronger title to call itself British popular geology. Let him remember that (as above quoted) Sir W. Thomson's remarks are directed entirely against British popular geology—

and that he has distinctly pointed out that by this term he meant the Uniformitarianism of Hutton, Playfair, and Lyell :— and then let him read these further remarks of Professor Huxley :—

"I have said that the three schools of geological speculation which I have termed Catastrophism, Uniformitarianism, and Evolutionism, are commonly supposed to be antagonistic to one another; and I presume it will have become obvious that, in my belief, the last is destined to swallow up the other two."

That is, because Professor Huxley, with but a few geologists as yet to back him, sees that Uniformitarianism cannot be successfully maintained (although according to him it is the teaching of British popular geologists), therefore

"It is not obvious, on the face of the matter, that we shall have to alter, or reform, our ways in any appreciable degree."

The only comment which this quibble requires is the pointing out how convenient is the Presidential "we," which really means Professor Huxley and a few other enlightened men, but is put forth to the world as meaning the Geological Society of London and with it the British popular geologists.

But the same spirit of quibbling is evident throughout all the foregoing extracts. Take, for instance, the so-called "analogy" of the clock. If Professor Huxley would only condescend for a moment to look at the question from the point of view of common sense, he would see that there is no uniformitarianism whatever in a clock—not even in a British, as distinguished from a French, one. For the *running down* of a clock is essentially a catastrophe; and, whether it pass uniformly (as a clock with weights, or with a spring and fusee, does) to its final stoppage; or, like French spring-timepieces which have no fusee, approach that consummation with continually decreasing force; matters not to the question. A clock bears absolutely no analogy to the case of the uniformitarian theory of the earth, treat it from what side you please: the mere fact of more or less of chain being on the barrel than on the fusee, and the constant change of their proportions, is alone sufficient entirely to upset Professor Huxley's reasoning; this *want of uniformity* being essential to the uniformity of the clock's going. But there is more to be observed, there is the exceedingly insidious danger that (as Professor Huxley without hesitation assumes may occur) there can be two, or more, sets of scientific men, studying the same phenomenon, and yet regarding it from such different points of view as to render unlikely any agreement between them. This, we need scarcely say, is absolute nonsense: for, if it has any meaning, it is calculated to justify the most perfunc-

tory performance of the duties of an observer, and to give credit to him who notes only those phases of a phenomenon which particularly suit his own views of its cause and relations. We are bound to assume that it is so meant, though Professor Huxley is surely far too shrewd a man to say (even to a popular audience, much less) to the great "court of scientific opinion to which we are all amenable," that there can be any excuse for a scientific man who looks at a question from so limited a point of view as his "analogy" appears to indicate.

There are many other points of a similar character, about which we should much like to say a few words. But we must let Sir W. Thomson have his own way in the matter of upsetting them. From his "Reply" to Professor Huxley we quote the following passage, which, long as it is, we fancy the reader would not wish to have had shortened:—in fact the obscurity of the Journal in which the Reply has appeared renders it more than usually necessary to furnish copious extracts:—

"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.

"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 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 Uniformitarianism' 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 twenty years as a temporary aberration worthy of being energetically protested against.

"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 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.

"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 asser-

tion 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."

We must refer to the original, or to the works, whether of Darwin and Jukes, or even of Haughton, Page, and others, for the unnecessarily elaborate proof of his accuracy given by Thomson. One of the extracts from Darwin is quite enough :—

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

Pages of extracts to the same purpose might easily be given. But if the reader will only carefully think of the bearings of this one, he will have as complete an idea of the circumstances as is required for our present argument.

In passing, however, let us once more cite the opinion of Thersites. He is actually presumptuous enough to say—

"One or two millions of years would be sufficient capital for the most extravagant disciple of Hutton and Lyell."

The reader, who may have thought, till now, that we were dealing too hardly with the *Pall Mall Gazette* critic, may well rub his eyes as he meets the above most astonishing display of ignorance on the part of a man who undertakes to criticise Sir W. Thomson.

A final quotation contains matter from Huxley and Darwin as well as from Thomson :—

"Professor Huxley, immediately after his statement . . . . . '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 inquire 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 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 revolution in natural science, yet does not admit how *incomprehensibly vast* have been the past periods of time, *may at*

[1] Darwin's *Origin of Species*, Edition 1859, p. 287.

*once close this volume.*' (Darwin's *Origin of Species by means of Natural Selection*, Edition 1859, p. 282)."

In the preceding pages we have, first, shown in what a very peculiar spirit the Geologists have received the proffered assistance of Mathematicians and Natural Philosophers, and we have given some apt, but painful, analogies from very common life. There are, however, some notable exceptions deserving of all honour; among them we may mention especially Professor Phillips, whose language on the subject of Geological Time has always been exceedingly moderate and philosophical. Secondly, we have endeavoured to show how it is that the "intrusion" of mathematical and physical science must be endured by the Geologist—since his subject requires such assistance, and he is generally unable to provide it for himself. Thirdly, we have briefly glanced at a few of the more prominent parts of two papers by Thomson, and of the Address of Huxley, and we hope we have made it clear that the geological "Attorney-General," however ready and versatile, has by far the worst case: that his side, in fact, cannot fail to lose. We must now, in conclusion, make a general survey of the subject, pointing out as far as our space enables us the extent to which it has been developed, the amount of uncertainty at present necessarily attending it, how far the mathematician has as yet been successful in his raid, and what data he requires in order to push the war still more vigorously home.

There are three points of view raised by Thomson which are at present mainly to be considered, and these we will briefly examine.

*First. The argument from underground temperature of the earth.* In regions where bores have been made, or mines sunk, the temperature is almost invariably found to increase (after the first few fathoms) as we penetrate more deeply, the accepted average being an increase of about 1° Fahr. for every 50 feet of descent. Now, the fundamental principle of the Dissipation of Energy, as exhibited in Thermal Conduction, is that heat *always* tends to a uniform distribution of temperature; and therefore always passes from places where the temperature is higher to those where it is lower. But it is certain that the upper strata are not, on the whole, becoming warmer year by year. Hence from mere observation of underground temperature, we *know* that there must be, even now, a constant flow of heat outward through the earth's crust. The problem then suggests itself:—How long has this outflow been going on, through a solid and habitable crust, and what was its rate at long distant epochs? The question is not easy to answer at once and definitely, for the difficulty consists not in the mathe-

matical part of the solution, but mainly in the *want of experimental data*, such as, for instance, the temperature of fusion of average surface rock, the law of its thermal conductivity as depending on temperature, its laws of dilatation, and its specific heat. Hence, at present, our solutions can only be approximate, but for all that the Natural Philosopher is enabled to assign certain limits; which are far less vague than those of the "popular geologists," and which have at least a genuine physical foundation. We must, however, first inquire whence is supplied that internal heat which is even now being lost. Several hypotheses have to be considered. Poisson long ago suggested that the earth may have, at some early period, passed through a warmer region of space, and there acquired, from without, the heat which it is now dissipating. This hypothesis is not very difficult to dispose of. The data, regarding the conductivity and thermal capacity of the different surface rocks in the neighbourhood of Edinburgh, furnished by the underground thermometers of the late Principal Forbes, have enabled Sir W. Thomson to show that if this supposed passage through a warmer region took place from 1250 to 5000 years ago, the temperature of that region must have been from 25° to 50° Fahr. above the present mean temperature of the earth's surface. If it took place 20,000 years ago, the excess must have been 100° F., being doubled when the period allowed is quadrupled. History proves the first to be untenable, and it is not likely that the geologists will admit the second. The hotter we assume this region of space to have been, the longer ago must the passage through it have been; and the longer must the temperature at the surface of the earth have been consistent with organic life. But, when we thus come to enormous periods, the actual *cause* of the earth's heating is comparatively of little consequence, so that this hypothesis becomes undistinguishable in results from the third below.

Next we have the supposition that the earth's internal heat is due to chemical action, in itself very improbable, except possibly in certain small detached regions of volcanic activity. It is scarcely necessary to make any farther remark on this than the very obvious one that, if it could be shown that such is really the cause, it is fatal to the Uniformitarian theory, for, in consequence of the steady loss above mentioned, the earth must now contain far less potential energy of chemical affinity than it did ages ago. Obvious as this may appear to the Natural Philosopher, it would seem that some geologists, with Lyell at their head, actually imagine that a species of uniformitarianism may be maintained in the interior by thermo-electric processes; the heat produced by chemical combination being supposed to pro-

duce thermo-electric currents, and these in turn being employed in decomposing again the products formed, thus giving a perpetual cycle. As Sir W. Thomson remarks, this extraordinary notion "violates the principles of natural philosophy in exactly the same manner, and to the same degree, as to believe that a clock constructed with a self-winding movement may fulfil the expectations of its ingenious inventor by going for ever."

If we take the far more probable hypothesis that the internal heat of the earth, like that of the sun, is due mainly to the impacts of discrete masses falling together from great distances by mutual gravitation, and that now it is merely a hot body cooling according to ordinary laws; it is obvious that by making reasonable assumptions (in the present want of definite experimental data) as to the melting-point of ordinary rock masses, we may determine roughly a superior limit to the time which has elapsed since the superficial strata were in a molten state. This has been done by Thomson, and he finds that 200,000,000 years may have elapsed since the crust consolidated if the melting point of rock be about 10,000° F. (this being an extremely high estimate). If, however, the more reasonable estimate of 7000° F. be taken, this superior limit is reduced to 98,000,000 years. Thomson goes on to show that when once the surface is consolidated, if it do not break up and sink (it contracts, according to Bischoff, 20 per cent. in solidifying) in the lighter fluid below, not many years may have passed before the globe became habitable. In fact, after 10,000 years the rate of increase of temperature downwards would not be more than about 2° F. per foot, a quantity which would produce little effect except on deep-rooted plants; and almost none as regards alteration of the mean temperature at the surface. It is well to observe, in connexion with these speculations, that Sir W. Thomson seems to prefer to assume that the consolidation took place almost simultaneously *throughout* the globe; the inner strata tending to consolidate at a far higher temperature than those near the surface, in consequence of the enormous pressure to which they are subjected. This follows as a thermodynamic consequence from the result of Bischoff just quoted. Though the melting point may be raised considerably by pressure, it does not necessarily follow that solidification takes place nearly simultaneously at all depths; so that it is possible that the crust may have solidified long before the interior. What would probably happen in such a case has been graphically described by Thomson as follows:—

"It is probable that crust may thus form over wide extents of surface, and may be temporarily buoyed up by the vesicular character it may have retained from the ebullition of the liquid in some places,

or, at all events, it may be held up by the viscidity of the liquid, until it has acquired some considerable thickness sufficient to allow gravity to manifest its claim, and sink the heavier solid below the lighter liquid. This process must go on until the sunk portions of crust build up from the bottom a sufficiently close-ribbed solid skeleton or frame, to allow fresh incrustations to remain bridging across the now small areas of lava pools or lakes.

"In the honeycombed solid and liquid mass thus formed, there must be a continual tendency for the liquid, in consequence of its less specific gravity, to work its way up, whether by masses of solid falling from the roofs of vesicles or tunnels, and causing earthquake shocks, or by the roof breaking quite through where very thin, so as to cause two such hollows to unite, or the liquid of any of them to flow out freely over the outer surface of the earth; or by gradual subsidence of the solid, owing to the thermodynamic melting, which portions of it, under intense stress, must experience, according to views recently published by Professor James Thomson. The results which must follow from this tendency seem sufficiently great and various to account for all that we see at present, and all that we learn from geological investigation, of earthquakes, of upheavals, and subsidences of solid, and eruptions of melted rock."

*Second. The argument from tidal retardation of the earth's rotation.* We have already considered this part of the subject, so far, at least, as to show its bearing upon the question of geological time. The discovery of this retardation, as something which really exists and can be measured, in contrast with Kant's pointing out that there is a *vera causa*, is very curious. The secular acceleration of the moon's mean motion, proved by calculating back to the recorded eclipses of the 3rd and 8th centuries B.C., was long a serious difficulty to physical astronomers, till Laplace first suggested a possible cause in the secular alteration of the eccentricity of the earth's orbit. His calculations gave almost exactly the observed result; and the question was supposed to be settled. Some years ago, however, Adams showed that Laplace's investigation was seriously defective, and that a correct analysis reduced his result by half; so that half of the acceleration of the moon's mean motion remained unaccounted for.

Then the hint given by Kant (which had been recently brought forward independently by Helmholtz, Mayer, J. Thomson, and others) was remembered, and applied to remove the remaining difficulty. It is obvious that, if the earth's rotation be really becoming slower, since it is employed fundamentally in our measurement of time, all other motions must appear relatively accelerated. With reference to this argument, Professor Huxley has committed a singular blunder, in meeting his adversary with a suggestion which is at once and with deadly

effect turned against its author. In fact, as Sir W. Thomson says, "Professor Huxley's hypothesis, . . . if it were valid, would therefore prove retardation by the tides six times as much as that which we have ventured to estimate!" He proceeds to make another and still graver blunder, when he asks, "If tidal retardation can be thus checked and overthrown by other temporary conditions, what becomes of the confident assertion, based upon the assumed uniformity of tidal retardation, that ten thousand million years ago the earth must have been rotating more than twice as fast as at present?" Thomson at once shows that this really entitles him to *shorten* the period which he had before roughly assigned: and he appends a note which, from so quiet and gentle an antagonist, Professor Huxley must look upon as strangely sarcastic, as to the opinion implied in the above extract, that tidal retardation is a *temporary* condition. A very small amount of mathematical training would have sufficed to preserve so able a man from serious mistakes like these.

*Third. The argument from the Sun's Heat.* Here again we must quote Thomson, as he has put the argument into an exceedingly compact and comprehensive form:—

"But it is not only to the effect of the tides that we refer for such conclusions. Go to other bodies besides the earth and moon; consider the sun. We depend on the sun very much for the existing order of things. Life on this earth would not be possible without the sun, that is, life under the present conditions—life such as we know and can reason about. When Playfair spoke of the planetary bodies as being perpetual in their motion, did it not occur to him to ask, What about the sun's heat? Is the sun a miraculous body ordered to give out heat and to shine for ever? Perhaps the sun was so created. He would be a rash man who would say it was not—all things are possible to Creative Power. But we know, also, that Creative Power has created in our minds a wish to investigate and a capacity for investigating; and there is nothing too rash, there is nothing audacious, in questioning human assumptions regarding Creative Power. Have we reason to believe Creative Power did order the sun to go on, and shine, and give out heat for ever? Are we to suppose that the sun is a perpetual miracle? I use the word *miracle* in the sense of a perpetual violation of those laws of action between matter and matter which we are allowed to investigate here at the surface of the earth, in our laboratories and mechanical workshops. The geologists who have uncompromisingly adopted Playfair's maxim have reasoned as if the sun were so created. I believe it was altogether thoughtlessness that led them ever to put themselves in that position; because these same geologists are very strenuous in insisting that we must consider the laws observable in the present state of things as perennial laws. I think we may even consider them as having gone too far in assuming

that we must consider present laws—a very small part of which we have been able to observe—as sufficient samples of the perennial laws regulating the whole universe in all time. But I believe it has been altogether an oversight by which they have been led to neglect so greatly the fact of the sun's heat and light.

"The mutual actions and motions of the heavenly bodies have been regarded as if light had been seen and heat felt without any evolution of mechanical energy at all. Yet what an amount of mechanical energy is emitted from the sun every year! If we calculate the exact mechanical value of the heat he emits in 81 days, we find it equivalent to the whole motion of the earth in her orbit round the sun. The motion of the earth in her orbit round the sun has a certain mechanical value; a certain quantity of steam power would be required, acting for a certain time, to set a body as great as the earth into motion with the same velocity. That same amount of steam power employed for the same time in rubbing two stones together would generate an enormous quantity of heat, as much heat as the sun emits in 81 days. But suppose the earth's motion were destroyed, what would become of the earth? Suppose it were to be suddenly, by an obstacle, stopped in its motion round the sun? It would suddenly give out 81 times as much heat as the sun gives out in a day, and would begin falling towards the sun, and would acquire on the way such a velocity that, in the collision, a blaze of light and heat would be produced in the course of a few minutes equal to what the sun emits in 95 years. That is, indeed, a prodigious amount of heat; but just consider the result if all the planetary bodies were to fall into the sun. Take Jupiter with its enormous mass, which, if falling into the sun, would in a few moments cause an evolution of 32,240 years' heat. Take them all together—suppose all the planets were falling into the sun—the whole emission of heat due to all the planets striking the sun, with the velocities they would acquire in falling from their present distances, would amount to something under 46,000 years' heat. We do not know these figures very well. They may be wrong by ten or twenty or thirty per cent., but that does not influence much the kind of inference we draw from them. Now, what a drop in the ocean is the amount of energy of the motion of the planets, and work to be done in them before they reach their haven of rest, the sun, compared with what the sun has emitted already! I suppose all geologists admit that the sun has shone more than 46,000 years? Indeed, all consider it well established, that the sun has already, in geological periods, emitted ten, twenty, a hundred, perhaps a thousand—I won't say a hundred thousand—but perhaps a thousand times as much heat as would be produced by all the planets falling together into the sun. And yet Playfair and his followers have totally disregarded this prodigious dissipation of energy. He speaks of the existing state of things as if it must or could have been perennial.

"Now, if the sun is not created a miraculous body, to shine on and give out heat for ever, we must suppose it to be a body subject to the laws of matter (I do not say there may not be laws which we

have not discovered) but, at all events, not violating any laws we have discovered or believe we have discovered. We must deal with the sun as we should with any large mass of molten iron, or silicon, or sodium. We do not know whether there is most of the iron, or the silicon, or the sodium—certainly there is sodium; as I learned from Stokes before the end of the year 1851; and certainly, as Kirchhoff has splendidly proved, there is iron. But we must reason upon the sun as if it were some body having properties such as bodies we know have. And this is also worthy of attention:—naturalists affirm that every body the earth has ever met in its course through the universe, has, when examined, been proved to contain only known elements—chemical substances such as we know and have previously met on the earth's surface. If we could get from the sun a piece of its substance cooled, we should find it to consist of stone or slag, or metal, or crystallized rock, or something that would not astonish us. So we must reason on the sun according to properties of matter known to us here.

"In 1854, I advocated the hypothesis that the energy continually emitted as light (or radiant heat) might be replenished constantly by meteors falling into the sun from year to year; but very strong reasons have induced me to leave that part of the theory then advocated by me which asserted that the energy radiating out from year to year is supplied from year to year; and to adopt Helmholtz's theory, that the sun's heat was generated in ancient times by the work of mutual gravity between masses falling together to form his body. The strongest reason which compelled me to give up the former hypothesis was, that the amount of bodies circulating round the sun within a short distance of his surface, which would be required to give even two or three thousand years of heat, must be so great, that a comet shooting in to near the sun's surface and coming away again, would inevitably show signs of resistance to a degree that no comet has shown. In fact, we have strong reason to believe that there is not circulating round the sun, at present, enough of meteors to constitute a few thousand years of future sun-heat. If, then, we are obliged to give up every source of supply from without—and I say it advisedly, because there is no sub-marine wire, no 'underground railway,' leading into the sun—we see all round the sun, and we know that there is no other access of energy into the sun than meteors,—if, then, we have strong reason to believe that there is no continual supply of energy to the sun, we are driven to the conclusion that it is losing energy. Now, let us take any reasonable view we can. Suppose it is a great burning mass, a great mass of material not yet combined, but ready to combine, a great mass of gun-cotton, a great mass of gunpowder, or nitro-glycerine, or some other body having in small compass the potential elements of a vast development of energy. We may imagine that to be the case, and that he (*sic*) is continually burning from the combustion of elements within himself; or we may imagine the sun to be merely a heated body cooling; but imagine it as we please, we cannot estimate more on any probable hypothesis, than a few million years of

heat. When I say a few millions, I must say at the same time, that I consider one hundred millions as being a few, and I cannot see a *decided* reason against admitting that the sun may have had in it one hundred million years of heat, according to its present rate of emission, in the shape of energy. An article, by myself, published in *Macmillan's Magazine*, for March 1862, on the age of the sun's heat, explains results of investigation into various questions as to possibilities regarding the amount of the heat that the sun could have, dealing with it as you would with a stone, or a piece of matter, only taking into account the sun's dimensions, which showed it to be possible that the sun may have already illuminated the earth for as many as one hundred million years, but at the same time also rendered it almost certain that he had not illuminated the earth for five hundred millions of years. The estimates here are necessarily very vague, but yet, vague as they are, I do not know that it is possible, upon any reasonable estimate, founded on known properties of matter, to say that we can believe the sun has really illuminated the earth for five hundred million years."

Professor Huxley endeavours to answer this by attempting to show that Sir W. Thomson, fifteen years ago, "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." Thomson shows that this assertion is incorrect, and that his view of the entire possible meteoric supply of solar heat, from masses nearer to the sun than is the earth, when properly stated, would give, at the utmost, material for 300,000 years only, at the present rate of dissipation. He carefully guarded himself, in his original paper, from any such charge as that brought by Huxley, for he expressly showed that a meteor supply, such as would annually make up for the sun's loss, if coming from space external to the earth's orbit, would involve such an augmentation of the sun's mass as would within the last 2000 years have dislocated the seasons by a month and a half:—the observed dislocation in 2600 years being but an hour and three-quarters. And he pointed out that the true test of how much of the sun's loss can be supplied by meteors at present circulating in orbits less than that of the earth is best to be determined by the perturbations of Mercury. These have been examined with great care by Leverrier; and the result is unfavourable to the existence of any supply worth taking into consideration in the study of the question before us, indicating, as it does, an amount of potential energy equivalent only to a few hundred years of solar heat. Hence, as it has been shown by Helmholtz, Thomson, and others, that if the sun's mass had been made up in the most effective manner of those chemical substances known to us, which would give the greatest possible result, the heat of combination of these could not have supplied

so much as 5000 years' loss, even at the *present* rate of radiation; the only theory of solar heat left us is that developed by Helmholtz, which regards the sun as a hot body cooling; the heat having been produced during the falling together of its parts. The specific heat of such a mass, in consequence of the pressure to which it is subject in the interior, must be, according to Thomson's latest estimate, from 10 to 10,000 times as great as that of an equal mass of water under ordinary pressure. These limits are purposely left very wide; and they show that the sun loses by its radiation 1° F. in temperature in a period longer than four years, but less than 4000 years. Thomson ends his reply on this part of the subject with the very sensible remark: "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."

Now it is to be carefully observed, with regard to the three independent lines of argument just explained, that it is no answer to show that each is, from its very nature, somewhat vague in the results which it yields. The argument from the three is not, as Professor Huxley seems to think, only as strong as the weakest of the three; on the contrary, the reasoning is strictly cumulative, and Thomson's position cannot be successfully attacked except by a complete upsetting of at least two of his lines of argument, combined with a great enfeebling of the third. In truth, when we come to examine the question as a whole, giving its full weight to each of the separate details, we find that we may, with considerable probability, say that Natural Philosophy already points to a period of some *ten* or *fifteen* millions of years as all that can be allowed for the purposes of the geologist and palæontologist; and that it is not unlikely that, with better experimental data, this period may be still farther reduced. In fact, even Professor Huxley's enlightened concession that a limit of 100,000,000, 200,000,000 or 300,000,000 years requires no complete revolution in geological speculation (though it is matter of notoriety that to Lyell and Darwin, and to the great mass of British popular geologists, such periods would be of little use):—even this concession will soon not satisfy the Natural Philosophers; who, but with the important difference of having right on their side, will soon follow up their advantage in a manner somewhat resembling the recent behaviour of the great Yankee nation in the matter of the *Alabama Claims*. For, elaborate and suggestive as have been all of Thomson's articles, this great question can hardly yet be said to be more than opened; and its future progress rests quite as much with the physical experimenter as with the mathematician.

At the commencement of this article we borrowed from Milton an account of the concomitants of the preparations for a terrific combat: there we had to stop, as farther quotation might have been personal; we have seen the issue of the fight, and can now sum it up in the words of Horace, which we take to be descriptive of the triumph of *Scientific Truth* over all assailants, however numerous and powerful:—

> "Sed quid Typhoëus et validus Mimas,
> aut quid minaci Porphyrion statu,
> quid Rhœtus, evulsisque truncis
> Enceladus jaculator audax,
> contra *sonantem Palladis ægida*
> possent ruentes?"

In conclusion, as the assailants named by Horace are unfortunately all of the gigantic order, we must supplement the passage by again recurring to our Thersites who writes anonymous nonsense for the *Pall Mall Gazette*, and who bitterly attacks, without understanding them, the conclusions of one of the greatest philosophers the world has ever seen. That a man should be more ignorant of Cervantes' great novel than is the merest schoolboy, implies no blame: no more does it imply blame that he should be so ignorant as to consider this question as one of "*Mathematics* versus *Geology*," instead of *Reasoning* versus *Unreason*; that he should fancy that any disciple of Hutton and Lyell could be content with one or two millions of years: nor even that he should imagine that Sir W. Thomson's arguments concerning an increase of 15 per cent. in the earth's angular velocity have something to do with the existence of life:—all this is his own misfortune; but why should he increase it by publishing his ignorance to the few readers of the *Pall Mall Gazette* who are able to distinguish between true science and venomous but absurd attempts at smartness? Such a writer does real harm, by preventing the popular extension of true scientific knowledge: and too often, as is the case with the present specimen, tries to hold up to ridicule lofty merit which he is utterly unable to appretiate. No true scientific man could have written as he has done about Sir W. Thomson, certainly not in such a tone, without appending at least his initials. And a genuine *littérateur* would never have made such an exhibition of himself; but would, in the shrewd words of Professor Huxley, have endeavoured "to gain his cause, mainly by force of mother-wit and common-sense, aided by training in other intellectual exercises."