On Geological Time

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1.—A GREAT reform in geological speculation seems now to have become necessary. A very earnest effort was made by geologists, at the end of last century, to bring geology within the region of physical science, to emancipate it from the dictation of authority and from dogmatic hypotheses. The necessity for *more time* to account for geological phenomena than was then generally supposed to be necessary, became apparent to all who studied with candour and with accuracy the phenomena presented by the surface of the earth. About the end of last century, also, physical astronomers made great steps in the theory of the motions of the heavenly bodies, and, among other remarkable propositions, the very celebrated theorem of the stability of the planetary motions was announced. That theorem was taken up somewhat rashly; and supposed to imply more than it really did with reference to the permanence of the solar system. It was probably it which Playfair had in his mind when he wrote that celebrated and often-quoted passage—

> "How often these vicissitudes of decay and renovation have been repeated is not for us to determine; they constitute a series of which, as the author of this theory has remarked, we neither see the beginning nor the end; a circumstance that accords well with what is known concerning other parts of the economy of the world. In the continuation of the different species of animals and vegetables that inhabit the earth, we discern neither a beginning nor an end; in the planetary motions where geometry has carried the eye so far both into the future and the past, we discover no mark either of the commencement or the termination of the present order. It is unreasonable, indeed, to suppose that such marks should anywhere exist. The Author of nature has not given laws to the universe, which, like the institutions of men, carry in themselves the elements of their own destruction. He has not permitted in His works any symptoms of infancy, or of old age, or any sign by which we may estimate either their future or their past duration. He may put an end, as He, no doubt, gave a beginning to the present system, at some determinate time; but we may safely conclude that this great *catastrophe* will not be brought about by any of the laws now existing, and that it is not indicated by anything which we perceive." (*Illustrations of the Huttonian Theory,* § 118)

Nothing could possibly be further from the truth than that statement. It is pervaded by a confusion between "present order," or "present system," and "laws now existing"—between destruction of the earth as a place habitable to beings such as now live on it, and a decline or failure of law and order in the universe. The theorem of the French mathematicians regarding the motions of the heavenly bodies is a theorem of approximate application, and one which professedly neglects frictional resistance of every kind; and the statement that the phenomena presented by the earth's crust contain no evidence of a beginning, and no indication of progress towards an end, is founded, I think, upon what is very clearly a complete misinterpretation of the physical laws under which all are agreed that these actions take place.

2.—I shall endeavour to arrange what I have to say in two divisions, taking the quotation from Playfair, as it were, as the text:—First, The motions of the heavenly bodies; the earth as one of them: and, Secondly, The phenomena presented by the earth's crust.

3.—Now, in the first place, the motions of the heavenly bodies are subject to resistance, which was not taken into account in the investigations of the French mathematicians. They gave out the theorem, that so far as the mutual attractions between the sun and the planets, and the law of inertia affecting the motions of each body, without any opposition of resistance, are concerned, certain disturbances known to exist among the motions of the heavenly bodies cannot become infinite, but must oscillate within certain limits.

4.—For instance, during a period—very many thousands of years say—the eccentricity of the earth's orbit round the sun may go on increasing. It might be supposed that that eccentricity could go on increasing so much, that at last the earth's path might cross that of one of the other planets. Serious disturbances in the motions of the two bodies might result, or even a fatal collision. But the theorem of the French mathematicians asserts, that while the eccentricity might go on increasing for a certain time, it has its limits; thus declaring that there are oscillations and variations, but no continued variation in one direction. And this is a very important theorem undoubtedly. On details of the formula expressing it are founded all the calculations of modern physical astronomers regarding what are called the secular variations of the elements of the planetary orbits. But the French mathematicians were quite aware that, in making this statement, they neglected resistance. Those who quoted the grand theorem at which they arrived, did not perceive that exclusion. English philosophers and naturalists might surely have taken warning from Newton's simple brief decisive statement, "*majora autem planetarum et cometarum corpora motûs suos et progressivos et circulares, in spatiis minus resistentibus factos, conservant diutius;*"[1] and have at least to some degree limited and qualified the expressions we so often meet in their popular writings, implying a perpetuity of the "existing order," past and future.

5.—Laplace was perfectly aware of the existence of resistance to fluid motion. In his theory of the tides, he points out most distinctly that if oscillation were established on the surface of the ocean—oscillation on a grand scale affecting the oceans—the waters of the Atlantic, for instance, swelling up, and those of the Pacific shrinking down, time about—that if such an oscillation were, by any force made to commence, then, in a very short time, he says "probably in a few months," we might expect it would altogether subside; and in his theory of the tides, he treats the motion of the sea altogether as a motion of oscillation. There then is a tacit admission of the fact of resistance. But that tidal resistance influences the rotation of the earth, or, by reaction, the motions of the moon and sun, Laplace does not explicitly state. The modern theory of energy was imperfectly understood by Laplace and Lagrange. Lagrange, it is true, gave a foundation for the mathematical treatment of Dynamics, in which the theory of energy was the one great principle; but he did not point out the application of the theory of energy to some of the consequence which now, in the present state of science, interest us perhaps more than any other conclusions which have been drawn from mathematical and physical reasoning. I am therefore entitled to speak so far of the science of energy as modern, although it was from Toricelli, Newton, John Bernouilli, and Lagrange, that we have learned the abstract dynamical principles of the science of energy. Even this abstract theory of energy teaches, that if there is resistance of any kind (against the tidal motion of the waters, for instance), *that* resistance must react upon some body, and take from that body, or from bodies connected with the phenomena, energy.

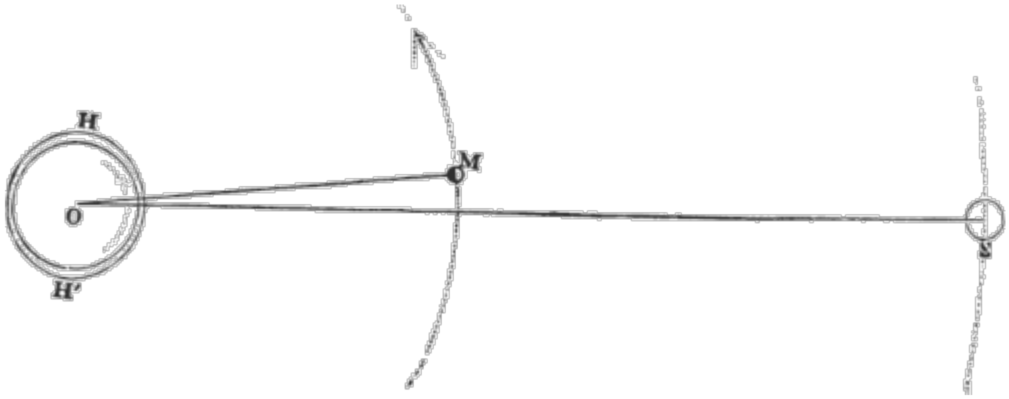
6.—The cause of the tides, as every one knows, of course, is the attraction of the moon and sun. The fact that the moon attracts the portion of the sea nearest to her more than she attracts the centre of the earth, and the centre of the earth than the remote parts of the ocean, gives rise to a tendency to draw water towards the moon, and leave a protuberance on the other side from the moon. That is the tendency; but the water of the ocean never gets time to take the exact form to which it tends. Just as if a large bath were suddenly tilted up and let down again: the water in it, at the time it was tilted up, tended to alter itself according to the new position of the bath, but there was no time for that tendency to have static effect; so it is for the waters of the ocean when the moon comes to be over, for instance, the middle of the Atlantic, and *tends* to draw the water towards her position and to leave it protuberant on the remote side of the earth. It is curious that in books of navigation the *tendency* has been so often spoken of as if that were the effect. An interesting correspondence occurred in the columns of the *North British Daily Mail* about a year ago, in which Newton's theory of the tides was disproved out of Norrie's *Navigation*. Norrie, in his work, describes the tendency; Newton in his theory describes the tendency, but points out that the waters of the ocean are in a state of continual oscillation and reverberation as it were (between two opposite continents, for instance, as between Africa on the one side and America on the other), and that at no one instant does the tendency have static effect according to what has been called "the equilibrium tide." Now it is the imaginary equilibrium tide that is often described as the theoretical tide in books on navigation, though the many readers of these books, with limited information as to what was written by Newton, Laplace, and Airy, accuse Newton of all the errors they have been taught.

7.—When we consider the moon as causing the tides, and the change from high to low as depending on the rotation of the earth, it becomes obvious that if there is resistance to the motion of the water that constitutes the tides, that resistance must directly affect the earth, and must react on those bodies, the moon and the sun, whose attractions cause the tides. The theory of energy declares, in perfectly general terms, that as there is frictional resistance, there must be loss of energy somewhere. We are not now merely content to say there is loss of energy by resistance, but the modern theory must account for what becomes of that energy. It is particularly to Joule that the full establishment of the true explanation as to what becomes of energy that is lost in friction is due. I suppose every one here present knows Joule's explanation, namely, that heat is generated. The friction of the waters against the bottom of the sea and against one another, in rubbing, so to speak, as they must to move about, to rise in one place and fall in another—the friction of waters especially in the channels where there are tide races, gives rise to the generation of heat. Well, now, the end, where it altogether leaves our earth to be dissipated through space, is heat. The beginning to which we can at present trace the first source of that energy is in the motions of the moon and the earth. A little consideration shows us, by a very general kind of reasoning, that that particular component of the motion which at zero would give rise to no tides must tend to become zero. This we see as included in a very general proposition applicable to every possible case of action in nature. Now, if the motion of the earth in its rotation, relative to the moon in its revolution round the earth, were zero, there would be no rise and fall of water in lunar tides; the earth would always turn the same face to the moon, and then it would be always high water towards the moon, low water in the intermediate circle, and high water from the moon, but there would be no motion of the waters relatively to the earth and so no friction. The tendency of friction must then, according to the general principle, be to reduce the relative motions of the earth and moon to that condition. However, it is satisfactory to know that we do not need to base a conclusion on so excessively general terms of the theory of energy as those. It is easy to see that the mutual action between the moon and the earth must tend, in virtue of the tides, to diminish the rapidity of the earth's rotation, and increase the moment of the moon's motion round the earth.

8.—"The tidal spheroid," you must understand, is not a reality, because the waters do not cover the whole earth, as we are here on *terra firma* to know. But there is a perfectly definite surface, being an elliptic spheroid calculated by mathematical rule, which is such that if it were the outer boundary of a distribution of water over a globe perfectly covered with water, this mass of water would exercise to an extremely close approximation the same force upon any distant particle of matter, and experience the same reacting force, as our tidally disturbed waters really do. That is what is properly called the tidal spheroid. It averages, as it were, for the whole globe, the tidal effect of the disturbing body considered. The tidal spheroid averaging the moon's effect alone, is called the luni-tidal spheroid; and that for the sun is called the soli-tidal spheroid. The resultant tidal spheroid, representing on the same principle the average displacement of the water produced by the combined influence of the two bodies, is found by simply adding the displacements from the undisturbed figure, represented respectively by the luni-tidal and soli-tidal spheroids.

9.—If there were no frictional resistance against the tides, each separate tidal spheroid would have its longest diameter perpendicular to the line joining the centre of the earth with that of the disturbing body, whether moon or sun. [2] When the joint influence of the sun and moon is analyzed by mathematical reasoning, it is found that there would be for either separately, a tidal spheroid fulfilling the condition just defined. Thus the dynamical result of the tendency of either body would be *low water* at the time of the high water of the imaginary equilibrium tide, and *vice versâ*, on the average of the whole earth. By the lunar tide, for instance, there would be low water when the moon is crossing the meridian, and (supposing for simplicity, the moon to be in the plane of the earth's equator) there would be high water when she is rising and setting. When the moon and sun are exactly in conjunction and opposition, the longest and shortest axes of their tidal spheroids would agree; and the highest and lowest tides on the average of the whole earth would be the high water and low water immediately before and after, or after and before, the time of new and full moon. This, be it remembered, is on the supposition of no tidal resistance, but does not involve any assumption whatever of regularity, whether as to the boundary of the sea of as to uniformity of its depth.

10.—Now, it is well known that, in this part of the world, the "spring tides" are observed to be late by from a day and a half to three days after new moon and full moon. On the West Coast of Ireland the interval is about thirty-six hours; it amounts to about sixty hours at London Bridge, and has intermediate values at intermediate points of the British Channel. Along the Atlantic Coast of Europe the interval seems to be between eighteen hours, which may be about its amount at the Cape of Good Hope, and thirty-six hours its value on the West Coast of Ireland; and it is probable that in all seas the spring tides are *late* by an interval of, in general, something more than twelve hours and less than three days after the time of new moon and full moon. Hence, the crests of the luni-tidal and soli-tidal spheroids are not coincident when the earth, moon, and sun are in one line, but are coincident at some time, probably exceeding twelve hours, after the moon has crossed the line joining the earth and sun. This then is decisive in showing a sensible effect of resistance to the tidal motions, as was first, I believe, remarked by Airy.[3] That there must be such resistance is quite certain to us, from our knowledge of the properties of matter; but it is very interesting, and it is very important with reference to the subject of my present statement, to find a sensible effect on the average tides of the whole ocean due to resistance against the tidal motions.



11.—The accompanying woodcut illustrates the position of the sun and moon, and of the longest axis of the resultant tidal spheroid at the average time of spring tides for the whole earth. It represents a section in the plane of the equator, in which for simplicity the sun and moon are both supposed to be. The spectator being supposed to look at the diagram from the north side, sees the earth rotating, and the moon revolving round the earth's centre, each in the direction opposite to the motion of the hands of a watch. If there were no tidal friction, OMS would be in one straight line, and HH′, the longest axis of the tidal spheroid would be perpendicular to it. What observation on the time of spring tides proves, is simply that OM is inclined to OS forwards by the angle through which the moon advances before the sun, in the time by which the spring tides are late. If this time were twelve hours, the angle MOS would be 6°. The dynamical theory proves that each must make an acute angle with OH, the line from the earth's centre to the tidal crest towards which the parts of the earth between it and OM or OS are rotating; or, in other words, that high water is made something *earlier* on the average than the time when it would be were there no friction; that is to say, a little before the rising and setting of the sun and moon. And thus what we have seen to be proved by observation on the average time of spring tides, is that the time of the lunar tide is more advanced by frictional resistance than is the time of the solar tide: a conclusion quite agreeing with what is to be anticipated by mathematical theory.[4] Considering now for simplicity the lunar tide alone, if we imagine the whole mass of the earth and waters upon it to be bisected by a plane perpendicular to HH′, through O, the centre of gravity, it is obvious that the attractions of the moon on these two halves will not balance round the centre of gravity of the whole, but that, on the contrary, the combined influence of the greater attraction on the nearer protuberance of the waters (H), and of the less attraction on the more remote protuberance (H′) would, if the whole were rigid, tend to turn the line H′H towards the direction OM. If the round earth rotated inside the waters, these not sharing its rotation, the effect would be as if a mechanical friction strap or belt were applied round the earth's equator, and were held from turning by a couple of forces equal in moment (or rotational importance) to that calculated as what is technically called "The moment round the earth's axis" of the moon's attraction on the two protuberances.

12.—But the waters do not get pulled back by the moon as a whole. They are, as a whole, drawn along with the solid earth by friction on the bottom, and by friction of water on water. Therefore, from century to century, the water moves along with the earth. Though it is due, in the first place, to a force in the water, the resultant effect on the earth and the water is the same as if the whole were a solid globe rotating inside the supposed friction-strap. The amount of each of these forces constituting the supposed couple holding back the equatorial friction-strap, would be equal to the weight of two million tons, according to the hypothesis and calculations taken from the Rede Lecture (Cambridge, May, 1866) on the "dissipation of energy," in the abstract appended to the present article. This hypothesis supposes HH′ to be inclined to OM, at an angle of 45°, the position in which, with a given amount of protuberance, the tidal retardation of the earth's rotation would be a maximum: having been assumed for the purpose of estimating a superior limit to the conceivable amount of the influence in question. The resulting retardation would be the same as if the earth had (as a common "terrestrial globe,") pivots at the North and South poled, each half an inch diameter, and resisting forces were applied tangentially to these pivots, amounting in all to four thousand million million tons force. With the same supposed degree of protuberance in the luni-tidal spheroid,[5] the frictional resistance would be something between the amount estimated, and one-fifth of it, if the angle HOM were anything between 45° and 84°. But if, as is most probably the case, the average lateness of the spring tides behind the full and change, amounts at least to twelve hours, the angle MOS cannot be less than 6°, this being the angle through which the moon moves in her orbit in twelve hours; and HOS is certainly something short of 90°. Hence it is almost certain that HOM has some value between 45° and 84°. I conclude that either the average spheroidal tide must be less than 1 1/8 ft. or the amount of resistance to the earth's rotation must exceed one-fifth of the amount which I had estimated as a superior limit; the only doubtful assumption being, that the lateness of the spring tides behind the times of full and change, is not less on the average than twelve hours.

13.—The general tendency of that action, then, is to diminish the velocity of the earth's rotation round its axis, and lengthen the duration of the day. That there is such a tendency has long been known to philosophers of the more abstract kind. It is difficult to say who first promulgated this idea. It has been recently stated that the metaphysician Kant first asserted that the earth's rotation is diminished through the influence of the tides. This I know for certain, that the idea was first given to me by my brother, Professor James Thomson. As long ago as the first meeting of the British Association in Glasgow, 1840, he propounded as a necessary result of the theory of energy, that friction of the tides in channels must give rise to a loss of something then called *vis-viva*, from the motion of the earth and the moon. More recently published articles, and especially those of Mayer, the great German advocate of the modern theory of heat, who did so much to urge the reception of the idea of an equivalence between heat and mechanical power, point out that the rotation of the earth must be diminished by the tides.

14.—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 forwards 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.

15.—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. the theory that the moon has been brought to her present condition of rotation by tidal friction of her own mass was, I believe, first given by Helmholtz. I cannot say so certainly, because so many philosophers have speculated and drawn conclusions regarding antecedents of the solar system from very general philosophic principles; but, so far as I know, that view was first given by him.

16.—It is impossible, with the imperfect data we possess as to the tides, to calculate how much their effect in diminishing the earth's rotation really is. But even from such data as those referred to in § 11, it can be shown that the tidal retardation of the earth's rotation must be something very sensible. Still, it is unsatisfactory to be in the position of asserting that we know there must be retardation (we cannot tell how much) and then to be told, in opposition to that theory, that observations of ancient eclipses make it certain that the length of the day has not varied by one ten-millionth part of twenty-four hours from 721 years before the Christian era.[6] The calculation was first made by Laplace. It depended in part on the historical facts of two eclipses of the moon, seen in Babylon, one of them March 19, 721 B.C., which was first perceived "when one hour after her rising was fully past;"[7] the other on the 22nd December, 313 years B.C., which was first perceived "half an hour before the end of night," and which, though now known to have lasted only about an hour and a half, had not come to an end when the moon set. The rotation of the earth cannot have experienced *much* retardation during these 2700 years, or else the moon-rise must have taken place *after* instead of *before* the beginning of the first of those eclipses; and it cannot have experienced much acceleration,[8] or else the moon must have set at Babylon before the second mentioned eclipse commenced, which, therefore could not have been seen from that place. But Dunthorne showed that these records and various observations regarding many other less ancient eclipses all agree in *demonstrating* the correctness of a suspicion which Halley had raised that the moon's mean angular motion has been accelerated somewhat *relatively to the earth as time-keeper*; and he estimated the amount of this acceleration to be 20 seconds of angular velocity per century gained per century.[9] Laplace, accepting this conclusion, attempted to explain it by showing that the planets cause indirectly an acceleration of the moon's angular velocity through their influence in producing a secular diminution of the eccentricity of the earth's orbit. The principle is admitted, and to Laplace is attributed, and must always be attributed, the very great discovery of the cause of an apparent secular acceleration of the moon's mean motion. He calculated out the results of this discovery, and they seemed to tally precisely with the supposition that the earth's velocity of rotation had been constant since 721 B.C.;[10] but in 1853 our great English physical astronomer, Adams, pointed out an error of a technical kind in Laplace's process—the omitting to take into account *in the tangential component* of the sun's disturbing force on the moon, the disturbing influence of the variation o the eccentricity of the earth's orbit, and he worked out the theory with this correction.

17.—The result, roughly stated, was to halve the amount of acceleration calculated by Laplace and to leave half of Dunthorne's observed *relative* acceleration of the moon to be accounted for otherwise. In 1853, Adams communicated to Delaunay, one of the great French mathematicians, his final result, that at the end of a century the moon is 5.7 seconds of angle in advance of the position she had when relatively to the meridian of the earth, according to the angular velocity of the moon's motion at the beginning of the century, and the acceleration of the moon's motion truly calculated from the various disturbing causes then recognised. This, then, shows an unaccounted for gain per century of 11″.4 per century of angular velocity of the moon's motion, on the hypothesis that the earth's angular velocity is uniform. Delaunay soon after verified this result, and about the beginning of 1866 suggested that the true explanation may be the retardation of the earth's rotation by tidal friction. Using the hypothesis that the cause of the discrepancy is retardation by tidal friction, and allowing for the consequent retardation of the moon's mean motion, Adams, in an estimate which he has recently worked out in conjunction with Professor Tait and myself, found, on a certain assumption as to the proportion of retardations due to the moon and the sun—that 22 seconds of time is the error by which the earth would in a century get behind a thoroughly perfect clock rated at the beginning of the century.

18.—Thus the most probable result that physical astronomy gives us up to the present time is that the earth is not an accurate chronometer, but, on the contrary, is getting slower and slower, if tested by a truly perfect clock—a clock as good as an astronomical clock ought just now to be, and that is at least 200 times as good as astronomical clocks are—because astronomical clocks are just a great disgrace to the mechanical genius of Europe and America as chronometer watches are a credit. Astronomical clocks go only about two or three times as well as pocket chronometer watches; although the latter, from the continual agitations to which they are exposed, are in very disadvantageous circumstances. When they shall be made two or three hundred times as good as they are, we shall have an instrument which, for use during a few centuries, will be a superior time-keeper to the earth; and it will not then be necessary to set the clock by the stars, but we shall test the earth's motion by the clock. However, that is only in anticipation. Perhaps we may not live to see that use of the clock. In the meantime we are obliged to put up with the earth and stars as a means for regulating our clocks. Failing a good clock to check the earth by, we have to take the best we can find and apply corrections to it. The moon is a very unequal time-keeper, but by prodigious labour, carried out by Newton, Clairaut, Laplace, Plana, Handen, Adams, and Delaunay, the errors in the moon's motion are very accurately known. The moon's rotation round the earth is as it were a clock hand going round in about 29 days, and the earth is as it were the hand of another clock going round in 24 hours. The only timekeeper by which we can at present test the accuracy of the earth's motion is the moon. Imperfect as the moon is, and error has, you see, been discovered in the earth as a time-keeper, on reference to the moon. Consider that fact, and see whether it justifies the statement I have referred to by Playfair in his Illustrations of Hutton's theory, that there is nothing in the motions of the heavenly bodies that tends to their own dissolution or to a permanent alteration of the existing state of things. For instance, no resistance tending to stop the progress of the earth!

19.—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.

20.—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 does not satisfy some great 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 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.

21.—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 suns 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.

22.—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 distance, 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.

23.—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 are 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 crystallised rock, or something that would not astonish us. So we must reason on the sun according to properties of matter known to us here.

24.—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 too 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 gun-powder, 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 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**,[11] 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.

25.—But Playfair looks to the earth, and says that while the heavenly bodies give every evidence of having gone on for ever as now, the earth, in the phenomena presented all through its crust, to unprejudiced observers, gives similar evidence, and seems to indicate no evidence of a beginning, and no progress or advance towards an end. Now, let us consider the question of underground heat. The earth, if we bore into it anywhere, is warm, and if we could apply the test deep enough, we should, no doubt, find it very warm. Suppose you should have here before you a globe of sandstone, and boring into it found it warm, boring into another place found it warm, and so on, would it be reasonable to say that that globe of sandstone has been just as it is for a thousand days? You would say, "No; that sandstone has been in the fire, and heated not many hours ago." It would be just as reasonable to take a hot water jar, such as is used in carriages, and say that that bottle has been as it is for ever—as it was for Playfair to assert that the earth could have been for ever as it is now, and that it shows no traces of a beginning, no progress towards an end.

26.—There have been feeble attempts to reason away the argument from underground heat. The geologists, to whose theory I object, do at the same time, I believe, admit that the temperature increases downwards, wherever observations have been made. They have hitherto taken a somewhat supine view of the subject. Admitting that there is in many places evidence of an increase of temperature downwards, they say they have not evidence enough to show that there is increase of temperature downwards in all parts of the earth, of enough of evidence to allow us to say that the theory that accounts for underground heat, by local chemical action, may not be true. This being the state of the case as regards underground heat, where must we apply to get evidence? Observation; observation only. We must go and look. We must bore the earth here in the neighbourhood. We must examine underground temperature in other places. We must send out and bore under the African deserts, where water has not reached for hundreds of years. The whole earth must be made subject to a geothermic survey. Having been deeply impressed with these views for many years, I have long endeavoured, in vain, to call the attention of geologists to them. I now feel very greatly indebted to the Geological Society of Glasgow, for giving me the opportunity of speaking of them this evening. I may be allowed to add that on the occasion of the recent meeting of the British Association, at Dundee, the importance of investigation of underground temperature was not denied by geologists, before whom the subject was brought in the first instance, on that occasion, by a paper by the Hungarian naturalist, Schwarez. A result of the discussion which followed the reading of that paper was the appointment of a committee[12] for investigating underground temperature.

27.—The laws of the progress of summer heat and winter cold downwards were investigated thoroughly by the great French mathematician Fourier, and made the subject of observation in different localities. We know very well now what temperature, so far as the annual variation is concerned, may be expected to be found at ten, twenty, or thirty feet down, according to the conductivity and capacity for heat of the strata. If we bore down to a depth of twenty-four feet we may find, in mid-winter, the highest temperature. Probably, last midsummer's heat is now about reaching thirty feet below this place. Principal Forbes instituted experiments on the Calton Hill, at the Craigleith Quarry, and the Experimental Gardens, and in these three places the observations were continued for several weeks, the temperature being observed every week. From these observations, he calculated the conducting powers of the different strata, and his results were, I believe, the first obtained of an accurate kind, regarding the conducting power of rock in its natural condition in the earth's crust. Ångström made similar experiments in Sweden, and deduced results on the same principles. Similar observations were made at Greenwich, and calculated by Dr. Everett; so from these results we may consider the conductivity of ordinary surface rocks as generally very well known.

28.—But the question, how much does temperature increase downwards from hundred feet to hundred feet, is one which has been but very imperfectly investigated indeed. Observation of temperature in mines, as Schwarez points out, and as Philips pointed out in the Geological Society of London, are very unsatisfactory. Air circulating through the mines, and water percolating and being pumped out, give rise to disturbances so great, that we cannot say if in a lower level of a mine, we find a colder temperature than in a higher level, the result is due to colder strata. The best ventilated deep mine will be the coolest; and in passing, I may remark, which is, perhaps, of some interest in the present and prospective state of the question of the supply of coal, that we know no limit of the depth to which coal may be worked, depending on terrestrial temperature. Suppose there was coal, or rather charcoal, where the strata were red hot, it might be gone into and that with perfect ease. All that is necessary is plenty of ventilation. This will keep the temperature cool enough for working, and thus there is no limit whatever to the depth to which the miner may proceed. I do not say it would not be enormously more expensive to bring up coal (gas-coke, or charcoal) from four thousand fathoms, if there is any at so great a depth, than to bring up what we call coal from a depth of one hundred or two hundred fathoms, but that it could be got at and brought out, notwithstanding even a red hot temperature of the surrounding strata is quite certain. Plenty of ventilation, conducted on proper thermodynamic principles,[13] will give quite a satisfactory temperature for the workers in the mines.

29.—All sound naturalists agree that we cannot derive accurate knowledge of underground temperature from mines. But every bore that is made for the purpose of testing minerals gives an opportunity of observation. If a bore is made, and is left for two or three days, it will take the temperature of the surrounding strata. Let down a thermometer into it, take proper means for ascertaining its indications, draw it up, and you have the measure of the temperature at each depth. There are most abundant opportunities for geothermic surveys in this locality by the numerous bores made with a view to testing minerals, and which have been left either for a time or permanently without being made the centre of a shaft. Through the kindness of Mr. Campbell, of Blythswood, several bores in the neighbourhood of his house have been put at the disposal of the committee of the British Association, to which I have referred. In one of these bores very accurate observations have been made, showing an increase of temperature downwards, but which is not exactly the same in all the strata, the difference being, no doubt, due to different thermal conductivities of their different substances. I need not specify minutely the numbers, but I may say in a general way, that the average increase is almost exactly 1/50 of a degree Fahrenheit per foot of descent; which agrees with the estimate generally admitted as a rough average for the rate of increase of underground temperature in other localities.

Another bore has been put at the disposal of the committee, and the investigation of it is to be commenced immediately, so that I hope in the course of a few days some accurate results will be got. It has been selected because the mining engineer states in his report that the coal has been very much burned or charred, showing the effect of heat; and it becomes an interesting question, Are there any remains of that heat that charred the coal in ancient times; or has it passed off so long ago that the strata are now not sensibly warmer on account of it?

30.—I shall conclude by simply referring to calculations regarding the quantity of heat at present conducted out from the interior of the earth, which I have given in an article, entitled **"The 'Doctrine of Uniformity' in Geology, briefly refuted;"**[14] and to analytical investigations regarding *antecedents* of the present condition of underground heat contained in a paper **"On the Secular Cooling of the Earth,"**[15] appended to the volume on *Natural Philosophy* by Professor Tait and myself, recently published. The first of these shows, by mere calculation of the actual conduction, that the present rate of increase of underground temperature could not last for twenty or thirty thousand million years, without there being dissipated out of the earth as much heat as would be given off by a quantity of ordinary surface rock equal to 100 times the earth's mass, cooling from 100° cent. to 0°. In the second, by the analytical investigation of antecedents it is shown that the present condition implies either a heating of the surface, within the last 20,000 years of as much as 100 degrees, Fahr., or a greater heating all over the surface at some time farther back than 20,000 years.[16]

Now, are geologists prepared to admit that at some time within the last 20,000 years there has been all over the earth so high a temperature as that? I presume not; no geologist—no *modern* geologist—would for a moment admit the hypothesis that the present state of underground heat is due to a heating of the surface at so late a period as 20,000 years ago. If that is not admitted we are driven to a greater heat at some time more than 20,000 years ago. A greater heating all over the surface than 100° Fahr., would kill nearly all existing plants and animals, I may safely say. Are geologists prepared to say that all life was killed off the earth 50,000, 100,000, or 200,000 years ago? For the uniformity theory, the further back the time of high surface temperature is put the better; but the further back the time of heating, the hotter it must have been. The best for those who draw most largely on time is that which puts it farthest back, and that is the theory that the heating was enough to melt the whole. But even if it was enough to melt the whole, we must still admit some limit, such as fifty million years, one hundred million years, or two or three hundred million years ago.[17] Beyond that we cannot go. The argument described (§ 19) above regarding the earth's rotation shows that the earth has not gone on as at present for a thousand million years. Dynamical theory of the sun's heat renders it almost impossible that the earth's surface has been illuminated by the sun many times ten million years. And when finally we consider underground temperature we find ourselves driven to the conclusion in every way, that the existing state of things on the earth, life on the earth, all geological history showing continuity of life, must be limited within some such period of past time as one hundred million years.

APPENDIX.

ON THE OBSERVATIONS AND CALCULATIONS REQUIRED TO FIND THE TIDAL RETARDATION OF THE EARTH'S ROTATION.[18]

[ED. Format of equations changed from original due to HTML limitations]

THE first *publication* of any definite estimate of the possible amount of the diminution of rotatory velocity experienced by the earth through tidal friction is due, I believe, to Kant. It is founded on calculating the moment round the earth's centre of the attraction of the moon, on a regular spheroidal shell of water symmetrical about its longest axis, this being (through the influence of fluid friction) kept in a position inclined backwards at an acute angle to the line from the earth's centre to the moon. One of the simplest ways of seeing the result is this:—First, by the known conclusions as to the attractions of ellipsoids, or still more easily by the consideration of the proper "spherical harmonic"[19] (or Laplace's coefficient) of the second degree, we see that an equipotential surface lying close to the bounding surface of a nearly spherical homogeneous solid ellipsoid is approximately an ellipsoid with axes differing from one another by three-fifths of the amounts of the differences of the corresponding axes of the ellipsoidal boundary. Now it is known[20] that a homogeneous prolate spheroid of revolution attracts points outside it approximately as if its mass were collected in a uniform bar having its ends in the foci of the equipotential spheroid. If, for example, a globe of water of 21,000,000 feet radius (this being nearly enough the earth's radius) be altered into a prolate spheroid with longest radii exceeding the shortest radii by two feet, the equipotential spheroid will have longest and shortest radii differing by 6/5 of a foot. The foci of this latter will be at 7,100 feet on each side of the centre; and therefore the resultant of gravitation between the supposed spheroid of water and external bodies will be the same as if its whole mass were collected in a uniform bar of 14,200 feet length. But by a well-known proposition, [21] a uniform line FF′ (a diagram is unnecessary) attracts a point M in the line MK bisecting the angle FMF′. Let CQ be a perpendicular from C, the middle point of F′F, to this bisecting line MK. If CM be $60 \times 21 \times 10^6$ (the moon's distance), and if the angle FCM be 45° we find, by elementary geometry, CQ=.02 pf a foot (about ¼ inch). The mass of a globe of water equal in bulk to the earth is $.97 \times 10^{21}$ tons.[22] And, the moon's mass being about 1/80 of the earth's, the attraction of the moon on a ton at the earth's distance is $1/80 \times 1/60^2$, or 1/290,000 of a ton force, if, for brevity, we call a ton force the ordinary terrestrial weight of a ton—that is to say, the amount of the earth's attraction on a ton at its surface. Hence the whole force of the moon on a globe of water equal in bulk to the earth is $(.97 \times 10^{21}) / 290{,}000$, or 3.3×10^{15} tons force. If, then, the tidal disturbance were exactly what we have supposed, or if it were (however irregular) such as to have the same resultant effect, the retarding influence of the moon's attraction would be that of 3.3×10^{15} tons force acting in the place of the equator and in a line passing the centre at 1/50 of a foot distance. Or it would be the same as a simple frictional resistance (as of a friction-brake) consisting of 3.3×10^{15} tons force acting tangentially against the motion of a pivot or axel of about ½ inch diameter. To estimate the retardation produced by this, we shall suppose the square of the earth's radius of gyration, instead of being 2/5, as it would be if the mass were homogeneous, to be 1/3 of the square of the radius of figure, as it is made to be, by Laplace's probable law of the increasing density inwards, and by the amount of precession calculated on the supposition that the earth is quite rigid. Hence (if we take g=32.2 feet per second generated per second, and the earth's mass = 5.3×10^{21} tons) the loss of angular velocity per second, on the other suppositions we have made, will be

$$(32.2 \times 3.3 \times 10^{15} \times .02) / (5.3 \times 10^{21} \times 1/3\ (21 \times 10^6)^2)\ ,\ \text{or}\ 2.7 \times 10^{-21}.$$

The loss of angular velocity in a century would be $31½ \times 10^{8}$ times this, or 8.5×10^{-12}, which is as much as $1.16 / 10^7$ of $2\pi / 86400$, the present angular velocity. Thus in a century the earth would be rotating so much slower that, regarded as a time-keeper, it would lose about 1.16 seconds in ten million, or 3.6 seconds in a year. And the accumulation of effect of uniform retardation at that rate would throw the earth as a time-keeper behind a perfect chronometer (set to agree with it in rate and absolute indication at any time) by 180 seconds at the end of a century, 720 seconds at the end of two centuries, and so on. In the present very imperfect state of clock-making (which scarcely produces an astronomical clock two or three times more accurate than a marine chronometer or good pocket-watch), the only chronometer by which we can check the earth is one which goes much worse—the moon. The marvellous skill and vast labour devoted to the lunar theory by the great physical astronomers Adams and Delaunay, seem to have settled that the earth has really lost in a century about ten seconds of time on the moon corrected for all the perturbations which they had taken into account. M. Delaunay has suggested that the true cause may be tidal friction, which he has proved to be probably sufficient by some such estimate as the preceding.[23] But the many disturbing influences to which the earth is exposed render it a very untrustworthy time-keeper. For instance, let us suppose ice to melt from the polar regions (20° round each pole, we may say) to the extent of something more than a foot thick, enough to give 1.1 foot of water over those areas, or .066 of a foot of water if spread over the whole globe, which would in reality raise the sea-level by only some such almost undiscoverable difference as ¾ of an inch, or an inch. This, or the reverse, which we believe might happen any year, and could certainly not be detected without far more accurate observations and calculations for the mean sea-level than any hitherto made, would slacken or quicken the earth's rate as a time-keeper by one-tenth of a second per year.[24]

Again an excellent suggestion, supported by calculations which show it to be not improbable, has been made to the French Academy by M. Dufour, that the retardation of the earth's rotation indicated by M. Delaunay, or some considerable part of it, may be due to an increase of its moment of inertia by the incorporation of meteors falling on its surface. If we suppose the previous average moment of momentum of the meteors round the earth's axis to be zero, their influence will be calculated just as I have calculated that of the supposed melting of ice. Thus meteors falling on the earth in fine powder (as is in all probability the lot of the greater number that enter the earth's atmosphere and do not escape into external space again) enough to form a layer about 1/20 of a foot thick in 100 years, if of 2.4 times the density of water, would produce the supposed retardation of 10^8 on the time shown by the earth's rotation. But this would also accelerate the moon's mean motion by the same proportional amount; and therefore a layer of meteor-dust accumulating at the rate of 1/40 of a foot per century, or 1 foot in 4,000 years, would suffice to explain Adams and Delaunay's result. I see no other way of directly testing the probable truth of M. Dufour's very interesting hypothesis than to chemically analyze quantities of natural dust taken from any suitable localities (such dust, for instance, as has accumulated in two or three thousand years to depths of many feet over Egyptian, Greek, and Roman monuments). Should a considerable amount of iron with a large proportion of nickel be found or not found, strong evidence for or against the meteoric origin of a sensible part of the dust would be afforded.

Another source of error in the earth as a time-keeper, which has often been discussed, is its shrinking by cooling. But I find by the estimates I have given elsewhere[25] of the present state of deep underground temperatures, and by taking 1/100000 as the vertical contraction per degree centigrade of cooling in the earth's crust, that the gain of time on this account by the earth, regarded as a clock, must be extremely small, and may even not amount to more in a century than 1/30 of a second or 1/6000 of the amount estimated above as conceivably due to tidal friction.

Footnotes

- [1] *Principia.* "Explanation of First Law of Motion."
- [2] This assertion is founded not on observation, but on dynamical principles. It depends on the truth that, if the tide-generating influence of either sun or moon were suddenly to cease, the period of the chief oscillation that would result would be *greater* than either twelve solar or twelve lunar hours. The period of this oscillation would be *less* than either twelve lunar or twelve solar hours, if the sea were very much deeper than it is, or if it were considerably deeper, and also less obstructed by land. (*See* § 11.) If this were the case, the greatest axes of the luni-tidal and soli-tidal spheroids would be in line with the moon and sun respectively, and there would be average high water of either component tide when the body to which it is due crosses the meridian; also, the average times of greatest tide would still be those of new and full moon. But if the depth of the sea and the configuration of the land were such that the chief period of oscillation could be intermediate between twelve solar and twelve lunar hours, the greatest axis of the luni-tidal spheroid would be in line with the moon; but that of the soli-tidal spheroid would be perpendicular to the line joining the earth's and the moon's centres. In this case, the times of spring tides would be those of quarter moons. In the first of these two unreal cases, the effect of tidal friction would be to make the time of average high water somewhat *later* in each component tide, than the time when the body producing it crosses the meridian; and this deviation would be greater for the sun than for the moon. Thus, the time of spring tides would be, as it is, somewhat later than the times of new moon and full moon. But, in the second of the imagined cases, the effect of friction would be to advance the time of solar high water and to retard the time of lunar high water; and thus the time of spring tides would be somewhat before the times of the quarter moons.
- [3] "Tides and Waves," § 544—*Encylopedia Metropolitana.*
- [4] *See* footnote on § 9.
- [5] This is such as would make the average amount of rise of tide from lowest to highest, for the whole surface of the earth, 1 1/3 feet, if the tides were everywhere, those of the luni-tidal spheroid covering the whole earth.
- [6] Poisson *Traitè de Mècanique*, Sec. 433, Vol. ii, Ed. 1833.

- [7] Dunthorne "On the Acceleration of the Moon," *Phil. Trans.* 1749. (Hutton's *Abridgement*, Vol. ix.)

- [8] As regards the earth's rotation, it seems to have been only *acceleration* (due to cooling and shrinking) that was suspected until Kant and others showed that the tides must produce retardation. But Laplace proved, by calculations founded on Fourier's theory of the conduction of heat (not at all on astronomical data), that the acceleration by shrinking on account of cooling cannot have shortened the day by as much as 1-300th of a second of time; that is to say, by about a twenty-five millionth of its own amount in the last 2,000 years.

- [9] Grant's *History of Physical Astronomy*, p. 60.

- [10] Laplace's theory gave him 21″.7 per century of angular velocity as the average gain per century during the last 2,500 years, which exceeds Dunthorne's estimate from observation by 8½ per cent.—Grant's *History of Physical Astronomy*, p. 63.

- [11] Republished as Appendix (E) to Thomson and Tait's *Natural Philosophy*, Vol i., Part ii, 2nd ed. (1885).

- [12] [Note of Jan. 4, 1893.] This Committee has been reappointed from year to year ever since, and, under the persevering and able guidance of Prof. Everett, continues to obtain and publish valuable information regarding underground temperature in all parts of the world.

- [13] That is to say, the air must be compressed and cooled at the upper surface, or at some convenient place in the mine or shaft, at no great depth below the surface. This cold dense air must be conducted to the lowest levels through a strong enough pipe, and allowed to expand into the mine through an engine, or engines, like a common high-pressure engine working expansively. A great part of the work of this engine must be spent otherwise than in generating heat in the mine; for instance, it may be used for working the gear to raise the mineral. A portion of its work may be spent in cutting out the minerals, as is sometimes done at present by compressed-air engines; but it must be remembered that the full dynamical equivalent of this part of the work of the engine is developed in heat in the mine. Probably best practical plan for working very deep mines will be to employ the engine power used at the surface all in compressed air; The compressed air to be cooled, either by water, if there is a sufficient cold water supply, or by radiation to the sky, and by atmospheric convection. This condensed air should be used for working the engine or engines in proper places at the great depths required to work the gear for raising the minerals, etc., and small cutting engines in various parts of the workings. Thus a sufficient supply of cool air may be distributed through the mine. If the ordinary method of ventilation by *drawing out air*, whether by an air pump, or by a fire burning at the foot of the vertical shaft used, the down current of fresh air will be warmed to the amount of nearly 1°C. for every fifty fathoms of descent, by the natural compression of the air through its own weight or more exactly 18° cent. per 1000 fathoms; being 1/329 of a degree centigrade per foot, according to an investigation which I have given in the *Proceedings of the Manchester Literary and Philosophical Society*, January 1862, "On the Convective Equilibrium of Temperature in the Atmosphere." (*Mathematical and Physical Papers*, Vol. iii., p. 255.)

- [14] *Proceedings R. S. E.*, December, 1865, and p. 6, above.
- [15] First republished in the *Trans. R. S. E.*, 1862, published in *Mathematical and Physical Papers*, Vol. iii., p. 295.
- [16] Thomson and Tait, Appendix D. § (j.)
- [17] Thomson and Tait, Appendix D. § (r.)
- [18] From the Rede Lecture, Cambridge, May 23, 1866, "On the Dissipation of Energy."
- [19] Thomson and Tait's *Natural Philosophy*, § 536 (4).
- [20] Thomson and Tait's *Natural Philosophy*, § 501 and § 480 (*e*).
- [21] *Ibid.*, § 480 (*b*) and (*a*).
- [22] In stating large masses, if English measures are used at all, the ton is convenient, because it is 1000 kilogrammes nearly enough for many practical purposes and rough estimates. It is 1016.047 kilogrammes; so that a ton diminished by about 1.6 per cent. would by just 1000 kilogrammes.
- [23] It seems hopeless, without waiting for some centuries, to arrive at any approach to an exact determination of the amount of the actual retardation of the earth's rotation by tidal friction, except by extensive and accurate observation of the amounts and times of the tides on the shores of continents and islands in all seas, and much assistance from *true* dynamical theory to estimate these elements all over the sea. But supposing them known for every part of the sea, the retardation of the earth's rotation could be calculated by quadratures.
- [24] The calculation is simply this. Let E be the earth's whole mass, a its radius, k its radius of gyration before, and k' after the supposed melting of the ice, and W the mass of ice melted. Then, since $(2/3)a2$ is the square of the radius of gyration of the thin shell of water supposed spread uniformly over the whole surface, and that of either ice-cap is very approximately $\frac{1}{2}a^2 (\sin 20°)^2$, we have

$$\mathrm{E}k'^2 = \mathrm{E}k^2 + \mathrm{W}a^2 [2/3 - \tfrac{1}{2}(\sin 20°)^2].$$

And by the principle of the conservation of moments of momentum, the rotatory velocity of the earth will vary inversely as the square of its radius of gyration. To put this into numbers, we take, as above, $k^2 = (1/3)a^2$ and $a=21\times10^6$. And as the mean density of the earth is about 5½ times that of water, and the bulk of a globe is the area of its surface into 1/3 of its radius,

E : W :: 55*a* / 3 : .066

- **[25] "Secular Cooling of the Earth,"** *Transactions of the Royal Society of Edinburgh*, 1862; and *Philosophical Magazine*, January, 1863