A new departure in science : being a second edition of a new chapter in the story of nature.

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A new Departure in Science

Sheet 9

A NEW DEPARTURE IN SCIENCE,

BY

CHARLES BLAND RADCLIFFE, M.D.,

BEING A SECOND EDITION OF

A NEW CHAPTER IN THE STORY OF NATURE.

"Non trita auctoribus via."

C. Plinii Sec.,
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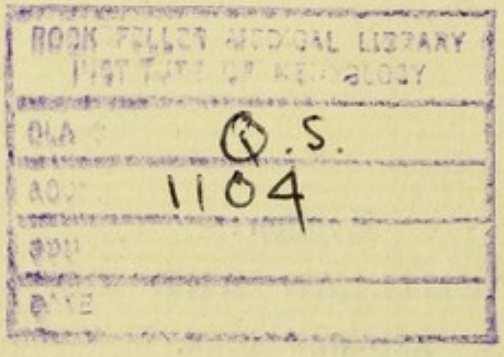

PREFACE.

VERY shortly after the publication of the "Vestiges of Creation," at the request of the editor of a periodical of some importance, I wrote a short review of the book which was not allowed to find its way into print because in it I objected to the conclusions of the anonymous author as intrinsically unsound.

I objected to the "Nebular Hypothesis" because the view of heat upon which it is based is inconsistent with that view of heat of which a sketch had just then been made in lines of light by Mr. (now Mr. Justice) Grove in the first edition of his great work on the "Correlation of the Physical Forces." I could not believe that heat in any case was a separate and independent energy: I could only believe that it was a mere mode of some central force of which light and electricity and magnetism and physical force in other aspects were also modes. I also saw dimly reasons for believing that eventually the source and spring of such a central force would be found to be in an interaction, electrical in its nature, between the earth on the one hand and the heavenly bodies generally on the other, and that, for want of

this interaction, the universe was likely to be cold and dark and forceless generally as well as formless before the day when the earth and the various heavenly bodies were formed and placed in position.

I objected to the doctrine of evolution in organic beings because the grand old law of unity in diversity and of diversity in unity seemed to me to be the law of laws in nature. I looked upon organic beings as invariable when left to themselves, and only slightly variable when the conditions of existence were modified naturally or artificially. Even in any so-called rudiment I could only see that which was perfectly fitted for the organism or organ in that particular state of things. In man I found a *raison d'être* for evolution in the shape of manifold imperfection; elsewhere I could only see a manifest state of perfection in which evolution must necessarily lead to confusion.

I also objected to the doctrine of organic evolution because I saw several reasons for believing that the past history of the earth did not reach back far enough to allow time for evolution to do its work. The wondrous state of preservation in many of the organic remains preserved in the fossiliferous rocks was to my mind a certain proof that natural decay had not had time to do its work before these remains were safely buried out of

reach, and in several instances the coal-seams only served to give point to the same conclusion, for certain coal-seams, with which I happened to be familiar, were so strictly parallel with the rocky strata above and below them, which strata were evidently the product of drifts, as to make it highly probable that the coal-seams themselves, which were as evidently stratified, had been produced in the same way. In point of fact, I found here a cogent reason for disbelieving in the extreme antiquity of the earth, for if the coal-seams were formed of drifts of vegetable matter, of which several may have been formed in a comparatively short time in the same period of submersion, there is then no longer any reason to believe that every coal-seam marks a time in which the land had been first upheaved and then submerged, with an interval between upheaval and submersion long enough to allow time for the growth on the spot of the vegetable matter which eventually became coal.

I was then at Paris attending lectures by De Blainville, Etienne St. Hilaire, Brongniart, Serres, Arago, Dumas, and other teachers of great eminence, at the Jardin des Plants, the Observatoire, the Sorbonne, and elsewhere, and around me were several clever friends who were ready enough to talk and to form their own conclusions in this matter, which were not

always opposed to mine. And as it was with me then so it is now, for, notwithstanding all that has been said and done in the forty years intervening between that time and this by Compte and Darwin and Wallace and Lyell and Huxley and Herbert Spencer and many others who have followed in their steps, I am still as incapable of accepting the doctrine of evolution as the key to nature as I was on the day in which I wrote the rejected notice of the "Vestiges of Creation," or in the earlier days in which I became acquainted with what Kant and Lamarck have to say on the same subject.

How I came to think as I do I can scarcely tell. Indeed, all I know is that light has dawned very slowly, and that, though not yet day, I am at length able to see, not indistinctly, that there is a tidal wave in the land, dependent upon tidal fluctuations in subterranean heat, which is an important factor in the production of the tides,—that subterranean heat must be referred to an action of the sun and moon ever operative,—that subterranean and other forms of natural heat have their source in an interaction between the earth and heavenly bodies which may be resolved into electrical incandescence, — that the causes leading to the development of subterranean heat, modified in their action in an intelligible manner, may have led to

the great revolution in which the bed of the ancient sea was made to take the place of the present land,—and much besides,—that, in fact, a new departure in science is necessary. I have something to say about evolution, but only incidentally, for, in reality, I get beyond this question. Indeed, in widening the domain of physical law as, I think, must be done, I am obliged to believe that the earth is not old enough to allow of the workings of evolution, and that, in fact, the actual and scriptural history of the earth are not wholly irreconcilable.

C. B. R.

25, CAVENDISH SQUARE,
June, 1886.

INTRODUCTORY TO THE PRESENT EDITION.

WHILE leaving the latter half of the book unaltered, or nearly so, I have, in this edition, recast the three first chapters which form the other half, making two chapters out of three, entering into the argument at a different point, finding much new light in the new barometrical and thermometrical evidence which is now supplied for the first time, and giving a fresh turn in several important particulars. I have changed the title from "A New Chapter in the Story of Nature," which has been objected to as too fanciful, to "A New Departure in Science," which is looked upon by my critics as more to the point. I have also divided the book into two parts, so that, after reading the first part, any one may stay and recover breath before going on to the second part, which also is calculated to take away the breath, or else make an end there and then.

C. B. R.

25, CAVENDISH SQUARE,
Oct., 1886.

PART I.

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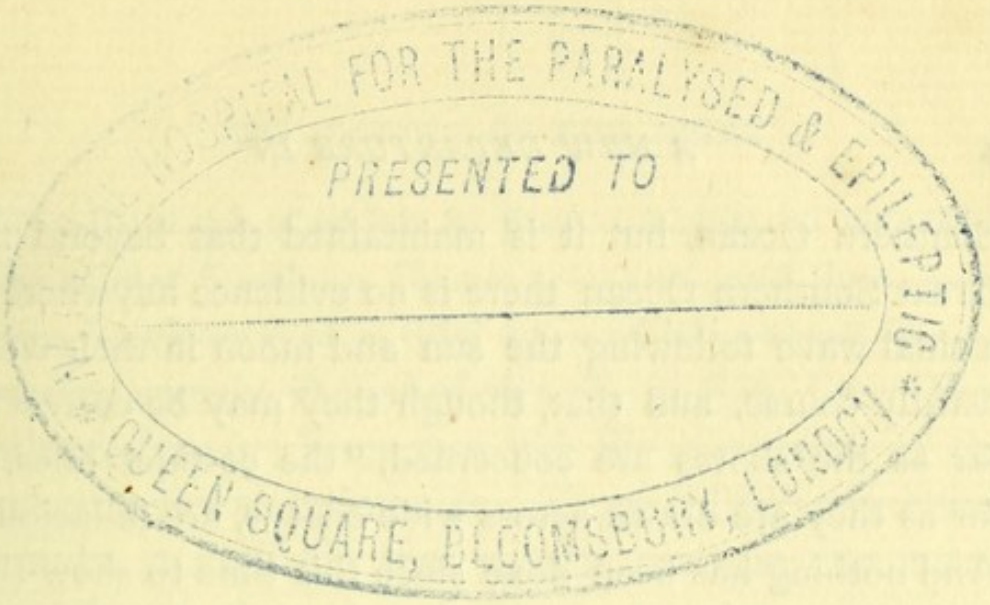
CHAPTER I.

A NEW DEPARTURE IN WHICH A TIDAL WAVE IN THE LAND, DEPENDENT UPON TIDAL FLUCTUATIONS IN SUBTERRANEAN HEAT, IS FOUND TO BE AN IMPORTANT FACTOR IN THE PRODUCTION OF THE TIDES.



THE theory of the tides is a subject upon which it cannot be said that the last word has been said which has to be said. Much has been said, by Sir John W. Lubbock and Dr. Whewell more especially, but it is a significant fact that after finishing his long and laborious "Researches on the Tides," Dr. Whewell wrote a paper "On our ignorance of the Tides," in which he substantially abandons nearly all that for which he had been contending. No doubt is expressed as to the accuracy of Sir John W. Lubbock's theory upon which the map of cotidal lines is framed, which view he had adopted. It is still assumed that the "cradle of the tides" is in the Great Antarctic Ocean—that it is only in this ocean, where there is a ring of deep water uninterrupted by cross-barriers of land, that the attraction of the sun and moon can tell in producing a tidal wave in the water—that, in fact, the seas elsewhere, owing to the presence of these cross-barriers of land, would be tideless were it not for the overwhelming oscillation of the wave set up by the attraction of the sun and moon in the Great

Southern Ocean, but it is maintained that beyond the Great Southern Ocean there is no evidence anywhere of a tidal wave following the sun and moon in their westwardly course, and that, though they may be correct so far as the shores are concerned, "the co-tidal lines, so far as they are drawn across wide oceans, are fallacious." And nothing has been done since this time to show that our ignorance is less than it was then stated to be. Indeed, the factotum of our visionary friend Theophrastus Such is not altogether wrong in replying to the question "What is the cause of the tides, Pummel?" by saying, "Well, Sir, nobody rightly knows, but if I was to give my opinion, it 'ud be different."

The more I look into the matter the more inclined am I to think that there is no reason for believing that the Antarctic Ocean is the "cradle of the tides," and but scant reason for supposing that the tides anywhere are caused by the attraction of the sun and moon.

The facts do not show that there is a great tidal wave in the Antarctic Ocean which with certainty can be said to be due to the attraction of the sun and moon. At the three great land-ends abutting into this ocean, and at Kerguelen's Land, which island is the very midst of these southern waters, the time and rise of the tide at spring tide are :—

	Time.	Rise.
At Cape Horn..	4·30 ..	9 feet
At Cape of Good Hope ..	9.0 ..	6·6 „
At S.W. Cape, New Zealand	Noon .	7 „
At Kerguelen's Land.. ..	2.0 ..	2 „

The facts out of which to form a history of the tides in the Great Southern Ocean are these, and these only. There is evidence of a tidal wave which, except in one instance, namely, in that of the tide at S.W. Cape, New Zealand, does not keep pace with the movements of the sun and moon at spring tide. There is little or nothing to make it probable that a tidal wave in the water, raised by the attraction of the sun and moon, is following the sun and moon in a westwardly direction as it ought to do. And most assuredly the facts do not justify the notion that there is in this ocean that tremendous tidal wave in the water which, with little or no regard to the attraction of the sun and moon, by oscillation simply, or chiefly, is capable of setting up a tidal wave in distant waters otherwise tideless. In fact, the rise of the tidal wave here is considerably less than the average rise of the tidal wave in other places. And certainly there is nothing in the history of the tide in the Antarctic Ocean to make it appear that the sun and moon are acting upon the Antarctic Ocean in a way which is peculiar to it,—that the attraction of the sun and moon is telling upon this ocean, and upon this ocean only.

Nor is it quite certain that the sun and moon are acting upon the tides in accordance with the Newtonian theory either in the Antarctic Ocean or elsewhere. There is, indeed, no conclusive evidence to this effect anywhere. At a comparatively short distance from shore a ship ceases to swing with the tides, and, speaking broadly, there are no tides to speak about at small islands in mid-ocean, where, as might be expected, the attraction

producing the tides would operate most powerfully. In point of fact, it is only around continents and wide expanses of land, most of all around continents, that the tides generally are of any considerable magnitude. Yielding to the attraction of the sun and moon it is to be expected that the waters would move westward and beat most forcibly on the eastern shores, but it is not so in reality, for though the water rises more on the east coast than on the west in Australia, in Africa, and in the Bay of Bengal, it rises more on the west than on the east in England and Wales, in Ireland, and in Madagascar, while in North and South America the rise on the east and west coast is identical. There is evidence of currents setting almost in any direction, and almost always in the same direction, but these currents are not themselves tidal except in the immediate neighbourhood of the coasts, and, in fact, it is difficult to know where to turn in order to find any certain evidence of a tide following the course of the sun and moon in a western direction in obedience to the law of attraction. Be the explanation what it may, so it is. And this difficulty is intensified when the times of the tides is looked into more closely, for these times are almost regardless of the clock, sometimes following, sometimes preceding the passage of the sun and moon over the meridian, now at longer intervals, then at shorter, as it may happen. And still more so when the mean of these times is taken. At springtide the sun and moon, in conjunction or opposition, are on the meridian at noon and midnight, and it is to be expected that the mean time of high water then would be at noon or midnight,

or thereabouts, *but instead of this the tide then is at high water at* 6.15. In point of fact, the mean time of high water at spring tide is as far as it can be from the time at which it ought to be theoretically. This is the result arrived at when the mean time of spring tide is taken from the times of spring tide in all parts of the world, 3,040 in number, which are given in the Official Tide Tables; and be the explanation what it may the fact is not to be called in question. The mean time of the spring tide is exactly what it ought not to be if it is to be in accordance with the Newtonian law of attraction. This is evident. And this difficulty as to the time of the tide is not lessened by reflecting on the rise of the tides. The tides are sometimes high and sometimes low, with little regard to rule as it would seem. At all events, the high tides in the Bay of Fundy and in the Bristol Channel are not to be explained by supposing that in these cases a tidal wave is packed up, as it were, in a funnel-shaped channel with its mouth to the sea, for it is found that the tides are equally high at Point Gallegos, far south of the River Plate, where the tides are barely perceptible, and yet in both cases the seabord is quite open, and, so far as can be seen, not different in conformation from other open coasts where the tides are quite of ordinary height.

What then? Is there any way out of this difficulty? Does such a way open out in the direction of two new facts to which I wish to direct attention, the one fact pointing to changes in atmospheric pressure at high water and low water afloat and ashore, which changes

show that the level of the land as well as the level of the water is in tidal movement, the other indicating fluctuations in subterranean heat which may carry with them tidal movements in the land, and be an all-important factor in the production of the tides? In this direction there is, I think, such a way; at all events, I think that this way is more or less open, and that a good working hypothesis may be found by making use of it.

I was at Eastbourne when I began to work in the direction which led to the discovery of the first fact. As it happened I could not keep my eyes away from the waves which were ever beating under my windows. When not lost in the enjoyment of their beauty I was struck by the way in which, whether the tide was coming in or going out, or whether it was in or out, they always came towards the shore. It seemed as if they were doing obeisance to the land. It seemed as if they were attracted, or in some other way strongly influenced, by the land. I could not get the land out of my head as having to do with them primarily; and question followed question which I did not know how to answer satisfactorily. Is there a tidal wave in the land which over-rides the tidal wave in the water? Is the tidal wave in the land due to tidal fluctuations in subterranean heat dependent upon an action of the sun and moon? Do the senses deceive us here as they did before the day when Copernicus won his laurel crown? I distinctly saw that a tidal wave in the water was the necessary accompaniment of gravitation; but the idea of a tidal wave in the land had got possession of my mind, and I

could not help but think that this was likely to prove a much more important factor in the production of the tides than any tidal wave in the water. Under these circumstances, intending to see what the atmospheric pressure really was at high water and at low water afloat and ashore, I took a very sensitive aneroid and paid more than one visit to the end of the pier at high water and at low water. I saw that the atmospheric pressure must increase if the land or water went down, and decrease in the contrary case. I saw that I should have no difficulty in detecting this movement, for the tidal rise and fall here was about 20 feet, and the aneroid I had with me was sufficiently delicate to mark plainly a movement of 5 feet up and down, one degree, which might easily have been sub-divided into five or ten, corresponding to ten feet of movement up and down. On the first day I found that the water went up at high water and down at low water, but that, instead of going down and up to the extent of 20 feet, the movement amounted to no more than 6 feet. On the second day, still only attending to the water, I found that there was no perceptible difference in the level of the water at high water and at low water. On the third day, turning from the water to the land, I found, to my great delight, that the *land* went down at high water and up at low water, and that this movement amounted fully to 9 feet. The aneroid was perfectly true, and there could be no doubt as to the fact. And here, unfortunately, the matter ended, for I was called back to town on the next day, and for a long time afterwards I had other matters to attend to which had little regard to the tides.

Later on the work begun in this way was supplemented by work which has been carried out for me, (1), for four months, at Waterloo Bridge, London, on the floating quarters of the Thames Police, and on the landing stage alongside, by Mr. Marler, the superintendent, (2), for one month, at Falmouth, ashore at the Meteorological Observatory, by Mr. Kitto, the superintendent, and afloat on H.M.S. "Ganges," by Captain Bridges, R.N., the captain, and, (3), for a couple of months, at the coastguard station at Little Hampton, by Mr. Auld, the officer in charge, and this work is that about which I have now to speak, premising only that the instruments used, were, at Falmouth, the standard mercurial barometer of the Observatory for the observations ashore, and the ordinary regulation aneroid of the ship for the readings afloat, neither instrument being removed from the place at which it rested ordinarily, and, at Waterloo Bridge and Little Hampton, an aneroid, made expressly for the purpose, of which one degree marked 10 feet of motion up and down. The observations—for the making of which I am much indebted to my coadjutors, to Mr. Auld especially, who was so interested in the work as to see to it himself night and day for some time—were made ashore and afloat at the same spots at every tide: the means, to which I have reduced these observations, are those which are set forth in the following tables:—

TABLE I.

THE Mean of Readings of Barometer at the same spots afloat and ashore at Waterloo Bridge, London, at each tide from 2nd May to 31st May, 1882.

High Water.		Low Water.	
Afloat.	Ashore.	Afloat.	Ashore.
30·284	30·292	30·293	30·276

TABLE II.

THE Mean of Readings of Barometer at the same spots afloat and ashore at Waterloo Bridge, London, at each tide from 24th June to 23rd July, 1883.

High Water.		Low Water.	
Afloat.	Ashore.	Afloat.	Ashore.
30·210	30·203	30·219	30·197

TABLE III.

THE Mean of Readings of Barometer at the same spots afloat and ashore at Waterloo Bridge, London, at each tide from 29th July to 30th August, 1885.

High Water.		Low Water.	
Afloat.	Ashore.	Afloat.	Ashore.
30·418	30·418	30·434	30·410

TABLE IV.

THE Mean of Readings of Barometer at the same spots afloat and ashore at Falmouth at each tide from 1st November to 30th November, 1883.

High Water.		Low Water.	
Afloat.	Ashore.	Afloat.	Ashore.
29·885	29·881	29·905	29·888

TABLE V.

THE Mean of Readings of Barometer at the same spots afloat and ashore at Little Hampton at each tide from 9th August to 9th October, 1886, the Mean of each week being given separately.

	High Water.		Low Water.	
	Afloat.	Ashore.	Afloat.	Ashore.
(1)	30·419	30·441	30·433	30·419
(2)	30·600	30·596	30·625	30·612
(3)	30·560	30·558	30·573	30·557
(4)	30·528	30·530	30·557	30·544
(5)	30·506	30·508	30·513	30·497
(6)	30·613	30·609	30·618	30·605
(7)	30·456	30·454	30·456	30·440
(8)	30·164	30·161	30·176	30·165
	30·480	30·479	34·493	30·479

TABLE VI.

SUMMARY of the Mean of Readings of Barometer afloat and ashore in the five preceding Tables.			
High Water.		Low Water.	
Afloat.	Ashore.	Afloat.	Ashore.
(1) 30·284	30·292	30·293	30·276
(2) 30·210	30·203	30·219	30·197
(3) 30·418	30·418	30·434	30·410
(4) 29·885	29·881	29·905	29·888
(5) 30·480	30·479	30·493	30·479
30·255	30·255	30·268	30·250

On looking at these tables the first point to which attention may be directed is the difference in atmospheric pressure *ashore* at high water and low water. The supposition is that the level of the land is stationary, and that, provided there be no changes in the six hours between high water and low water, produced by the action of the winds or in any other way, the atmospheric pressure over the land at high water and low water will be the same. And what is the actual case? It is that the land has gone down to 30·255 at high water, and up to 30·250 at low water. It is that there is low land with what seems to be high water, and high land with what

seems to be low water. This is the simple case. And so far as I can see, there is no way of accounting for it except on the supposition that the land is moving up and down in a tidal wave, going down when it is high water, going up when it is low water. In a word, the evidence of a tidal wave in the land, with these peculiarities, seems to be fairly conclusive.

With the barometric readings belonging to high and low water *afloat* it is more difficult to come to a conclusion, but it is easy to see that the facts may be accounted for without having recourse to the explanation which now passes current. The atmospheric pressure over the water is 30·255 at high water, and 30·268 at low water. It seems as if the level of the water had gone up at high water and down at low water. There is a difference of ·013, or 13 feet, between high water and low water : there is a difference which is considerable, but which is not sufficient to account for the rise and fall of the tide at the spot, which is fully 20 feet. But it does not follow as a matter of course that the water has gone up and down even to this extent, for it may be that the water merely remains at its own level, or moves only so far as to regain this level—that, in fact, the level of 30·255 is as much the natural level of the water for high water, as the level of 30·268 is the natural level of the water for low water.

The observations at Waterloo Bridge differ considerably from the observations at Falmouth and at Little Hampton, and at first sight there appears to be little agreement between them.

At Waterloo Bridge the barometric readings show

very plainly that the atmospheric pressure *ashore* is decidedly greater at high water than at low water, the readings being 30·304 at high water and 30·294 at low water—a difference showing that the atmosphere *over the land* is heavier to the extent of as much as ·010, or ten feet, at high water. The case, indeed, is that which is here tabulated.

Table VII.

The Mean of Readings of Barometer at the same spots afloat and ashore at Waterloo Bridge, London, at each tide during four months.

High Water.		Low Water.	
Afloat.	Ashore.	Afloat.	Ashore.
30·304	30·304	30·315	30·294

At Falmouth, the atmospheric pressure *ashore* is increased where it ought to be decreased, and *v. v.*, being increased at low water and decreased at high water, thus :—

TABLE VIII.

THE Mean of Readings of Barometer at the same spots afloat and ashore at Falmouth at each tide during two months.			
High Water.		Low Water.	
Afloat.	Ashore.	Afloat.	Ashore.
30·205	30·206	30·226	30·210

At Little Hampton, on the other hand, the atmospheric pressure *ashore* is the same at high water and at low water, thus :—

TABLE IX.

THE Mean of Readings of Barometer at the same spots afloat and ashore at Little Hampton at each tide from August 9th to October 9th, 1886.			
High Water.		Low Water.	
Afloat.	Ashore.	Afloat.	Ashore.
30·480	30·479	30·493	30·479

Instead of going down at high water and up at low water, as at Waterloo Bridge, it seems as if the land has

gone up at high water and down at low water at Falmouth, and as if at Little Hampton it remained at the same level, but so far from this being the case it is very possible that in the six hours between high water and low water, there may have been changes in atmospheric pressure, produced by the action of the winds, and in other ways, which altogether over-ride the changes produced simply by the up and down movements of the land. Nay, it appears to be so, for when the barometric readings at Little Hampton, which are divided into weeks, are looked into, it is found (Table V.) that in five out of the eight weeks the atmospheric pressure *ashore* is, as it is at Waterloo Bridge, higher at high water than at low water, being 30·514 at high water and 30,505 at low water, a difference of ·009, or 9 feet, which is very nearly the same as at Waterloo Bridge, and that this state of things is departed from only in three weeks out of eight, so that the evidence at Waterloo Bridge, which takes in a period of four months out of seven, must be amplified by adding to it five out of eight of the weeks at Little Hampton, which is more than another month.

No doubt additional evidence is wanted before it is possible to lay down the law positively; but it is, I think, not too much to say that the evidence, so far as it goes, is in favour of the notion that *ashore* there is diminished atmospheric pressure at low water, and increased atmospheric pressure at high water,—that, in fact, the land is moving up and down in a tidal wave.

Nor is it to be wondered at that the action of the sun and moon, which is here quite unmistakable, should be as deep-seated and as instantaneous as it is found to be.

For, as will be seen in the sequel, the simple fact is that the earth is traversed instantaneously by electrical currents from the sun and moon,—that these currents in certain parts of their course across the earth are, as instantaneously, frittered down into heat by resistance, —and that this action, in consequence of the current passing directly across the earth from the sun and moon, is manifested on opposite sides of the earth at the same time, so that a tidal wave in the land in any of its stages here is repeated at the antipodes by a tidal wave in the same stage.

And, to return, it may not be wrong to conclude that the evidence in favour of a tidal wave in the land is even more conclusive than the evidence of a tidal wave in the water.

The second fact to which I wish to direct attention is the complement of the first. Seeing that fluctuations in subterranean heat were likely to be concerned in the production of the tidal wave in the land, and that these fluctuations, if present, would, to a certainty, manifest themselves in a thermal spring, I went down to Bath, and, with a thermopile and the automatic apparatus necessary for getting photographic tracings of the movements of the needle of the galvanometer connected therewith, I set to work to investigate the heat-history of one of the hot springs. The thermopile consisted of two couplets of stout German silver and iron wire, each couplet being about 17 feet in length; the rest of the apparatus had nothing peculiar about it. Properly insulated by indiarubber tubing, and properly connected,

the face of the thermopile, or thermo-couplet, was passed obliquely downward into one of the hot springs from a darkened room in its vicinity, and, this being done, the other end, which is out of the water, was connected with the galvanometer and the automatic apparatus in the usual way. Everything is going on satisfactorily now, but it was not always so. Indeed, for some time all went wrong, and, in fact, nothing would have gone right had it not been for the help of my friends Major Davis and Mr. Alderman Wilkinson, both of whom happened to be closely connected with the administration of the thermal waters—of Major Davis, more especially in mastering the many local difficulties which stood in the way of getting at the hot water—of Mr. Alderman Wilkinson, in co-operating with Major Davis most perseveringly, and also, when other means had failed, in taking upon himself to keep the automatic and other apparatus in working order, and, what was of still greater moment, to act as a photographer.

The tables which follow give the results of the photographic tracings, taken by Mr. Alderman Wilkinson, of the behaviour of the thermopile, or thermo-couplet, in one of the hot springs at Bath on the days of spring tide and neap tide for eight lunations, these days being selected as those in which any differences in the action of the sun and moon upon subterranean heat—it was assumed that the sun and moon did so act—would be most conspicuous. Certain hours, for reasons which will appear presently, are selected, namely, midnight, 6 a.m., noon, 6 p.m., and midnight again. The mean of these hours is first given. Then the mean of these

means is taken, first at midnight, noon, and midnight jointly, and then at 6 a.m. and 6 p.m. jointly. The mean time of high water at spring tide is supposed to be about 6 a.m. and 6 p.m., because this, I find, is the mean time of high water at spring tide all over the world as taken from the places, 3,040 in number, of which an account is given in the Official Tide Tables published by the Lords of the Admiralty. The mean time of high water at neap tide is supposed to be about midnight, and noon, and midnight, because the mean time of high water at neap tide is about six hours before or after the mean time of high water at spring tide. The case, indeed, is plain enough. If the mean time of high water at spring tide be at 6 a.m. and 6 p.m., the mean time of low water at spring tide will be at midnight, noon, and midnight. If the mean time of high water at neap tide be about midnight, noon, and midnight, the mean time of low water at neap tide will be about 6 a.m. and 6 p.m. There are, indeed, two sets of means with which I have specially to do, the one bringing together midnight, noon, and midnight, and being necessarily the same for each of these hours; the other connecting 6 a.m. and 6 p.m., and being as necessarily the same for either of these hours. And, for the rest, all I have to say is that the tracings are reduced to rule by using an arbitrary scale of which one division is found to be equal, approximately, to $\frac{1}{10}$th of a degree Fahrenheit, and that thus I am able to add to the tables tracings giving the results in degrees Fahrenheit.

TABLE X.

● THE Mean of Readings of the Thermopile in one of the Hot Springs at Bath on the day of Spring Tide at New Moon.

	MIDNIGHT	6 A.M.	NOON	6 P.M.	MIDNIGHT
16 March, 1885 ...	15·4	14·4	14·2	14·6	14·4
15 April ,, ..	11·5	11·	11·7	11·8	11·8
14 May ,, ...	17·5	17·3	18·3	18·	17·7
8 Sept. ,, ...	8·4	7·4	7·7	7·	7·3
8 October ,, ...	16·	15·6	15·8	16·2	16·6
4 April, 1886 ...	10·4	10·6	9·7	9·2	8·4
4 May ,, ...	5·	6·1	5·	3·8	4·1
2 June ,, ...	4·8	5·5	5·	4·7	4·5
	11·12	10·98	10·92	10·66	10·6

Mean at Midnight, Noon, and Midnight ... ... 10·88
Mean at 6 a.m. and 6 p.m.... ... ... 10·82

	MIDNIGHT	6 A.M.	NOON	6 P.M.	MIDNIGHT
Thus:	10·88	10·82	10·88	10·82	10·88

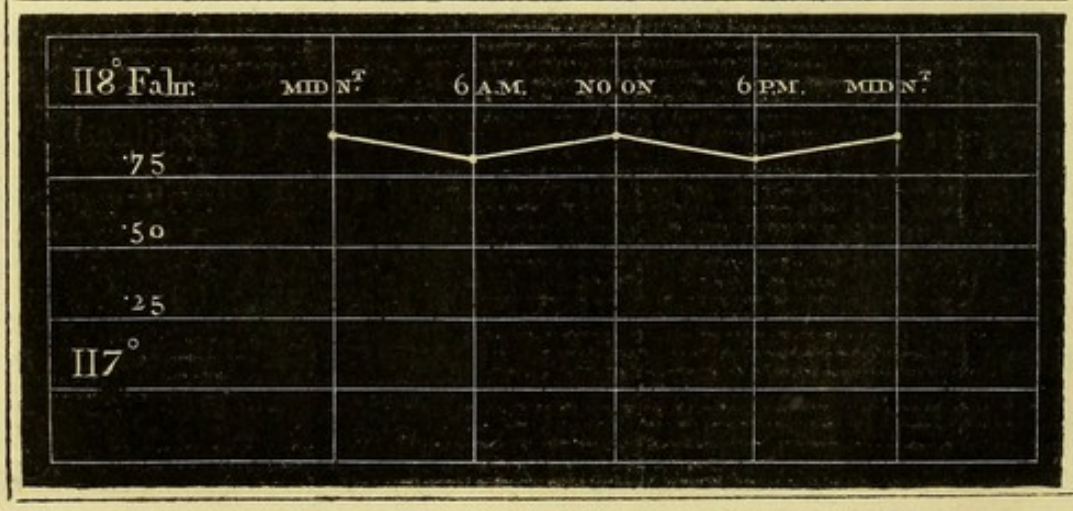


TABLE XI.

☽ THE Mean of Readings of the Thermopile in one of the Hot Springs at Bath on the day of Neap Tide at the 1st Quarter.

	MIDNIGHT	6 A.M.	NOON	6 P.M.	MIDNIGHT
23 March, 1885 ...	11·2	10·6	11·	11·2	10·8
21 April ,, ...	15·7	15·8	16·8	17·7	
21 May ,, ...	17·5	17·6	17·9	18·	17·9
16 Sept. ,, ...	6·5	6·5	6·3	6·1	5·8
16 Oct. ,, ...	15·7	15·8	16·3	16·7	16·9
11 April, 1886 ...	15·5	15·8	15·6	15·7	15·
11 May ,, ...	·5	·5	·6	1·	1·3
9 June ,, ...	1·6	2·	1·6	1·3	1·6
	10·52	10·57	10·76	10·96	9·90

Mean at Midnight, Noon, and Midnight ... ... 10·41
Mean at 6 a.m. and 6 p.m. ... ... 10·76

	MIDNIGHT	6 A.M.	NOON	6 P.M.	MIDNIGHT
Thus:	10·41	10·76	10·41	10·76	10·41

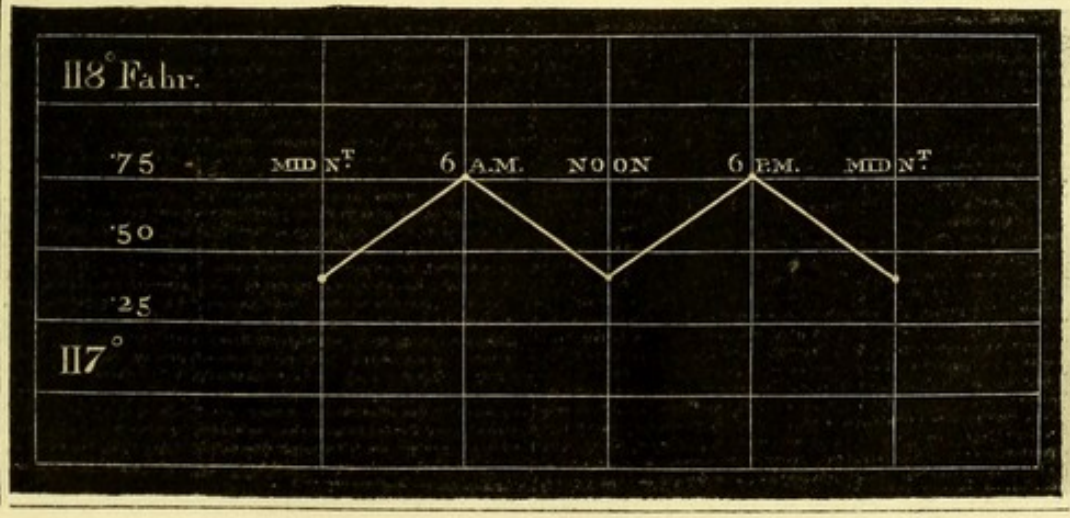


TABLE XII.

○ THE Mean of Readings of Thermopile in one of the Hot Springs at Bath on the day of Spring Tide at Full Moon.

	MIDNIGHT	6 A.M.	NOON	6 P.M.	MIDNIGHT
30 March, 1885 ...	11·	10·9	11·4	11·8	18·8
29 April ,, ...	17·7	17·6	18·2	15·3	18·
28 May ,, ...	19·7	19·6	20·8	20·7	20·5
24 Sept. ,, ...	4·6	4·	4·2	4·	3·8
23 Oct. ,, ...	17·5	17·7	17·7	17·8	18·2
18 April, 1886 ...	13·	13·1	12·6	12·	11·3
18 May ,, ...	6·	6·1	6·	5·7	6·
16 June ,, ...	3·5	3·6	3·7	3·2	3·
	11·62	11·57	11·82	11·31	11·57

Mean at Midnight, Noon, and Midnight ... ... 11·67
Mean at 6 a.m. and 6 p.m. ... ... 11·44

	MIDNIGHT	6 A.M.	NOON	6 P.M.	MIDNIGHT
Thus:	11·67	11·44	11·67	11·44	11·67

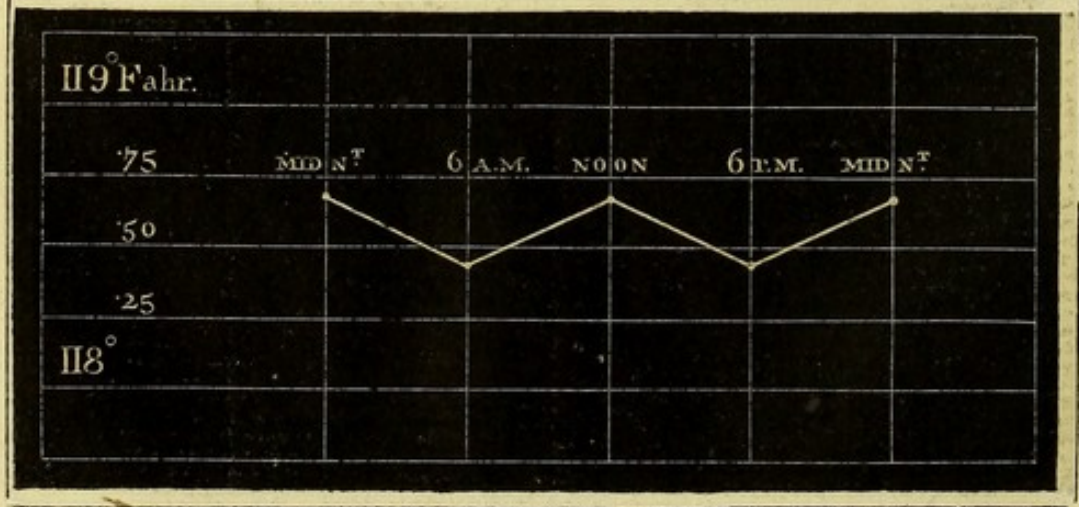


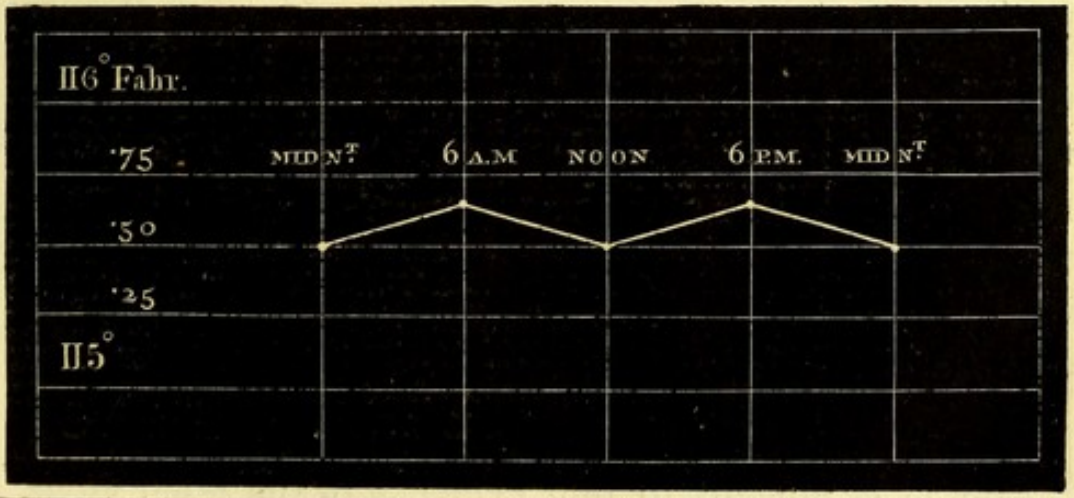
TABLE XIII.

⊙ THE Mean of Readings of Thermopile in one of the Hot Springs of Bath on the day of Neap Tide at the 2nd Quarter.

	MIDNIGHT	6 A.M.	NOON	6 P.M.	MIDNIGHT
7 April, 1885 ...	10·3	10·6	10·4	10·8	10·5
7 May ,, ...	18·5	18·8	19·	18·8	18·3
1 October ,, ...	1·	·8	1·	1·	1·
30 October ,, ...	17·	16·8	16·7	17·	17·3
26 April, 1886 ...	4·4	5·5	4·7	4·	4·3
25 May ,, ...	6·5	6·9	6·4	5·6	5·3
24 June ,, ...	2·6	2·9	2·	1·6	1·5
	8·61	8·9	8·6	8·4	8·11

Mean at Midnight, Noon, and Midnight ... ... 8·50
Mean at 6 a.m. and 6 p.m. ... ... 8·65

	MIDNIGHT	6 A.M.	NOON	6 P.M.	MIDNIGHT
Thus:	8·50	8·65	8·50	8·65	8·50



The facts here are plain enough. At spring tide the mean temperature of subterranean heat is higher at midnight, noon, and midnight, than it is at 6 a.m. and 6 p.m. At neap tide the mean temperature of subterranean heat is higher at 6 a.m. and 6 p.m. than it is at midnight, noon, and midnight. As regards heat, the two cases are quite opposed to each other. There can be no doubt that there are tidal fluctuations in subterranean heat dependent upon the action of the sun and moon. There can be no doubt that there are causes at work which are calculated to alter the level of the land—that at spring tide the land will rise at midnight, noon, and midnight, and fall at 6 a.m. and 6 p.m., and that at neap tide this state of things will be reversed—that, in fact, at spring tide there will be high land and low water at midnight, noon, and midnight, and low land and high water at 6 a.m. and 6 p.m., and that at neap tide there will be low land and high water at midnight, noon, and midnight, and high land and low water at 6 a.m. and 6 p.m. These are the simple facts, and these are the inferences which would seem to be fairly deducible therefrom.

And so it is that I am at liberty to say, not only that the days of spring tide and neap tide are marked by differences in the development of subterranean heat which are unintelligible unless it be supposed that the sun and moon are directly concerned in producing them, but also that these differences may give rise to a distinct tidal wave in the land which is strangely in agreement with the true tidal wave at spring tide and neap tide.

Nor is it difficult to find evidence to the same effect

in the exceptionally high and low tides which are met with here and there.

By marking the coasts where the tides are exceptionally high and low on a good geological map of the world it may be seen at a glance that the geological construction of the coast where the rise of the tide is exceptionally high is not the same as that of the coast where this rise is exceptionally low. The coast where the rise is high, so far as is known, is made up of primary rocks, of carboniferous rocks chiefly. The coast where this rise is low, is made up of cretaceous and comparatively modern rocks of various sorts—of rocks belonging to the post-tertiary, tertiary, and later secondary series. Upon such a map, so marked, after what I have said, I find it difficult to look without coming to the conclusion that these exceptionally high and low tides may be due, not to differences in the rise and fall of a tidal wave in the water, but to differences in the rise and fall of a tidal wave in the land, which are themselves dependent upon peculiarities as regards expansibility in different sorts of rocks under the action of tidal movements in subterranean heat. This conclusion, as it seems to me, arises not unnaturally out of the premisses, and, so far as I know, there is no valid objection to it. At all events, the current explanation of some of the tides under consideration cannot be looked upon as satisfactory. There are many bays and channels in which the tide should be as high as it is in the Bay of Fundy or Bristol Channel if the rise of the tide in these two instances be really caused by the packing-up, as it were, of a tidal wave in a funnel-shaped channel

with its mouth to the ocean. Moreover, the tide is nearly as high as it is in these two well-known instances in several places where there is nothing to interfere with the ordinary play of the oceanic tidal wave, as at Point Darwin and Pierce Point on the N.W. coast of Australia, and at Point Gallagos on the E. coast of Patagonia. Nor is it enough to say that the tides in the Mediterranean and Red Sea are exceptionally low because the doorway between the ocean and the sea is too narrow to allow the oceanic tidal wave to pass in and out freely, for, in fact, tides equally low are met with in places which are as open as they well can be to the ingress and egress of the oceanic tidal wave, as at Cape Palmas and Cape Three Points on the west coast of Africa, at Aulopolai, Negapatam, and Madras on the east and west coasts of Hindoostan, at Point S. Julian low down on the west coast of South America, and at Cape Florida at the northernmost end of the Bay of Mexico. In a word the only way of accounting for these exceptionally high and low tides which I can find is one which leads me to believe that I have still to do with a tidal wave in the land rather than with a tidal wave in the water—that some rocks expand and shrink more than others under the action of tidal fluctuations in subterranean heat—and that a day may come in which the rising and falling of the tidal wave may give the key to the geological construction of the seabord—when, for example, an exceptionally high tide may be looked upon as pointing, possibly, to the presence of coal at no great distance from the sea in that locality.

Again: it may be that certain annual variations in the rise of the tides may have to be interpreted in the same manner. At Holyhead, at London, and at Liverpool the level of the tides is considerably higher in December and in January than it is in the summer months. At the mouth of the Swan River in Western Australia the greatest range of any one month very rarely exceeds 3 feet, but the annual range reaches to 5 feet, the highest level being reached in the coldest months of the year. There are also indications of the sea-level being higher in January than in June at Esquimalt in Vancouver's Island, and also in the central parts of the Red Sea, where, as is shown by the exposure of coral reefs, the sea-level appears to be fully 2 feet lower in the summer than in the winter months. Sir Frederick Evans, R.N., the Hydrographer General of the Royal Navy, who directs attention to these facts, also points out "that the waters of the southern hemisphere attain a higher level at the period of the year when the sun is to the north of the equator, and that the northern waters are highest when the sun is to the south of the line." It is indeed very possible that here also an additional reason may be found for believing that the action of the sun and moon in causing the tides may be upon the land rather than upon the water, for in order to account for the facts under consideration all that is necessary is to suppose that the water is higher in winter than in summer because the development of subterranean heat which raises the land above the waters is less energetic in the winter months

of the year than in the summer months. So it may be. And in fact it is more easy to entertain this notion than to believe that these differences in the level of the water are to be explained by the water being attracted towards the sun and moon more at one time than at another, for any conclusion which may be drawn from the state of things in the northern hemisphere is, as it seems to me, contradicted in great measure by the contemporaneous state of things in the southern hemisphere, and *v. v.*

Again: it may be that the explanation which has been applied to exceptionally high and low tides may have to be applied to the "establishment of the port." If the geological construction of the earth were the same everywhere it may be supposed that the actual time of high water at spring tide at any place—the time to which the name of "establishment of the port" is given —would be the same at every place on the sea-bord. But not so in an earth of which the geological construction is not the same everywhere. For in this case it may be supposed that the actual time of highwater at spring tide will approximate to the mean time of high water at spring-tide, which is 6.15 o'clock morning and evening, when the land rises and falls *quickly* under the action of subterranean heat, and that the contrary state of things will be met with when the land rises and falls *slowly*.

And there is nothing extravagant in the assumption that this tidal wave in the land may vie in magnitude with any tidal wave in the water, for the experiments of Colonel Totten (reported by Lieut. Bartlett in Silliman's

Journal for 1852) show that the expansion caused by 1° F. is ·0000579 of an inch in a foot of granite, ·000060816 of an inch in a foot of marble, and ·000114384 of an inch in a foot of red sandstone—is enough, that is to say, to elongate the radius of an earth composed of the least expansible of these rocks to the extent of 102 feet. Indeed the heat at my disposal, even though this be no more than two or three degrees Fahrenheit, is more than I know what to do with, unless I be permitted to believe that some of it is expended in filling up vacant spaces of various sorts within the earth.

In watching the tidal dance of the sea upon the shore it is difficult to believe that the reason is here deluded by the senses as it was by the apparent movements of the heavenly bodies before the days of Copernicus. Out at sea, and not very far out, it is less difficult, for there a ship does not swing with the tides as it does at or near the shore. Out at sea, and not very far out, indeed, any evidence of true tidal movement is in fact lost. And what is lost at sea, strange to say, is found far inland, namely at Sécheron, a village on the Lake of Geneva. At all events, I may point to certain observations with a spirit level recently carried out by M. Ph. Plantamour at 9 a.m., 12 m., 3 p.m., 6 p.m., and 9 p.m.,* and say that there are movements of the bubble which show very plainly that in

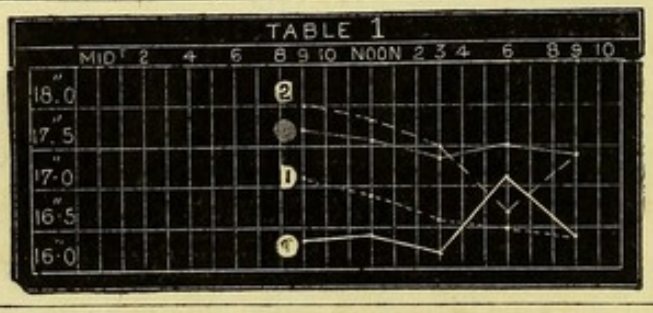
* "Des mouvements périodiques du sol accusés par des niveaux à bulle d'air :" par M. Ph. Plantamour. Archives des sciences physiques et naturelles. Genève, 15 Dec. 1879, and 15 Fev. 1881.

the course of the year the earth rises to a small degree for half the year towards the east, and then falls back to its original level before the year is over, and also that there are every day, towards the east especially, unequivocal risings and fallings of the same sort, the means of the latter movements for each of the days of spring-tide and neap-tide in 1880 being those which are given in this table:—

TABLE XIV.

MEANS of certain readings of the movements of the bubble in a spirit-level on the days of spring tide and neap tide at the undermentioned hours at Sécheron in 1880.

	9	Noon.	3	6	9
●	17·7	17·6	17·4	17·5	17·4
☽	17·1	16·9	16·6	16·5	16·4
○	16·3	16·4	16·2	17·1	16·4
☾	18·0	17·8	17·5	16·7	17·4



The story here is only half-told, and all that can be said of it is that so far as it goes it serves to show that the earth is moving up and down when it is supposed to

be still, and also to make it somewhat less difficult to believe that the tidal wave in the land, about which I have been speaking, is an actual fact, and that the oceanic tides are brought about by the movement of a tidal wave in the land rather than by the movement of a tidal wave in the water, the land really rising and falling when it seems to be still, the water being mainly at rest, or moving only so far as to recover its natural level, when it appears to be rising and falling.

But it does not follow that gravitation has no work to do in this matter. On the contrary it is very evident that a tidal wave in the sea is raised by the attraction of the sun and moon, of the moon especially. That there is such a wave is not to be doubted; but it may be doubted whether this wave can be a wave of any very appreciable magnitude. The force of terrestrial gravity in a second will cause a body to fall 16 feet at the earth, and ·18656 of an inch at the moon, the simple fact being that the fall which takes place in a second here will occupy a full minute at the distance of the moon. In other words, as compared with the attraction of the earth at the earth the attraction of the earth at the moon is as 1 to 3600, or as 1 to the square of 60, as must needs be, for the distance of the moon from the earth's centre is 60 times that of the earth's radius. This may be said about the attraction of the earth for the moon, but not about the attraction of the moon for the earth, for as the mass of the moon is little more than the 90th part of that of the earth, the attraction of the moon for the earth, instead of being as

1 to 3600, will be only the 90th part of 1, or as 1 to 324000. And a doubt as to whether the attraction of the earth is so far over-mastered by the attraction of the sun and moon as to give rise to any very marked tidal wave in the sea is even suggested casually by Newton himself, for after saying ("System of the World," p. 47), "The weight of 1 grain in 4000 is not sensible in the nicest balance," he goes on to say, "The sun's force to move the tides is 12868200 times less than the force of gravity; the sum of the forces of both moon and sun, which forces exceed the sun's force in the ratio of 6⅓ to 1, is still 2032890 times less than the force of gravity; and, this being so, it is evident that both forces together are 500 times less than what is required sensibly to increase or diminish the weight of any body in a balance. And, therefore, these forces will not sensibly move any suspended body; nor will they produce any sensible effect on pendulums, or barometers, or bodies swimming in stagnant water, or in like statical experiments. In the atmosphere, indeed, they will excite such a flux and reflux as they do in the sea, but with so small a motion that no sensible wind will be thence produced." No very marked tidal wave in the atmosphere is indeed produced by the attraction of the sun and moon: that is evident. Nay, it may even be said that, here at least, any such wave is imperceptible. And yet, according to theory, this wave ought to be more perceptible than the wave in the water, for the atmosphere is more mobile than the water. I do not see how it can be otherwise; indeed, all I see is that the absence of a perceptible tide in the atmosphere—which

absence is attested also by the prevailing calmness of the atmosphere—is, after what has been said, an additional reason for believing that there is no perceptible tide in the water ; that, in fact, the apparent up and down tidal movement of the sea is almost incalculably beyond any up and down tidal movement which can be fairly attributed to gravitation. And certainly there is nothing in what Newton says elsewhere to show that it may not be so. It is made out most conclusively that the attraction of the sun and moon must raise a tidal wave in the sea in a particular way, but nothing is said to show that the height of the tidal wave in the sea, modified by certain disturbing forces, is commensurate with the force of gravitation. It is simply taken for granted that it is so. Indeed, the only conclusion to which I can come is that the wave in the water resulting from gravitation may be really as insignificant as the wave in the atmosphere arising from the same cause.

CHAPTER II.

A NEW DEPARTURE IN WHICH SUBTERRANEAN AND OTHER FORMS OF NATURAL HEAT ARE FOUND TO BE DEPENDENT UPON ELECTRICAL INCANDESCENCE, &C.

THE evidence supplied by the thermopile or thermo-couplet at Bath is not the only evidence which plainly goes to show that subterranean heat is affected by the sun and moon. Indeed, evidence to the same effect may be found in the unpublished readings of temperature—for manuscript copies of which I am indebted to the Ex-Astronomer-Royal, Sir George B. Airy—which readings were carried out in 1846 and 1847, night and day, at intervals of an hour, at the Royal Observatory at Greenwich, underground, at the depths of 1 inch, 3 feet 2 inches, 6 feet 4 inches, 12 feet 8 inches, and 25 feet 4 inches, and above ground in the case containing the scales of the "earth-thermometers," and I begin with this evidence, premising only that as at Bath so in this case, and for the same reasons, I have made use only of the days of spring-tide and the days of neap-tide. The facts with which I have to do are those which are set forth in the following tables*:—

* There ought to be a sixth table, namely that belonging to the thermometer giving the temperature at the depth of 6 feet 4 inches, but, owing to frequent gaps in the record the data for the construction of the table are not forthcoming.

TABLE XV.

Means of the *temperature of the earth at the depth of* 1 *inch* at the Royal Observatory at Greenwich for alternate hours on each of the days of spring-tide and neap-tide during five months in 1847.

	12	14	16	18	20	22	Noon.	2	4	6	8	10	12
●	53·30	52·56	51·40	50·84	50·64	51·90	54·52	56·64	57·76	57·60	56·66	55·90	54·60
☽	55·30	54·76	53·90	53·44	53·28	54·40	55·94	58·40	59·50	59·74	58·98	57·80	56·50
○	56·50	55·40	54·72	55·36	55·48	56·48	58·16	59·72	59·94	59·10	57·60	56·88	55·10
☾	54·25	53·71	52·91	52·26	52·61	54·16	56·40	57·73	58·71	58·16	58·01	56·56	55·46

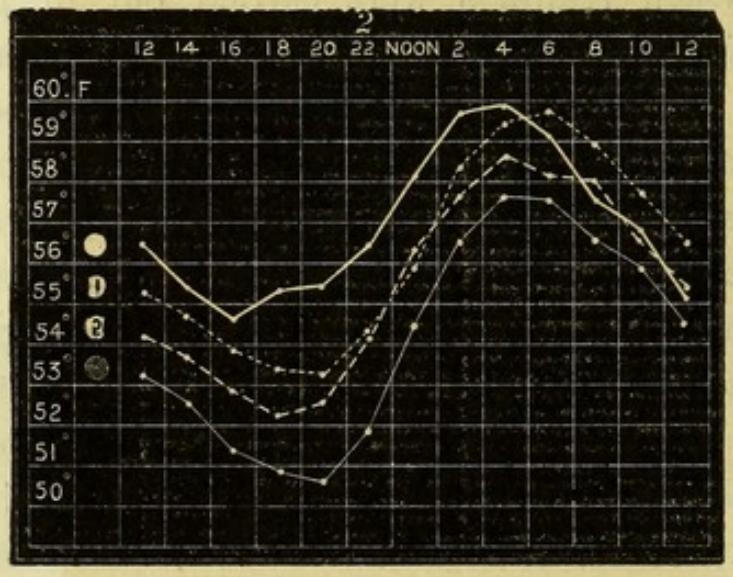


TABLE XVI.

Means of the *temperature of the earth at the depth of* 3·2 *feet* at the Royal Observatory at Greenwich for alternate hours on each of the days of spring-tide and neap-tide during five months in 1847.

	12	14	16	18	20	22	Noon.	2	4	6	8	10	12
●	54·07	54·04	54·03	54·02	54·05	54·09	54·16	54·20	54·20	54·17	54·16	54·09	54·09
☽	54·59	54·57	54·55	54·57	54·59	54·59	54·67	54·70	54·74	54·69	54·67	54·61	54·62
○	55·04	55·05	55·03	55·03	55·05	55·11	55·18	55·21	55·21	55·19	55·15	55·07	55·07
☾	53·72	53·71	53·70	53·70	53·71	53·83	53·91	53·94	53·95	53·95	53·93	53·90	53·87

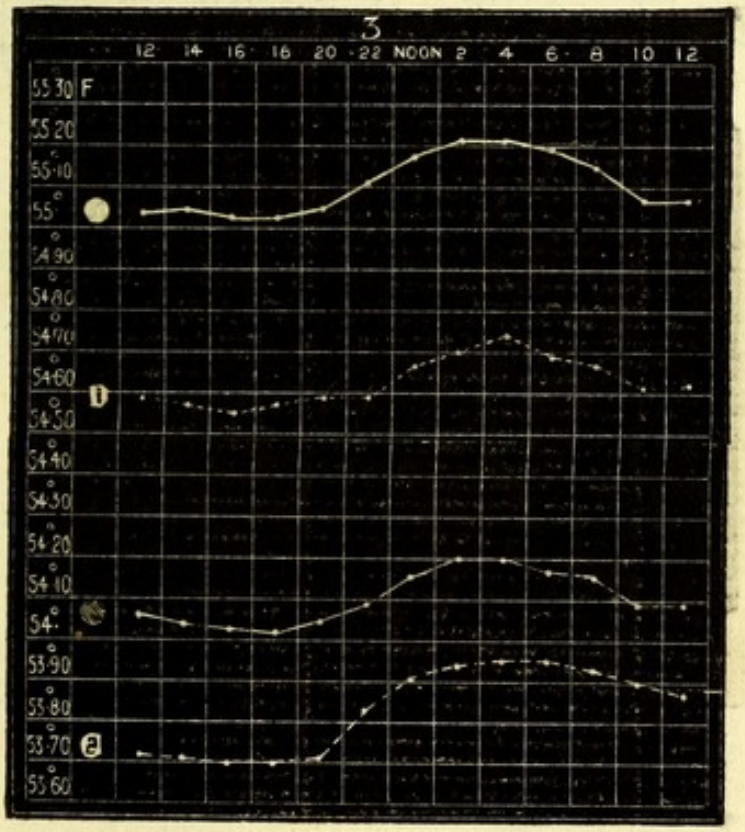


TABLE XVII.

Means of the *temperature of the earth at the depth of* 12·8 *feet* at the Royal Observatory at Greenwich for alternate hours on each of the days of spring-tide and neap-tide during five months in 1847.

	12	14	16	18	20	22	Noon.	2	4	6	8	10	12
☉	51·18	51·16	51·18	51·18	51·19	51·20	51·22	51·25	51·26	51·25	51·25	51·23	51·23
☽	51·37	51·36	51·36	51·36	51·38	51·33	51·38	51·43	51·42	51·42	51·41	51·41	51·40
○	51·57	51·57	51·56	51·56	51·58	51·59	51·60	51·62	51·64	51·63	51·62	51·60	51·58
☾	50·91	50·91	50·90	50·91	50·89	50·92	50·95	50·96	50·95	50·99	50·98	50·97	50·96

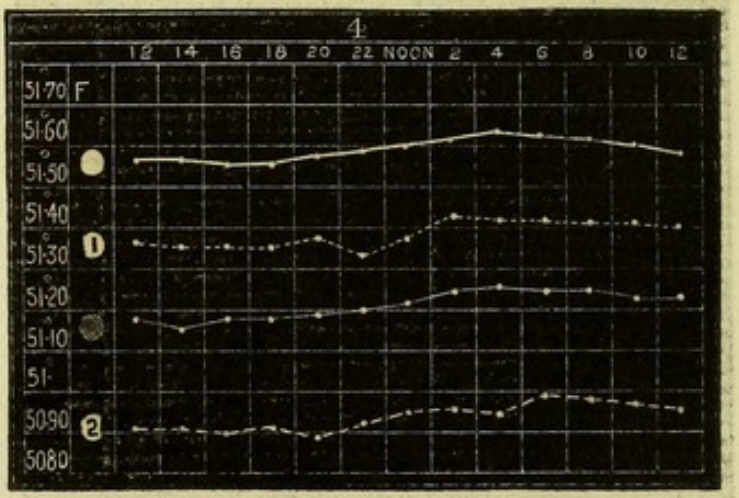


TABLE XVIII.

Means of the *temperature of the earth at the depth of* 25·4 *feet* at the Royal Observatory at Greenwich for alternate hours on each of the days of spring-tide and neap-tide during five months in 1847.

	12	14	16	18	20	22	Noon.	2	4	6	8	10	12
●	49·87	49·88	49·87	49·87	49·87	49·86	49·88	49·89	49·91	49·90	49·90	49·89	49·89
☽	49·97	49·97	49·97	49·97	49·97	49·96	49·97	49·99	49·99	49·99	49·98	49·98	49·97
○	50·02	50·02	50·01	50·01	50·01	50·03	50·03	50·04	50·04	50·04	50·03	50·02	50·01
☾	50·14	50·16	50·14	50·14	50·13	50·15	50·16	50·17	50·15	50·16	50·16	50·15	50·14

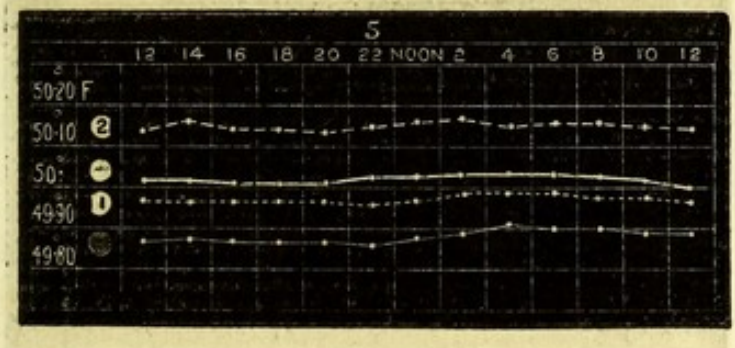
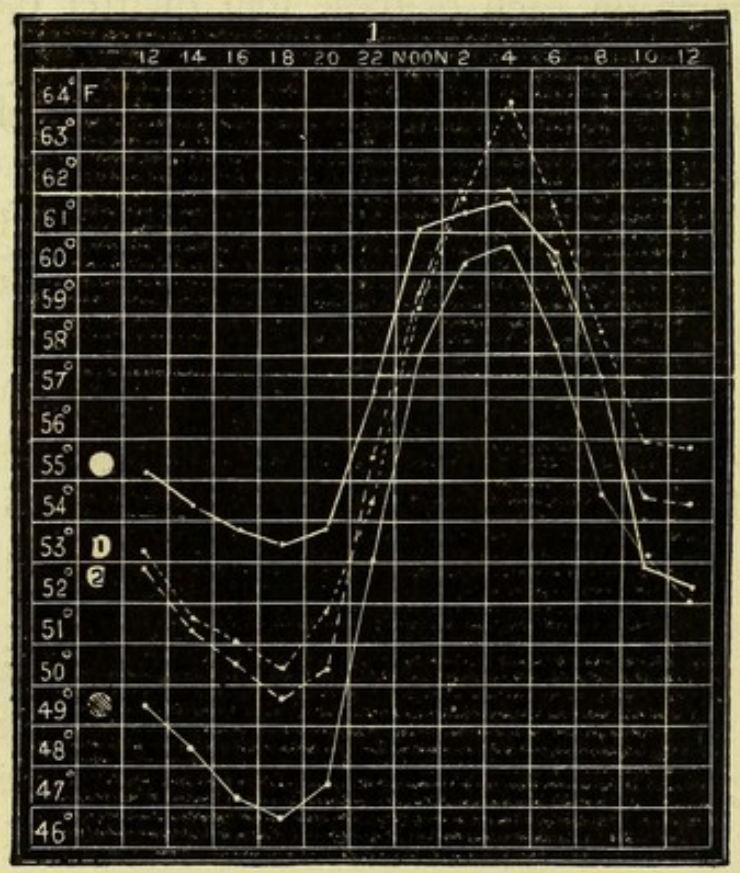


TABLE XIX.

Means of the *temperature of the air* in the case containing the scales of the "earth-thermometers" at the Royal Observatory at Greenwich for alternate hours on each of the days of spring-tide and neap-tide during five months in 1847.

	12	14	16	18	20	22	Noon	2	4	6	8	10	12
●	49·54	48·48	47·26	46·76	47·60	53·06	57·94	60·25	60·70	58·26	54·70	53·12	52·00
☽	53·30	51·70	51·10	50·40	51·82	54·44	59·14	61·94	64·20	61·70	58·64	55·98	55·82
○	55·26	54·60	53·80	53·46	53·82	57·22	61·06	61·56	61·80	60·46	57·42	52·96	52·36
☾	52·85	51·33	50·50	49·68	50·41	55·50	59·56	62·00	62·05	60·25	57·36	54·66	54·46



The differences in the hourly readings on each quarter day of the moon, and the differences in the daily readings of the four quarter days, seem to tell their own

story very plainly, the former pointing sunwards by marking the progress of the sun from hour to hour, the latter pointing moonwards by marking the progress of the moon from one quarter to another. It is also easy to see that the four lines of temperature, separated by well-marked interspaces, marking the four quarter days, are drawn by the moon and not by the sun—by the moon, because the position of the moon in relation to the earth and sun is widely different on the four quarter days,—and not by the sun, because the position of the sun in relation to the earth is practically the same on each of these four quarter days. Indeed it may be looked upon as certain that the four lines of the quarters would run into one line if the sun were alone operative in this matter.

How then are these facts to be accounted for? How is it that the moon, which is supposed to be quite cold, is able to act thus calorifically? How is it that the heat of the sun, which penetrates by conduction only to a very short distance into the earth, is capable of reaching at least to the depth of two miles, as it would seem to do at Bath? How is it that the sun and moon act at all calorifically, for it is certain that for millions upon millions of miles out in space the rays of the sun and moon are as cold as cold can be, even as cold as -273° Fahrenheit, or absolute zero? Where then does the heat come from? Is it that underlying heat are electrical currents which are frittered down into heat by resistance, and which, missing this resistance, are coldness itself? Is it that the material contents of outer space are kept in a state of utter tenuity, not by heat but by

electrical repulsion? And these questions are not without answers. Indeed, there are facts which show that along with the signs of solar and lunar heat are signs of magnetism, which are only intelligible on the supposition that the sun and moon are acting upon the earth magnetically, and therefore electrically, as well as calorifically.

The facts with which I have now to do are to be found by turning, not to the published reports of any observatory in which magnetic observations are carried on, but to a copy in manuscript of unpublished documents containing the hourly readings of horizontal and vertical magnetic force at the Royal Observatory at Greenwich on the days of spring-tide and neap-tide in 1880 —a copy for the possession of which I am also indebted to of the Ex-Astronomer Royal, Sir George B. Airy. I do not turn to the published reports because these only give certain general means, daily, monthly and annual, of magnetic movements at the hour of noon only. I turn to the unpublished records of magnetic movements at spring-tide and neap-tide, because I can conceive of no better way of arriving at a clearer knowledge of the action of the sun and moon upon magnetic force, which is the end I have now in view, than that which opens out in a comparison of the movements at spring-tide, when the sun and moon, in conjunction or opposition, are acting upon the earth in the same lines, with the movements at neap-tide, when the moon is acting upon the earth at right angles to the sun. And I am, I think, fully justified in thus turning from the published to the unpublished evidence by the data which

are set forth in the two following tables. Indeed, I cannot but see that I should have had little or nothing new to say about the action of the sun and moon in terrestrial magnetism if I had not so turned.

The facts themselves are here tabulated :—

TABLE XX.

Means of *Horizontal Magnetic Force* at the Royal Observatory at Greenwich for alternate hours on each of the days of spring-tide and neap-tide in 1880.

	12	14	16	18	20	22	Noon.	2	4	6	8	10
●	·1299	·1299	·1301	·1300	·1293	·1286	·1292	·1297	·1300	·1302	·1304	·1300
☽	·1299	·1298	·1298	·1299	·1295	·1287	·1288	·1295	·1299	·1301	·1301	·1302
○	·1298	·1299	·1299	·1297	·1290	·1286	·1290	·1299	·1300	·1300	·1299	·1298
☾	·1293	·1294	·1294	·1294	·1291	·1282	·1285	·1290	·1296	·1297	·1296	·1294

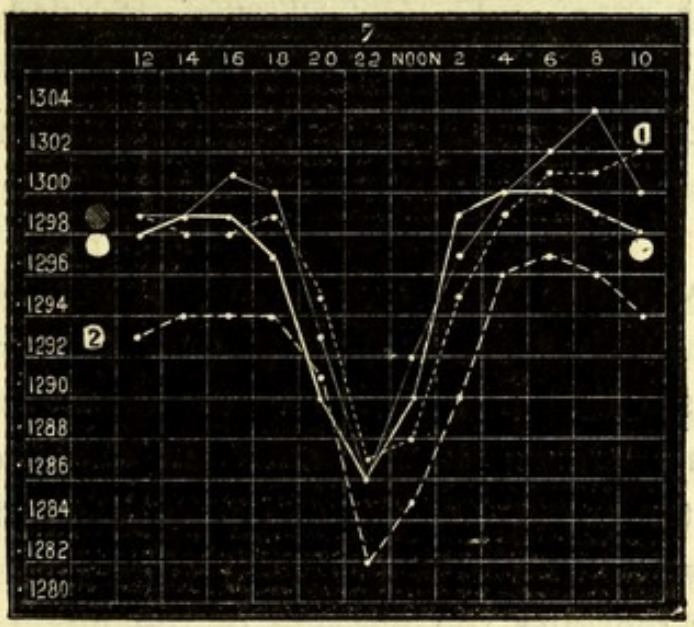
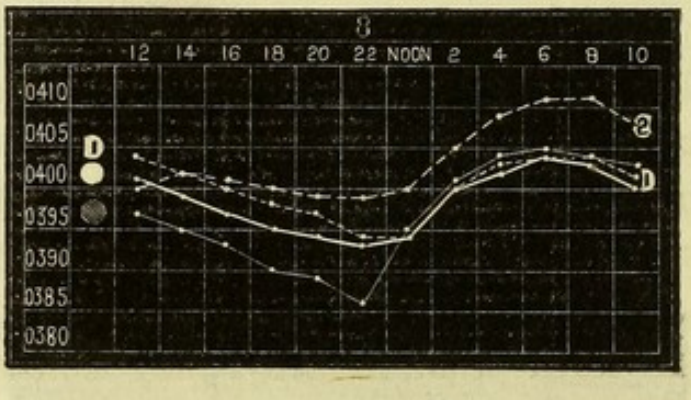


TABLE XXI.

Means of *Vertical Magnetic Force* at the Royal Observatory at Greenwich for alternate hours on each of the days of spring-tide and neap-tide in 1880.

	12	14	16	18	20	22	Noon.	2	4	6	8	10
	·0397	·0395	·0393	·0390	·0389	·0386	·0395	·0401	·0404	·0405	·0404	·0403
☽	·0404	·0402	·0400	·0398	·0397	·0394	·0394	·0400	·0403	·0404	·0404	·0402
○	·0401	·0399	·0397	·0395	·0394	·0393	·0394	·0400	·0402	·0404	·0403	·0400
☾	·0400	·0402	·0401	·0400	·0399	·0399	·0400	·0405	·0409	·0411	·0411	·0408



After what has been said already it is not necessary to spend much time in poring over these tables in order to see that in addition to the movements which are plainly shown to be solar in their origin by marking the passage of the sun from hour to hour are other movements which are as plainly lunar in their origin by marking the passage of the moon from one quarterly stage to another.

It is also not a little significant that the magnetic force should be reduced to a minimum about noon, and, in a lesser degree, about midnight, as it is seen to be in the tables. And why should this be so? Is it that the heat of the sun is inimical to the development of magnetism? It may be so; for it is certain that the manifestations of magnetism are impaired by heat. And if it be so, may it not follow that the magnetism and the heat are convertible forces—that the source of the magnetism as well as the source of the heat is in the sun and moon as well as in the earth—that the facts point away, as it were, from the earth to the sun and moon? As I believe, it is very possible that it may be so. Indeed, when I review the facts in question, and see how the development of terrestrial magnetism is at a minimum when the development of solar heat is at a maximum, the only conclusion to which I can come is that the sun and moon must be regarded as having even more to do with the development of terrestrial magnetism than the earth—that, in fact, I may look upon the sun and moon as winding a magnetic belt loosely around the earth, and upon the earth as spinning round within this belt without carrying it along with her.

What then is the meaning of this connexion between the magnetic belt of the earth and the sun and moon? Is the earth traversed by electric currents from the sun and moon? Is the belt of terrestrial magnetism no more than the natural development of magnetism around these currents? Is there a process of electrical interaction continually going on between the sun and the earth, and are these currents to be explained by supposing

that the potential of the sun is higher than that of the earth, and that the currents are the natural consequences of these differences in potential? Is it that the moon is in the same case as the sun, with this difference, that the potential of the earth is higher than that of the moon and the current in the opposite direction?* I see several reasons for answering these questions in the affirmative, and none to the contrary. I see that the origin of the currents may easily be accounted for in this way. I see no difficulty in believing that the earth may be traversed by these currents, and that terrestrial magnetism may hold the same relation to these currents which ordinary magnetism holds to any current in any case. I do not see how, in the absence of current, the development of magnetism in its terrestrial or any other form is ever possible. And, therefore, I may fairly consider that in what I have said about the development of terrestrial magnetism there may be a thread of truth which is not easily snapped across.

* The fact that the spring tides are higher than the neap tides may supply a reason for believing that the solar and lunar currents are, as I have hinted, opposed to each other in direction. For what is implied according to the premisses? It is that at spring-tide the development of subterranean heat, which is dependent upon the action of the solar and lunar currents, is diminished because these currents are then as far opposed to each other as they can be, and that, for this reason, the tide is higher because the land is less lifted up. It is that at neap-tide the development of subterranean heat, which is dependent on the action of the solar and lunar currents, is increased, because these currents are as little opposed to each other as they can be, and that, for this reason, the tide is lower because the land is more lifted up. All this is quite in accordance with the premisses, but the evidence does not go far enough to allow me to use it as an argument in support of what I have said about the tides, for a good deal has to be done before it can be said that the evidence in favour of this opposition in the solar and lunar currents is made out satisfactorily.

And it is also possible that the solar and lunar currents, which are supposed to traverse the earth and to lead to the development of terrestrial magnetism in the way which has been indicated, may be powerful currents. The magnetic needle is made to point north and south by the directive force of the earth's magnetism. The magnetic needle is made to point across a wire or coil of wire along which a current is passed, and if this wire or coil be placed in a line with the magnetic meridian, and the current be strong enough, this pointing is almost due east and west. In reality, however, the current is never strong enough to drive the needle into a position which is exactly at right angles to the direction of the wire or coil, never strong enough to abolish the directive force of the magnetism of the earth. The case is indeed one in which it seems fair to assume that the currents concerned in the development of terrestrial magnetism are more powerful than any current which has been used in the experiment upon the wire or coil, for, so far as is known, the force of the current and the force of the magnetism connected therewith are directly proportionate to each other. Moreover, the high potential of atmospheric electricity at a very short distance above the surface of the earth—a potential which at the height of a very few feet has to be measured by several hundreds of volts—may surely be looked upon as a reason for believing that the atmosphere is traversed by very powerful currents. And certainly no contradiction to this notion is to be found in the fact that in 1880 the measurement of terrestrial magnetic force in dyne-units was ·18 for horizontal and ·49 for vertical

magnetic force, the practical unit of one ampère being equal to one-tenth of one of these dyne-units.

And this being the case, it is (after what has been said about the action of the sun and moon in terrestrial magnetism) not unfair to conclude that underlying the solar and lunar rays are electrical currents which are frittered down into heat when a certain degree of resistance is opposed to their passage, but not otherwise, the case being not really dissimilar to that which is exemplified in the familiar experiment in which, on passing an electrical current along a chain composed of links of silver and platinum wire placed alternately, the platinum becomes incandescent, and the silver remains cool, or nearly so, simply because the resistance opposed to the passage of the current is high in the former metal and low in the latter.

And without doing violence to common sense it is also possible to imagine that this view may be applied to the explanation of subterranean heat. Indeed to allow of this being done all that is necessary is to suppose that the earth is traversed by powerful electric currents from the sun and moon, and that these currents are frittered down into heat in the way which is exemplified in certain experiments which, with the permission of General Webber, the manager, and with the help of Mr. Mordey, one of the officers, were carried out at the works of the Anglo-American Electric Light Corporation in Lambeth with the powerful currents used for purposes of electric lighting on a large scale.

Wishing to see how heat is developed in the track of the current across a very bad conductor of which the

form is actually or virtually spherical, what was done in these experiments was to pass a powerful electric current across a hollow globe or a shallow circular tray filled with cast-iron filings, diametrically in the former case, from edge to edge through the centre in the latter. The globe and tray were formed of highly varnished papier maché, the globe being a little more than 6 inches in diameter, the tray being about $4\frac{1}{2}$ inches across by 1 inch in depth. The globe, in addition to having binding-screws on opposite sides for fixing the wires conveying the current, had an open mouth at the top through which the filings and the thermometers, which figure conspicuously in the experiments, could be passed in and out. The temperature was taken at the same time by three mercurial thermometers having scales reaching to 600° F., at three points, B, A, and C, point A being at the centre, point B being on one side of the centre and midway between it and the adjacent electrode, point C being on the other side of the centre in a position exactly corresponding to that of point B. And besides this all that need be said is that the thermometers were kept in position partly by their bulbs being deeply buried in the filings, and partly by their free ends being attached to the horizontal arm of a suitable stand. The preparations for the experiments with the tray were less complicated, for here no thermometers were required, and all that had to be done was to take the tray, to fit it with binding-screws for the wires conveying the current to the proper places, to fill it with filings, and to pass the current.

The particulars and results which have to be noticed in some of the experiments in which the globe full of filings was used are those which are here set forth in a tabular form.

TABLE XXII.

EXPERIMENT 1. Direction and Duration of Current.	Thermometer Readings at			Electromotive Force of Dynamo-electric Machine in Volts.
	B.	A.	C.	
+ — ——→				
0 min. 30 secs.	200°	190°	300°	800
1 „	370°	270°	470°	„

TABLE XXII.

EXPERIMENT 2. Direction and Duration of Current.	Thermometer Readings at			Electromotive Force of Dynamo-electric Machine in Volts.
	B.	A.	C.	
+ — ←——				
1 minute	105°	68°	120°	700
2 minutes	160°	82°	168°	„
3 „	220°	120°	225°	„
4 „	340°	166°	340°	„
5 „	350°	174°	356°	„
6 „	380°	194°	384°	„
7 „	420°	220°	404°	„
8 „	440°	232°	418°	„
9 „	458°	250°	430°	„
10 „	470°	264°	492°	„

TABLE XXIV.

EXPERIMENT 3. Direction and Duration of Current.	Thermometer Readings at			Electromotive Force of Dynamo-electric Machine in Volts.
	B.	A.	C.	
+ → −				
1 minute	144°	138°	148°	700
2 minutes	180°	140°	181°	,,
3 ,,	228°	148°	236°	,,
4 ,,	275°	160°	284°	,,
5 ,,	320°	180°	320°	,,
6 ,,	350°	196°	350°	,,
7 ,,	378°	218°	376°	,,
8 ,,	398°	234°	396°	,,
9 ,,	450°	250°	410°	,,
10 ,,	476°	268°	430°	,,

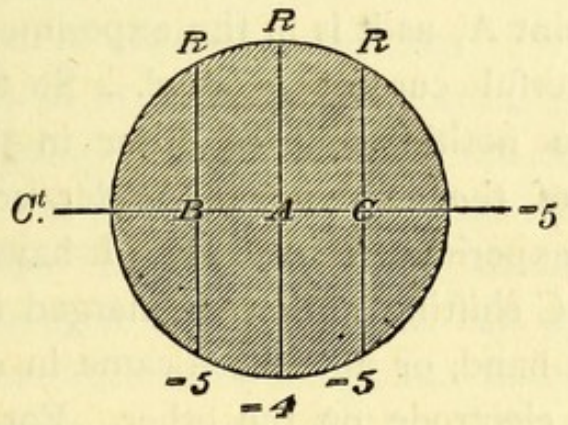
TABLE XXV.

EXPERIMENT 4. Direction and Duration of Current.	Thermometer Readings at			Electromotive Force of Dynamo-electric Machine in Volts.
	B.	A.	C.	
+ → −				
1 min.	65°	100°	67°	400
1 ,, 30 secs.	83°	145°	80°	,,
2 mins.	108°	184°	95°	,,
2 ,, 30 secs.	137°	220°	110°	,,
3 ,,	150°	238°	120°	,,
3 ,, 30 secs.	161°	252°	134°	,,
4 ,,	170°	270°	145°	,,
4 ,, 30 secs.	180°	281°	158°	,,
5 ,,	192°	292°	164°	,,
6 ,,	220°	318°	174°	,,
7 ,,	242°	332°	194°	,,
8 ,,	260°	346°	210°	,,
9 ,,	275°	356°	224°	,,
10 ,,	290°	366°	238°	,,

When the more powerful current is used more heat is developed at points B and C than at point A; when the less powerful current is used more heat is developed at point A than at points B and C. This always happened. And in reality it is not at all surprising that it should be so. Indeed, it would almost seem to follow as a matter of course if it be allowed, as must needs be, that in passing diametrically across the spherical mass of iron filings the current must encounter a degree of resistance which is not the same at all points, being, in accordance with the law of Ohm, inversely proportionate to the cross-sectional area of the mass at any point, and that, in order to bring about the full development of heat, there must be a proper balancing between the action of the current and the counteraction of resistance, too much resistance interfering with the development of heat by enfeebling the current, too little having the same effect by allowing the current to pass without paying full toll in heat. The force of the current, be this what it may, is the same at B and A and C, because it is the same in every part of the circuit. The force of resistance, which is inversely proportional to the cross-sectional area of the conductor, instead of being the same at B and A and C, will be the same at B and C, but not at A, because the cross-sectional area, which is the measure of the resistance, is the same at B and C but not at A, being at A about one-fifth lower than it is at B and C. The development of heat is also at its maximum when the action of the current and the reaction of resistance are duly balanced. Supposing the value of the resist-

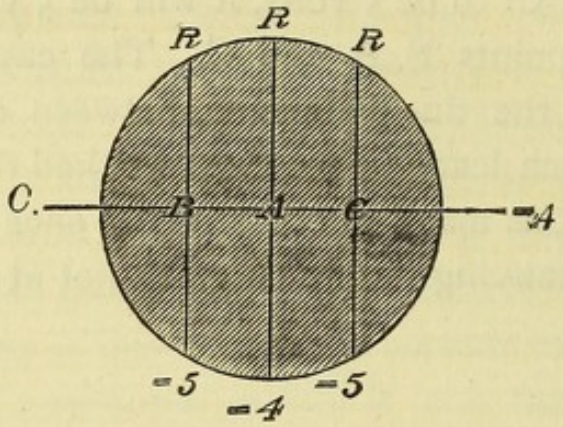
ance, R, to be 4 ohms at point A, it will be, speaking roughly, 5 ohms at points B and C. Supposing the value of the current C^t to be 5 volts, it will be 5 volts equally at the three points B, A and C. The case, indeed, is one in which the due balancing between current and resistance, which leads to the most marked development of heat, will be, as may be understood at once by referring to the accompanying figure (Fig. 1), not at the central point A,

FIGURE 1.



but at the two lateral points B and C, as it is, in fact, in the experiments in which the more powerful currents are used. Supposing the value of the resistance to be still 4 ohms at the point A, and 5 ohms at the points B and C; supposing the value of the current to be 4 volts instead of 5; then

FIGURE 2.



the most marked development of heat will be, as may be seen at once by referring to the accompanying figure (Fig. 2), not at the two lateral points B and C, but at the central point A, as it is in the experiment in which the less powerful current is used. So that in fact there may be nothing out of order in the seeming irregularities of the experiments under consideration, or in other experiments in which I have found the points B and C shifting until they merged in the point A on the one hand, or until each came in contact with the adjacent electrode on the other. For, after what has been said, it is plain that this shifting towards and merging in the point A of the two points B and C must take place as the value of the current in volts falls from 5 to 4, and also that this shifting towards and coming in contact with the electrodes of the two points B and C must come to pass as the value of the current in volts rises higher and higher up to a certain point above 5. All this is plain enough if only a moderate

degree of attention be paid to the case. But be the explanation what it may, the facts remain, and it is with the facts and not with the explanation that I am specially concerned now.

In the experiments with the tray full of filings the current was supplied, sometimes by dynamo-electric machines, and sometimes by secondary batteries or accumulators, its value varying from 200 to 400 volts and from 7 to 20 ampères. In the experiments with the globe the current was arrested before the heat developed in the filings was high enough to burst the thermometers: in these, the current was allowed to pass until the filings at certain points became incandescent and molten, and until the parts of the tray underlying these parts were charred into holes. As in the former experiments so in these it was indeed quite plain that the heat developed in the track of the current was, not at all points in this track equally, but at certain points only, and also that the position of these points varied with the strength of the current, this position corresponding more or less to the points B and C when the stronger current was used, and to the point A in the contrary case. All this was made plain enough by the incandescent and molten condition of the filings at these points when the current was passing, as well as by the semi-solid or solid nodules of iron, and by the underlying charred places in the tray, remaining afterwards.

Before trying these experiments I had been led to the same conclusion by other experiments in which the currents used were supplied sometimes by voltaic

batteries, sometimes by dynamo-electric machines, and sometimes by secondary batteries or accumulators. At first, by having greatly underrated the value of the resistance opposed by the filings to the passage of the current, and by having made use of comparatively feeble currents, I failed to get any distinct manifestations of heat. Afterwards, being taught by experience how to act, the thermometers gave feeble indications of heat similar to those which are exhibited so distinctly in the experiments of which an account has been given. The most powerful current of which I made use was in fact not sufficiently powerful to produce any very decided indication of heat except in one instance, where I found that on passing a strong current from several secondary batteries across a tray full of filings the filings became incandescent and molten during the passage of the current at two points corresponding to B and C, and solidified afterwards, and the tray underlying these heated parts was burnt through and through into actual holes. I saw enough to make me not at all surprised at what happened at the works of the Anglo-American Electric Light Corporation in Lambeth. I saw enough to justify me in saying that the experiments there are in no way exceptional—that when heat is developed in a body of high resistance of which the form is actually or virtually spherical, the law of development is that which is exhibited in these experiments.

What then? Am I at liberty to believe that there is in these experiments that which is calculated to elucidate the dark problem of subterranean heat? Is the earth,

at right angles to her axis, or nearly so, traversed by currents proceeding from the sun and to the moon? Is the resistance of the earth to be measured by that of the mass of iron filings? Is the solar current frittered down into heat by the resistance opposed to its passage through the earth, not at all points equally in its track, but chiefly at two points, far apart, with the centre of the earth midway between them, and with each of them deep down underground, and equally so, and is the lunar current in the same case? Are the currents under consideration, that is, the solar and lunar currents, comparable in value to the more powerful currents made use of in the experiments? There is, so far as I can see, no reason why it may not be so except one which may be found in the want of resistance in the earth, and at first sight this reason would seem to be fatal. But in fact the actual case is very different to what it is supposed to be. When the circuit of the electric telegraph is completed by a return wire this wire opposes a certain degree of resistance to the transmission of the message. When this circuit is completed by the earth in the ordinary way this resistance becomes nil or very nearly so. And the law of Ohm shows why it should be so, for if the resistance of a conductor be inversely related to the area of the cross-section, the resistance of the earth, or even of that part of it which is acting as a conductor, must be all but infinitely small in comparison with that of the wire by which the earth is put in communication with the battery of the telegraph. But the earth must be, what Count du Moncel calls *une bonne terre* in order that it may act as a perfect conductor:

and, in fact, the evidence supplied by this very excellent observer, who is allowed on all hands to be the chief authority in this matter, is not the only evidence which serves to show very conclusively that the resistance of dry earthy matter is very far from being nil—that the absence of resistance in the earth is very greatly dependent upon the presence of dampness. Usually no doubt the resistance of the earth to the transmission of the telegraphic message may be regarded as practically nothing, but now and then, as Mr. Latimer Clark tells me, the circuit of the telegraph could not be completed by the earth, and it was necessary, as in the case of the line over the 60 miles of desert between Suez and Cairo, to use the old return wire before the message would pass.

But the case with which the telegraph engineer has to deal is not that with which I have to deal here. The case with which the telegraph engineer has to deal is that of a current entering into and returning from the earth through channels which are infinitely narrow in comparison with the breadth of the interposed track of earth through which the current has to pass in order to complete the circuit, and therefore it is easy to see that the resistance opposed to the passage of the current in the earth may be infinitely small, even though the part of the earth which is acting as *une bonne terre* is very limited in extent. The case with which I have to deal is that of solar and lunar currents striking upon *every part* of the hemisphere of the earth exposed to the sun and moon, and forcing their way across the earth by continuing to move on in the same

direction. It is one in which there may be no spreading out of currents as in the isolated current of the telegraph, and, for that reason, no reduction of the resistance of the earth to nothing or thereabouts. It is one in which the earth may be, as it were, crowded by currents passing across it and encountering all the resistance opposed to their passage simply because they are not at liberty to spread out. It is one indeed in which, instead of being nil, the resistance of the earth may be very high, for there is reason to believe that the damp earth which is, as it would seem, chiefly concerned in carrying the telegraphic message does not extend to any great depth below the surface of the earth or the floor of the sea. Indeed it is not improbable that for thousands of miles the dry and compressed heart of the solid earth may have a very high degree of resistance, for, as is clearly made out by Du Moncel and others, many rocks when dry are very far from being good conductors, marble, in fact, being so very bad a conductor as to occupy, along with cotton wool, paper, and the human body, a place midway between conductors and non-conductors. And thus, after all, it is by no means improbable that the resistance of the earth to the passage of the solar and lunar currents may not differ very widely from that of the mass of iron filings in the experiments under consideration.

And if this be so then it is easy to see that the experiments under consideration may supply a key to the mystery of subterranean heat,—that in fact the earth may be traversed, at right angles to her axis, or nearly so, by electrical currents from the sun and to the moon,—

that these currents may be frittered down into heat by the resistance opposed to their passage across the earth, not equally at all points in their track, but chiefly at two points, far apart, with the centre of the earth midway between them, and with each of them deep down underground, and equally so,—that the solar and lunar currents are in the same case in this respect,—that the solar and lunar points of heat are coincident at full moon and new moon, and as far apart as they can be at each of the intermediate lunar quarters,—that the heat so generated is fierce enough to reduce rock to the condition of molten lava,—that the movement of these heated points as the earth rotates upon her axis may form a ring of fire stretching from the equator to the tropics, and beyond them, and thinning off, or opening out, on each side towards the poles,—and that the heat of this ring may change the earth from the form of a sphere into that of an oblate spheroid flattened at the poles, and cause the land to rise above the waters. And besides all this it may be supposed—and this is a matter of primary consequence as bearing upon the interpretation of the tides—that there are daily variations in subterranean heat dependent upon movements of the sun and moon, which variations must tell upon opposite sides of the earth at the same time equally, so that any stage in a tidal wave in the land here must be repeated by a wave in the same stage at the antipodes.

Nor is it necessary to change the point of view in order to see how to explain the peculiarities of solar and lunar action as regards heat. As regards solar

and lunar heat, the question is not of rays of heat radiating in all directions from a single source and only becoming cooler as the distance from the source increases—of rays of heat which are always more or less hot: it is one of lines of electrical force connecting two or more points, in which lines heat may or may not be developed by the counteraction of resistance. Between the earth and the sun these lines form a truncated cone with the sun at the base and the earth at the truncated apex, the difference in diameter between the base and apex being as 900,000 to 8,000 miles, or thereabouts. The case is one in which, assuming that the current encounters a certain degree of resistance at the base and apex of this cone, and none elsewhere, it is supposable that electricity may be frittered down into heat at the earth and not at the sun, for as the resistance is inversely related to the cross sectional area of the cone, it is possible that the resistance necessary to the development of heat should be present at the earth and absent at the sun. Between the earth and the moon, on the other hand these lines of force are arranged in a truncated cone with the earth at the base and the moon at the truncated apex, the difference in diameter between the base and apex being as 8,000 miles to 2,150 miles, or thereabouts. The case here is one in which the resistance opposed to the passage of the current at the base and apex of the cone, and there only, must be less at the earth than it is at the moon, and may be what is necessary to fritter down electricity into heat at the moon, but not at the earth. The case of the earth and moon is indeed exactly the reverse of the case of the earth and

the sun, and if heat be the consequence of the contention between current and resistance it is quite conceivable that at the sun the solar currents, which are hot here, may be no hotter than the lunar currents here, and that at the moon the lunar currents, which are cold here, may be as hot as the solar currents in the desert of Sahara at midsummer, or hotter. Nay it is quite conceivable that the solar and lunar currents here may vary in temperature in consequence of some accidental alteration in the resistance opposed to their passage earthwards or moonwards. Thus, it is possible that the unusual warmth of the two winters before the last may have been owing to the increased resistance to the passage of the solar and lunar currents caused by the presence in the outskirts of the atmosphere of the widespread cloud of dry dust, cosmic or volcanic, to which the brilliant prolongation of the *abendroth* and *morgenroth* was attributable. Thus, it is possible that the clouds may keep the earth warm, not so much by interfering, as a blanket would do, with the radiation of heat from the earth, as by adding to the resistance which is necessary to the development of heat in the solar and lunar currents.

And if this may be said in explanation of solar and lunar and subterranean heat, it is quite possible that the same explanation may be applied to stellar heat, for between the stars and the earth it is easy to see that there may be the same electrical interaction, the same differences of potential, the same current movement, the same resistance, and the same development of heat if the contention between current and resistance be properly proportioned.

And certainly there is no occasion to change this point of view in order to see why the solar and lunar and stellar currents should be utterly cold for millions upon millions of miles in empty space. Here these currents are more or less hot because the resistance is present which is necessary to fritter them down into heat: in *empty* space, for millions upon millions of miles, these currents are as cold as cold can be simply because this resistance is absent. This is all. Nor, as I have said, is heat necessary to account for the extreme tenuity of the material contents of the emptier parts of space, for this state of things may be nothing more than the natural consequence of the operation of unresisted electrical repulsion among the molecules.

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THERE can be no doubt as to the present land having once been the bed of the sea. There can be no doubt as to the climate having been very much warmer during the whole of the time spent in the formation of the primary, secondary, and tertiary fossiliferous rocks. There can be no doubt as to the present state of things as to climate having been ushered in by a time or times of intense cold. Indeed doubt upon these points can only be entertained by one who has yet to become acquainted with the very rudiments of geology.

I need say nothing to prove that the present land was once the bed of the sea, and but little to show that the climate was more or less tropical during the whole of the time spent in the formation of the three great groups of fossiliferous rocks, and that the present state of things as to climate was ushered in by cold of arctic severity.

In a few words Sir Charles Lyell says all that need be said upon the second of these points, and these words I copy without comment.

"In the greater part of the miocene and preceding eocene epoch the fauna and flora of central Europe were sub-tropical, and a vegetation resembling that now

seen in northern Europe extended into the arctic regions as far they have been yet explored, and probably reached the pole itself. In the secondary or mesozoic ages, the predominance of reptile life, and the general character of the fossil types of that great class of vertebrata, indicate a warm climate, and an absence of frost between the 40th parallel of latitude and the pole, a large icthyosaurus having been found in latitude 77° 16′ N. The great development of the tetrabranchiate and dibranchiate cephalopoda, with the general character of the molluscs and corals, as well as of the plants, is in perfect accordance with the inferences deduced from the associated fossil reptiles. If we then carry back our retrospect to the primary or palæozoic ages, we find an assemblage of plants which imply that a warm, humid, and equable climate extended in the carboniferous period uninterruptedly from the 30th parallel of latitude to within a few degrees of the pole, or to northern regions, where at present the severe winter's frost, and the almost universal covering of snow, lasting for many months, preclude the existence of a luxuriant vegetation. A still older flora, the devonian, so far as we have yet traced its geographical distribution, leads to similar inferences, and the invertebrate fauna of the devonian, silurian, and cambrian rocks have such a generic resemblance to that of the carboniferous, permian, and triassic periods as to imply that a similarity of conditions as regards temperature prevailed throughout these six periods."—(Lyell's "Principles," vol. I., p. 231.)

And upon the third point the evidence is not less conclusive.

The climate of this country, and of a very great part of northern Europe and America, was as severe as that of Greenland and Spitzbergen in the time intervening between the formation of the three great groups of fossiliferous rocks and the advent of the present state of things Everywhere in these regions are signs which show that the present land was then buried under enormous glaciers. Rocks in all directions are grooved, and scored, and scratched, and rounded off, and planed down, and polished, exactly as they now are by glaciers which are still at work, or which were at work not long ago. Fragments of rock, evidently brought from a distance, and often from a great distance, are found heaped together in a way which is only intelligible on the supposition that they are actually the moraines of some ancient glacier. Erratic blocks of rock, as evidently brought from a distance, and often from a great distance, lie scattered about on the ground, or half buried in the ground, as *roches moutonnées*, or perched up on pinnacles of various sorts, in a way which is unintelligible unless it be supposed that at one time they lay upon, and were carried along by, some ancient glacier. And the same story is told by the sort of deposit to which the name of "till" is given, and by the planed and scored rocky surface often met with under the "till." For "till" (which is an unstratified deposit of tough clay or half hardened stone in which are embedded rocky fragments of various sizes and shapes, some of them smoothed down and polished and scored, and others not so) is evidently nothing more than an inspissated and hardened form of the sticky and unctuous mud, mixed

with rocky fragments of various shapes and sizes, marked or not marked by ice-action, which is now met with under a glacier, and which is formed by the grinding of the glacier against its bed—by that very grinding to which the planed and scored surface of this bed also bears witness often enough.

How then are these facts to be accounted for? Has there been an alteration in the direction of the axis of the earth? Must I agree with those who believe that this direction was perpendicular to the plane of the ecliptic when the climate was sub-tropical or even tropical? I can see reason for so believing, for it is certain that in this case, by being in a state of constant equinox, the earth would be winterless. I can see that the causes which lead to the development of subterranean heat according to the premisses, by an alteration of the axis of the earth from a position in which it was perpendicular to the plane of the ecliptic into its present position, would lead, through a stage of universal deluge and terrible cold, to the tremendous revolution in which the bed of the ancient sea was made to take the place of the present land. Nay, I can even see how this alteration in the direction of the axis of the earth may have been brought about by natural causes, for it is quite conceivable that this alteration may have been caused by the earth having fallen foul of a large aërolite in a certain way,—that this aërolite may have been shivered into fragments by this collision,—that these fragments still go on circulating in the same orbit as that in which the parent aërolite moved, —and that the angular meteorlites which now and then fall upon the earth are some of these very fragments.

It is not difficult to see that this alteration in the direction of the axis of the earth may have led to that mighty revolution in which the bed of the ancient sea was converted into the present land.

If it be allowed that the development of the subterranean ring of fire is more marked under the parts of the earth on which the rays of the sun and moon fall directly than it is under the parts on which they fall obliquely, and that, owing to its great depth below the surface, the ring, once formed, must be very slow in losing heat by conduction, it is easy to see that great changes would attend upon that alteration in the direction of the axis of the earth about which I am speaking. Before the time of this alteration, in consequence of the sun and moon being then directly over the equatorial region at all times, the ring, and the land upraised by its heat, may have occupied a position which did not extend very far north and south of the line: after this time, owing to the fact that in the course of the year every part of the inter-tropical region would be directly exposed to the direct action of the sun and moon, the ring, and the land raised by its heat, must widen very considerably. If, by the alteration in the direction of the axis of the earth, the part of the earth directly exposed to the action of the sun were changed from the equator to one of the solstices, there may then have been two rings of subterranean fire, neither of them very broad, one under the equator, the other under the tropical line. Subsequently, as the year rolled on, other rings may have formed so as to fill up, first, the space between this tropical

line and the equator, and, afterwards, the space between the equator and the other tropical line, in which case the old ring, and the overlying land upraised by its heat, may have gradually widened until it took in all the inter-tropical region, and, not improbably, a wide extent of the extra-tropical region as well. In this way, in the first instance, a new ring of subterranean fire may have been formed upwards of fifteen hundred miles away from the old equatorial ring, and under a part of the earth which was then covered with water. In this way, by the heat of this new ring and of other new rings following it, as the days wore on, the bed of the ancient sea may have been raised until it became the present land. In this way, by the rising of its bed up to a certain point, the ancient sea may have been forced over the ancient land until the state of things was no other than that of a universal deluge. In this way, by the rising of its bed beyond this point, the sea, seeking its own level, may have settled down into its present bed, and so left the land dry as it is now. All this appears to follow almost as a matter of course. Indeed it is not necessary to go far out of the way in order to see how, in the space of a year, the great revolution in which the bed of the ancient sea was converted into the present land may have been brought about by the shifting of the axis of the earth from a position perpendicular to the plane of the ecliptic into its present position.

And, this being the case, it is not to be wondered at that the present state of things as to climate should have been ushered in by a time of terrible cold, for the state

of universal deluge of which I have been speaking would carry with it a state of refrigeration in which the ice-caps of the poles would be drawn together until their edges were upon the point of meeting at the line.

The earth is heated chiefly by an action of the sun which tells more upon the land than upon the water: and the climates of the earth would be much altered by any marked difference in the distribution of land and sea. With land in the tropics and water elsewhere the earth would be much hotter than at present: with this state of things reversed, the earth would be much colder than at present, the latter arrangement, as Lyell pointed out long ago, being likely to produce a glacial epoch in regions now temperate and sub-tropical. With water everywhere it may be supposed that the fall of temperature would be still more considerable. With land everywhere the earth would be unsupportably hot: with water everywhere the earth would be unsupportably cold. So it must be. And, therefore, I may fairly conclude that a state of universal deluge would carry with it a state of refrigeration in which the arctic regions would approximate closely to the equator.

Nor is unusual keenness of vision necessary in order to see that the new land, which is the present land, would lie under enormous glaciers until the ice had time to melt away, and that the reign of frost by which the present state of things as to climate was ushered in may be thus accounted for.

It cannot be doubted that the sub-tropical period in which the early fossiliferous rocks were deposited was followed for a time by one or more glacial epochs, and

the only question is whether this great fall of temperature was continuous so as to form one glacial epoch only, or interrupted by interglacial periods of warmth so as to form more than one such epoch. The occasional presence between layers of "till" of deposits containing remains of plants and animals which could have flourished only in a warm climate is supposed to point to these warm interglacial periods, but it is quite possible that these remains may have been brought to the places where they are found by one and the same glacier having happened to plough its way through fossiliferous strata which may have been formed when the axis of the earth was perpendicular to the plane of the ecliptic, and when the earth was warmer by being perpetually in a state of winterless equinox. And there is no difficulty in entertaining this notion, even though, in addition to the ordinary signs of ice-action, many deep valleys and many extensive denudations in the tertiary strata and chalk may have to be included. In this way the fossils which are supposed to point to warm interglacial periods may really point to tertiary and secondary fossiliferous rocks which were formed in the warm times which preceded the present state of things as to climate, and which were broken down by, and carried along under or on the glaciers which covered the greater part of the modern land until the ice had time to melt away. Indeed, there are reasons, with which I have to concern myself presently, which make me more ready to entertain this notion than to believe that there have been many glacial epochs separated by warm interglacial epochs, the glacial and the interglacial epochs alike extending over periods

of time well-nigh interminable. At all events there is some show of reason for believing that the revolution in which the bed of the ancient sea was converted into the present land was attended by a time of terrible cold,—that the new land was covered with tremendous glaciers until the ice had time to melt away,—and that the deposits among the signs of ice action containing the remains of plants and animals which could only have flourished in a warm climate may point rather to the ploughing of one and the same glacier through fossiliferous rocks which had been formed long before than to warm interglacial epochs which have to be accounted for by slowly recurring astronomical causes.

And thus it is quite possible to see how the revolution was brought about in which the bed of the ancient sea was converted into the modern land, and how other dark passages in the past history of the earth may be elucidated.

CHAPTER II.

A NEW DEPARTURE IN WHICH THE AGE OF THE EARTH IS NOT FOUND TO REACH VERY FAR BACK INTO IMMEMORIAL TIMES.

I CANNOT agree with those who believe that the history of the earth reaches far back into immemorial times. On the contrary, I am constrained to believe that the time spent in the compilation of the "record of the rocks" may be shortened until it is brought within comparatively narrow limits.

Among the reasons for thinking that interminable ages have not been spent in the formation of the principal fossiliferous rocks is one which to my mind is almost conclusive, and to which, strange to say, attention has not been directed, and this is to be found in the absence of any signs of glacial epochs among these rocks. For, as will be seen in the following table, in which Sir C. Lyell places side by side the results of the laborious calculations of Dr. James Croll and Mr. John Carrick Moore, several glacial epochs must have left their marks among the fossiliferous rocks if the time spent in the deposition

of these rocks reached no further back than a single million of years.

Table showing the variations in the eccentricity of the earth's orbit for a million years prior to A.D. 1800, *&c.*

	1. Number of years before A.D. 1800.	2. Eccentricity of orbit.	3. Difference of distance in millions of miles.	4. Number of winter days in excess.
	0	·0168	3	8·1
	50,000	·0131	2¼	6·3
A	100,000	·0473	8½	23·0
	150,000	·0332	6	16·1
B	200,000	·0567	10¼	27·7
	250,000	·0258	4½	12·5
	300,000	·0424	7¾	20·6
	350,000	·0195	3½	9·5
	400,000	·0170	3	8·2
	450,000	·0308	5½	15·0
	500,000	·0388	7	18·8
	550,000	·0166	3	8·0
	600,000	·0417	7½	20·3
	650,000	·0226	4	11·0
	700,000	·0220	4	10·2
C	750,000	·0575	10½	27·8
	800,000	·0132	2¼	6·4
D	850,000	·0747	13½	36·4
	900,000	·0102	1¼	4·9
E	950,000	·0517	9¼	25·1
	1,000,000	·0151	2¾	7·3

Explanation of the Table.

Column 1. Division of the million years preceding A.D. 1800 in 20 equal parts.

Column 2. (computed by Dr. James Croll by aid of Leverrier's formula) giving the eccentricity of the earth's orbit, in parts of a unit equal to the mean distance or half the longer diameter of the ellipse.

Column 3 (computed by Mr. John Carrick Moore) giving in millions of miles the difference between the greatest and least distances of the earth from the sun during the eccentricities given in column 2.

Column 4 (also computed by Mr. Moore) giving the number of days by which winter occurring in aphelion is longer than the summer occurring in perihelion at these different epochs.

Here then are facts which show that in the last million years the orbit of the earth must have undergone great variations in eccentricity in the five periods to which the letters A, B, C, D, and E are prefixed. At present the eccentricity of the orbit amounts to about three million miles: in these five periods this eccentricity was 3, 3·5, 3·5, 4·5, and more than three times greater than it is now. Now the winter here is in perihelion and the cold very moderate: in these five periods, with winter at the aphelion, the cold here would be as severe as it is in Greenland or Spitzbergen, for then, when most in aphelion, the earth would be nearly 11 million miles more distant from the sun, and the winter 36 days longer, and the heat of the sun one-fifth less intense, the summer, even though much hotter in consequence of the earth being in perihelion, not serving to melt all the snow and ice which had fallen and formed in the winter. These periods of great cold must have continued for a long time, and have recurred oftener than the five times which are indicated in the table. They must have continued during a very considerable portion of the time occupied in each precession of the equinoxes, which amounts to 21,000 years. They must have continued during a very considerable portion of the time occupied in each revolution of the apsides, which portion would be long enough to include many precessions of the equinoxes. There must, in fact, have been several periods which were no other than glacial epochs—times long enough for the flora and fauna to acquire an arctic complexion, and for beds of "till" and other signs of glacial action to be formed. I do not see how it

could have been otherwise. And if so, then the only conclusion which can be drawn from the absence of any signs of these epochs among the early fossiliferous rocks is that the time spent in the formation of these rocks fell far short of a million years—that in fact it did not reach back as far as one hundred thousand years, when, as is seen in the table, the orbit of the earth would for the first time exhibit a high degree of eccentricity.

Nor do I lose count of time when I attempt to ascertain the age of the carboniferous and calcareous strata.

Very possibly many ages may have been spent in the formation of the coal-seams. But to speak of many ages is not quite the same thing as to speak of innumerable ages. The forests which supplied the material for the coal-seams grew in times when the climate of the place was tropical or sub-tropical. They were composed chiefly of flowerless or cryptogamous plants and trees, of the families of fern, club-mosses, and horsetails, of which the growth may have to be measured by that of fungi rather than by that of shrubs and trees of a higher grade; and in fact I can find no conclusive reason for supposing that very many thousands of years should have been spent in this growth. It is also very possible that many coal-seams may be made up, not of vegetable material which has lived and died on the spot, but of vegetable material which has *drifted* there. On this supposition it is easy to account for the absence of roots and soil on the under surface of many coal-seams. On this supposition it is easy to see why

the coal-seams, like ordinary sedimentary rocks, should, as they do, split in a plane which is or once was horizontal. On this supposition it is easy to see why the coal-seams should be, as they are, parallel to the other strata above and below them. On this supposition it is easy to see why the coal-seams may, as they often do, attain a degree of thickness which is altogether inexplicable if the materials entering into their formation grew on the spot—grew, that is to say, long after the soil must have been utterly exhausted. Nor is it necessary to turn elsewhere in order to account for the "fossil forests" which are continually cropping up on the shores of the Bay of Fundy as the softer material in which they are imbedded is carried away by the wash of the sea. For what is the actual case in respect of these trees? It is that some of them have been uprooted and laid prostrate; it is that others, although erect, are without roots; it is that many, with roots pointing downwards, are rooted in rock which may have been sludge, but which could never have been soil. The case, in fact, is more explicable on the supposition that these fossil trees are not trees which have lived and died on the spot, but ancient "snags," which, like the modern "snags" of the Mississippi, have drifted from a distance to the spot where they are now buried.

Again, drifts may have had an important part to play in the formation of many calcareous strata. In the oolite the corals are often laid flat like the shells with which they are mixed, and in the marlstone bands met with in the lias of Yorkshire the oysters are chiefly single valves, many of them water-worn, and all of

them looking as if, like the corals and shells in the oolite, they had drifted into their present position. There is indeed nothing like a modern coral reef or a modern oyster bed in the strata which are almost entirely made up of the fossil remains of the creatures which once lived on such reefs and beds. And these cases are by no means exceptional. Nay, it is even possible that a good deal of the coral rag and white chalk and flint deposits now met with in the crust of the earth may have been formed in the course of a few thousand years if the coral polypes and microscopic globigerinæ and other foraminifera, together with the microscopic radiolariæ and diatoms, worked as hard in ancient times as they do now. Moreover, the work of forming chalk or flint may have been in great measure a simply chemical work, the material being deposited, perhaps rapidly, from water in which it had been dissolved previously, just as travertine, which is one of the forms in which limestone presents itself, is deposited quickly when the water in which it was dissolved by the help of free carbonic acid loses a certain portion of this acid.

To my mind also the mere presence of any fossil in which the petrification extends to soft and perishable textures is a fact which points very plainly to the speedy formation of the rock in which the fossil is imbedded. Only the rapid accumulation of the inorganic imbedding material could have preserved these textures from decay and dissolution. And this idea is emphasized by the distorted and shattered condition of the fossil remains of the icthyosauri and other large

reptiles in the trias, for this state of things is scarcely to be accounted for except on the supposition that these animals, alive or dead, were crushed under the thick and heavy coverings suddenly heaped upon the bed on which they happened to lie. In point of fact, by so regarding them, the fossils themselves in many instances supply abundant reason for believing that long ages were not spent in the formation of the fossiliferous strata.

Nor is the actual arrangement of the fossils in the rocks that which compels me to believe that while the rocks have been forming a process of evolution has been going on in plants and animals which could only be accomplished in countless ages.

The order in which the fossils are ranged in the several strata is very far from being in full accordance with the doctrine of evolution. In the earlier primary rocks (the lower and upper silurian) are certain cryptogams (algæ and lycopodiaceæ): in the later primary rocks (the devonian, carboniferous, and permian) are these and other cryptogams, together with very many conifers (which are flowering plants with naked seeds) and a single monocotyledon, the *pothocites grantonii.* In the secondary rocks (the trias and lias, and the oolitic and cretaceous groups) cryptogams in still greater number are met with, with conifers and cycads (the other family of flowering plants with naked seeds) in abundance, and several monocotyledons. In the tertiary and post-tertiary rocks (eocene, miocene, pliocene, post-pliocene, and recent) cryptogams, gymno-

sperms, monocotyledons and dicotyledons, all the leading forms of vegetable life now existing, are fully represented. In all the fossiliferous rocks are examples of all, or almost all, the invertebrata, of the highest types as well as of the lowest, all mixed together in a way which makes it impossible to say that the simpler forms are first in order. Some forms, like the crinoids, are more abundant in the primary than in the secondary and tertiary rocks; others, as the trilobites, are present in the primary and absent in the secondary and tertiary rocks; but the plain fact is evidently this—that examples of all, or nearly all, the invertebrata, mixed together almost indiscriminately, are met with in all the fossiliferous rocks. Nor is the case altogether different with the vertebrata. In the upper silurian, the devonian, the carboniferous, and the permian rocks, are plagiostome fishes (sharks and rays); in the two last named rocks, together with these fishes, are many ganoids, many labyrinthodont amphibiæ, and a few lacertilian reptiles, but no birds and no mammals. In the secondary rocks fishes are more abundant, reptiles, many of them of great size, and birds and marsupials are present, several marsupials being met with in the trias, or lowermost secondary stratum. The oldest fish (pteropsis) which is met with in the upper silurian rock, is by no means of the lowest grade; and Professor Owen, speaking of fossil fishes generally, says that the idea imparted by their history is that "of mutation rather than that of progression." And lastly, in the tertiary and post-tertiary rocks are examples, not only of fishes, and reptiles, and birds in plenty, but also

of almost all the mammals now existing, all mixed together as indiscriminately as are the invertebrata. It is indeed very difficult to find conclusive evidence in favour of evolution in these facts. The fossils of the tertiary and post-tertiary rocks plainly show that the plants and animals then existing were divided into the same great groups as those now met with, and the natural inference appears to be that in ancient times as now there was a system of things in which a vast crowd of dissimilar plants and animals had to minister to a common purpose, each organism having a part to play which was indispensable to the welfare of all the rest. And the gaps which are met with in the secondary and primary rocks do not necessarily show that the case was otherwise when those rocks were in course of formation. For the presence of these gaps may have to be explained, not by supposing that certain plants and animals are missing because evolution had not then had time to do its work, but by supposing that the material of the secondary and primary rocks, organic and inorganic, had been washed away from regions which were less richly provided with plants and animals than the regions which had supplied the material for the formation of the tertiary and post-tertiary rocks. Nay it is very possible that the order of deposition might have been other than it is—that the tertiary rocks might have been first or second in the series instead of third, for in order to bring about this alteration all that is necessary is to suppose that the three regions which supplied the material for the three orders of rock were co-existent, and that the change which led to the denudation of

one district before or after another was timed accordingly. At all events, until these doubts are set at rest it is impossible to point to the order in which the fossils are ranged in the fossiliferous rocks, and say that countless ages must have been spent in the formation of these rocks because countless ages are wanted for the purposes of evolution.

And do what I may I cannot persuade myself that these doubts are in any way set at rest by the presence among the fossils of intermediate or intercalary forms which have no living representatives now. The palæotherium to a certain extent was at once horse-like, and rhinoceros-like, and tapir-like. The anaplotherium in a feeble fashion was intermediate between pigs and ruminants. The iguanadon inclines towards birds on the one hand, and towards saurians on the other: and so does the archæopteryx. But I fail to find here any evidence in favour of evolution. I see plainly enough that these intermediate or intercalary forms may be only so many additional reasons for believing that the same archetypal plan is carried out everywhere. I see nothing to make me ready to believe that these forms are transitional—that they mark the passage of one form into another by way of evolution. Nor am I inclined to think that this statement is in any way invalidated by the geological pedigree of the horse as made out from American fossils by Professor Marsh. I cannot agree with Professor Huxley in thinking that this pedigree supplies demonstrative evidence in favour of evolution. It is by no means certain that the horse of the present day and of the days corresponding in time with the formation of the post-pliocene and recent rocks may be

traced back, first to the pliohippus and protohippus (hipparion) of the pliocene, then to the miohippus (anchitherium) and mesohippus of the miocene rocks, and lastly to the oldest member of the equine series yet known, the diminutive orohippus of the eocene rocks, with four complete toes on the fore limb and three on the hind limb, with well developed ulna and fibula, and with equine short crowned grinders of simple pattern. The differences in question are greater than those which are met with in the varieties producible in any species by domestication, or by climate, or in any other known way, and it may be going much too far to suppose that the horse is as closely related to the pliohippus, or protohippus, or miohippus, or orohippus, as it is to the ass or zebra, or quagga. The orohippus may indeed be nearly as far out of the true ancestral line of the horse as the hippopotamus. The horse is no doubt a perfect animal but there is no reason to suppose that the orohippus was less perfect. Man is not less perfect because his limbs are not hoofed like those of the horse, and why should the eocene fossil orohippus be in a different case? In fact, I cannot bring myself to believe that any creature, plant or animal, is imperfect, and for that reason in need of evolution. I cannot look upon any of these intermediate or intercalary forms as transitional, as the outcome of evolution; I can look upon them as supplying additional illustrations of archetypal unity: this, and this only, is all I can do.

And most assuredly a safe foundation for the doctrine of evolution is not to be found in the modifications which are brought about in plants and animals by artificial and

natural means, or in the embryonic development of any one of the higher animals.

Very extraordinary changes are brought about in many plants and animals by the interference of man, and in other ways, but none of them are such as to show that a lower species is perfected by being raised to the level of a higher species. In every case, so far as I know, the species themselves remain as far apart as ever; or if, in a few instances, certain closely allied species may intercross and produce hybrid forms, nature, as a rule, shows her disapproval by making these forms infertile. When left to themselves also the varieties in plants and animals which are produced by the interference of man or in other ways, and which are capable of propagating themselves in the ordinary way, are very generally in a hurry to revert to the ancestral type. The plant or animal has been shorn of some power by which it is able to live on indefinitely in a descendant exactly like itself. The garden rose has had its stamens and pistils transformed into petals: it is no longer fertile in itself: it must be propagated by the gardener artificially: and if it be not so cared for it quickly reverts to the wild type. The fancy pigeon, which has been produced by man by careful intercrossings, has lost none of its fertility, and there is no danger of extinction by reason of such loss, but if it be allowed to follow its own devices in mateing, its descendants soon don again the livery of the rock pigeon (*columba livia*), which pigeon is the common parent of all pigeons. And so with all other plants and animals whose natural growth has been interfered with artificially. Left to themselves the original

state of wildness soon gains the mastery; there is an element of weakness inherent in every variety; and therefore it may be doubted whether any variety in plant or animal, however produced, is really a change for the better—is really a step forwards in the direction of evolution. And this too is the lesson which is to be drawn from the history of the rudimentary creatures at the foot of the scale of being, whose very vitality is somewhat doubtful. Bacteria, among the simplest of living units, may, as Dr. Bastian points out so clearly, be developed (possibly from inorganic elements), almost at the will of the experimenter, into monads and amœbæ and paramæciæ, or into the lowest forms of fungi—into forms of animal life, that is to say, or into forms of vegetable life: but not much is to be built upon this fact in favour of evolution. For what follows? Simply this—that instead of passing on into higher forms of being, these forms are unstable in the highest degree, and always in haste to break up again into bacteria. The tendency to retrograde is, to say the least, quite as marked as the tendency to advance: and as respects evolution, the conclusion to be drawn is even that which has been drawn from the history of the rose and pigeon—this and no other. Look indeed where I may, I see enough to show convincingly that each plant or animal has its own part to play in nature, and nothing to make me suspect that any plant or animal is not perfectly fitted for its special work. I see plainly, in fact, that any movement towards evolution anywhere must derange, and may upset, the equilibrium of nature.

And certainly I find nothing in the history of em-

bryonic development to make me waver in this conviction. Certain passing resemblances to lower types of being are brought to light in this history, but this fact does not point to a state of things in which one creature may become another. Development always ending in the production of well defined differences is not to be confounded with evolution, even though the same path be travelled over up to a certain point. In fact the process of embryonic development has nothing to do with the process of evolution, and it is quite beside the purpose to speak of the two processes as being identical. The process of development may and does supply much additional evidence in favour of archetypal uniformity of plan in organic beings, and this, and only this, is what it does do. At all events, I do not see how the facts can be so twisted as to supply the evolutionist with conclusive evidence in favour of his own particular view.

A chief reason for believing in the extreme antiquity of the fossiliferous rocks is that which is supplied by the doctrine of evolution. Set aside this doctrine, and the reasons for entertaining a view altogether different to it retain all their force. So it may be: nay so it must be. And if so, then there is so far nothing in the history of the fossiliferous rocks to make it absolutely certain that immemorial ages have been spent in the formation of these rocks.

It is also difficult to believe that the cutting out of river-beds and river-gorges has always been a very slow process. Thus: "After the heavy rains which followed

the eruption of Vesuvius in 1824, the water flowing from the Atrio del Calvallo cut in three days a new chasm through strata of tuff and ejected volcanic matter to the depth of 25 feet, the chasm crossing the old mule path and making it impassable." Thus: "In many places in Georgia and Alabama land floods have given rise to recent ravines from 70 to 80 feet in depth in tertiary and cretaceous formations." Sir Charles Lyell, whose words are here quoted, also says, "When we are speculating upon the excavating power which a river may have exerted in any particular valley, the most important question is, not the volume of the existing stream, nor the present level of the channel, nor even the nature of the rocks, but the probability of a succession of floods at some period since the time when the valley may have been first elevated above the sea." What has had to be done may in fact have been done in a comparatively short period, even in such a case as that of the gorge below the falls of Niagara. For instead of believing that the falls have formed the gorge by eating their way backwards from the escarpment above Queenstown, where they were originally, to their present position, a distance of seven miles, either at the rate of one inch a year for 35,000 years, or at the more rapid rate of three inches a year for a third of this period, I find it more easy to believe that in the first instance the river found its way to the escarpment at a much higher level, in a bed of which there are distinct traces not far from the top on each side of the gorge, and that, afterwards, the gorge was formed, either suddenly, under the tearing-action of some terrible earthquake or flood, or else, less suddenly, under

the grinding-action of the glacier which covered the district for some time immediately before the advent of the present state of things as to climate. Indeed, do what I may, I can only allow that a very small part of the ravine now existing has been scooped out by the river slowly creeping backwards at the falls.

And nothing to the contrary is to be found in the important diggings and borings carried out under the direction of Mr. Horner at Heliopolis and Thebes (Phil. Trans., 1855 and 1858), with a view to ascertain the rate at which the alluvium of the Nile is deposited. The depth of the alluvium, or the soil which is regarded as alluvium, under which a monument of known date is buried, supplies the gauge; the particular case on which the argument is chiefly based being that of the partially buried pedestal once under the fallen colossal statue of Rameses II. at Memphis. Over the base of the pedestal lie 9 feet 2 inches of alluvium; under it are 36 feet of the same sort of soil. These facts, which are brought to light by the diggings and borings, are supposed to show that the Nile has been throwing down mud here for 13,000 years at the rate of 3·5 inches in a century, because this is assumed to be the rate at which the base of the pedestal has been buried in the years which have gone by since the erection of the statue in 1361 B.C. But in objection to this conclusion it may be stated, as was done by Mr. Samuel Sharpe, that the Egyptians were in the habit of protecting their temples and monuments from the annual inundations of the Nile by enclosing them with dykes, and that the mud in which

this particular pedestal is buried only began to accumulate at the doubtful date when the dyke around it was allowed to get out of repair. And this objection cannot be set aside as frivolous, for, by being thus protected, many of the temples and monuments which were seen by Herodotus were in hollows considerably under the level of the surrounding ground. Moreover, the conclusion drawn by Mr. Horner in this particular case is not borne out by the results of other diggings and borings, nineteen in number, carried out by himself at the same time in the same district, some of them in close proximity to the statue. Sooner or later at different depths the work of digging and boring was always stopped by the influx of water, the underlying rock being reached in no single instance. The soil taken out was of two principal sorts, an argillaceous earth or loam more or less mixed with sand, which is regarded as Nile sediment or mud, and quartzose sand which is called desert sand. Sometimes the quartzose sand was absent, sometimes it was below the argillaceous loam, once, with the exception of a thin layer of loam at the top, there was nothing else but it, and in three or four instances the column of loam was broken in more than one place by layers of this sand, some of them of considerable thickness. Evidently the quartzose sand could not be looked upon as a foundation for the loam; evidently the winds of the desert, sand laden, have brought sand to the spot at irregular intervals; and as evidently all the argillaceous loam cannot be looked upon as sediment from the waters of the Nile, for as M. Malley (quoted by Mr. Horner) points out, "Pendant

trois ou quatre mois de l'année la surface de l'Égypte dénuée de végétation, sèche et poudreuse, est balayée par des vents violents, qui soulevent dans les airs la poussière du sol, en laissant précipiter une partie dans le fleuve qui l'entraine à la mer, et en dispersant une autre dans les déserts, ou en l'accumulant sur d'autres portions de l'Égypte." It is indeed certain that some districts have been bared and others buried in this way; and therefore, instead of supposing that the depth of the soil in which the pedestal of the fallen statue of Rameses II. is buried is a gauge by which to measure the rate at which the alluvium has accumulated since the erection of the statue in 1361 B.C., it may be that this gives only a small portion of this depth. And besides all this, it is not improbable that a portion, perhaps a large portion, of the deep seated soil of the Nile valley may have been deposited, not from the waters of the Nile after the establishment of the present state of things, but from the waters of the sea while Egypt was a shallow marine estuary. At all events, in the case under consideration, there is nothing to show conclusively that the Nile has been depositing mud upon the land for 13,500 years.

And not more conclusive, as it seems to me, is the evidence which is thought to point to the existence of man upon the earth long before this period. Human bones, with works of art and several other indications of the presence of man, are met with in the post-tertiary (quarternary or pleistocene, and recent) formations, namely, loam, and loess of various sorts, fresh

water gravel and alluvium, peat mosses, sand dunes, and other superficial deposits ; and, from the order in which these relics are deposited, there is reason to speak of an age of stone as having been followed by an age of bronze, and of an age of iron as coming last of all. But it does not follow that the first of these ages reaches back to immemorial times, or that the last, which has not yet come to an end, had its beginning very long ago. It is even possible that Herodotus, if he had happened to wander in the right direction, might have had something to say about the men who lived in lake dwellings, and who left behind them such large kitchen middens, or dust heaps, as well as about the men belonging to the so-called bronze and iron ages whose remains are to be found in so many peat-mosses, and sand dunes, and barrows. And so with the relics of the same sort which are met with in beds of fresh water gravel and alluvium now many feet above the level of the river which was concerned in forming them—the beds which supply the chief data for supposing that man must have lived upon the earth for ages well nigh uncountable. There is, for example, no certain evidence that man existed very many ages ago in the traces of humanity which are met with in the terraces of loam and river gravel raised high up above the present level of the Somme near Abbeville. These traces would supply such evidence if the valley had been scooped out by the river at the slow rate of erosion now going on, but it is by no means certain that the valley was formed in this manner. On the contrary, it is quite possible that the valley may have been formed,

not slowly by the simple wash of the river, but quickly, either by the outburst of pent-up waters, or by the overwhelming grinding of the glaciers which would seem to have covered the district in the glacial epoch which ushered in the present state of things as to climate. The earlier deposits containing articles of palæolithic manufacture, with the bones of the mammoth, the hairy rhinoceros, and other extinct animals, and with, in addition, a human bone here and there, as well as the later deposits containing articles of neolithic manufacture, with various other rude works of art, together with the bones of dogs, and hogs, and horses, and sheep, and short-horns (of domesticated animals, that is to say, which do not belong to the indigenous fauna of Europe, and which may have been brought from Central Asia by a higher race of man in some migration), and with several kinds of grain and fruit-seed, also not indigenous, and with many bones of man, are all found in the post-tertiary formations which are contemporaneous with the glacial times which ushered in the present state of things as to climate; and all that can be said with certainty at present is that man lived upon the earth in these glacial times, and that these times may after all be post-diluvian. At all events it is not difficult to explain the presence of man at the time when the terraces of gravel and loam in the valley of the Somme near Abbeville were formed high up above the present level of the river, without supposing that man lived in this valley or elsewhere in immemorial times.

In the stratification of the present land it is evident that this land was formed under water, and raised into its present position afterwards, but it is not so evident that the land generally has been in and out of the water several times. This up and down movement may have happened more than once in the case of some of the coal-seams; in all probability it did so happen; but after what has been said about the action of drifts in the formation of these seams, it is difficult to believe that coal often marks the site of an ancient forest which has lived and died on the spot. Indeed, so far as I can see, the evidence generally points towards quickly forming drifts rather than towards slowly growing forest or scrub. And as in this case, so in that of the strata built up in the main of the remains of animals which are supposed to have lived and died on the spot, all the facts may be so read as to convey the same lesson. Nor can it be said that there is anything in the remarkable contortions of many of the strata to show that the strata have been in and out of water several times. For the most part all the strata, contorted or not, as the case may be, lie conformably one above the other in the same order, and on looking into the matter with any degree of care, it seems to follow almost as a matter of course that all the strata may have been formed without any break in the same period of submersion. And with respect to the duration of this period, all I can say is that I am more than ever disposed to shorten it, for in addition to what has been already said in opposition to the notion that interminable ages have been spent in the work of stratification, it remains to say that the ancient earth may have

been much looser in contexture than the present earth, and that, for this reason, the formation of the strata of the present earth, as far as the inorganic constitutents of the strata is concerned, may have been greatly hastened. Indeed, I cannot help fancying that the rocky character of the present earth is in the main attributable to the action of the heat and pressure which were brought to bear upon the strata from below when the bed of the ancient sea was converted into the present land. And, in short, if anyone were to say that the fossiliferous rocks had been formed in historic times, I should not venture to contradict him.

Nor do I want much time in order to account for the cold which ushered in the present state of things. All I want in the first instance is the time necessary to freeze the waters of the sea, as sooner or later they must have been frozen, in a state of universal deluge, and, so far as I can see, a few months at most will serve for this. For after what has been said it may be supposed that the great revolution in which the bed of the ancient sea was converted into the present land, may have been completed in a single year, and that the state of universal deluge which would happen when this revolution was midway between the beginning and the end, would be comprised within this short period. And all I want afterwards is time enough for the tremendous glaciers which in the first instance covered the present land in a way in which it is not covered now to melt away—a time which would not and could not be very long, because the sun would be then

acting upon the earth as at present. Indeed, I find no occasion to call in the help of ice-epochs connected with many precessions of the equinoxes and many revolutions of the apsides, each of them lasting thousands upon thousands of years, and each separated from the rest by thousands upon thousands of years, in order to account for the cold of arctic severity which ushered in the present state of things as to the climate.

And, finally, I do not find it necessary to believe that countless ages have been spent in the formation of the igneous and other unstratified rocks which underlie the fossiliferous rocks. I cannot bring myself to believe that these rocks were once in a state of fiery mist, and that ages without number had to pass by before they became solidified by cooling. I can easily believe that the universe was cold, and dark, and probably motionless, before the day when the various orbs in space, as *bodies*, began to interact electrically as they do now, and that this day, instead of being immeasurably distant, may only mark the commencement of historic time. And I am, I think, much the gainer by thus changing the point of view from heat to electricity. I can see how electrical repulsion may do the work of heat in keeping molecules apart. I can see how electricity may pass from the statical into the current state if only the interacting bodies in which the electricity originates are of different degrees of potential, the current passing from the body at the higher potential to the body at the lower. I can see how current electricity may be frittered down into heat by resistance, and how there may be no heat when

no resistance is opposed to the passage of the current. I do not want rays of heat; I only want electrical force moving in lines between two or more bodies, and the movement impeded by a certain degree of resistance where heat is developed, and there only. And I can even imagine that heat as heat has its seat only in the nervous apparatus which takes cognizance of heat. And as with heat so with light, I can see that there may be the same electrical basis, and that electricity may be frittered down into light as well as into heat by resistance; for the fact that the velocity of light is much less than that of electricity may help to show that light is electricity bridled down by resistance. I do not want rays of light; I can do without luminiferous ether; I only want the electrical relations between different bodies which are exhibited in the laboratory, and liberty to believe that the lines of electrical force between two or more bodies (which lines imply the existence everywhere of a medium which is perhaps a shade less subtle than luminiferous ether) are bright where they are frittered down into light by resistance, and dark elsewhere. Indeed, as with heat so with light, I can imagine that light as light has its seat only in the apparatus of vision. Nay, I can dimly see how a key to gravitation and to tangential movement may be found by seeking for it in the very same lines of electrical force which serve as a basis for heat and light. I can see how gravitation may be nothing more than the movement in the current of a body at a higher potential to a body at a lower potential. I can see how the tangential movement, which may have to act as continuously as gravitation,

and which must so act, if, as it would seem, space is pervaded with a medium capable of opposing any degree of resistance to the movements of the earth and heavenly bodies, may be nothing more than the movement of a body in the magnetical current which passes across the electrical current—which passes, it may be, not around this latter current, but across it. By giving rein to my fancy in this manner, I become more and more unbelieving in the "nebular hypothesis," and in the consequent notion that countless ages have been spent in the formation of the igneous and other unstratified rocks underlying the fossiliferous rocks, and more and more inclined to believe that the universe was cold and dark and forceless as well as formless before the day when the various orbs in space were formed and placed in position, and when they began, *as bodies*, to interact electrically as they do now, and that this day, instead of being immeasurably distant, may only mark the commencement of historic time.

CHAPTER III

A NEW DEPARTURE IN WHICH THE ACTUAL AND SCRIPTURAL HISTORY OF THE EARTH ARE FOUND TO BE NOT WHOLLY IRRECONCILABLE.

THE guiding voice to which I am compelled to listen when I try to understand what is told in Genesis about the past history of the earth and heavenly bodies is not that which gives the word of command to the scientific train-bands in the regulation uniform of the present day. It does not tell me to take part on either side in the brilliant controversy in which Mr. Gladstone and Mr. Huxley have been recently engaged. It does not tell me to try to find an account of the days of creation in the "records of the rocks," or to pin my faith either to the "nebular hypothesis," or to the doctrine of organic evolution. It tells me to believe that the universe was cold and dark and forceless as well as formless before the day when the earth and the heavenly bodies were formed and placed in position, and when they began, *as bodies*, to interact electrically as they do now, and simply to read the words of Scripture in accordance with the premisses.

"In the beginning God created the heaven and the earth. And the earth was without form and void; and darkness was upon the face of the deep." The story so far is quite consistent with the notion that the universe may have been cold and dark and motionless before the day when the earth and heavenly bodies

were formed and placed in position, and when, *as bodies*, they began to interact electrically as they do now. Nay the universe may have been empty also.

"And the Spirit of God moved upon the face of the waters." Waters! what waters? Not waters on the surface of the earth simply: that is evident. The firmament which divided the waters from the waters is not the terrestrial dry land, for the dry land did not appear until the morning of the third day: and hence the waters in question must mean more than separate seas. The firmament is not the atmosphere, and the waters above it the clouds, as Mr. Ruskin imagines, for the firmament, which is called heaven, is afterwards described as the place where the sun and moon and stars are set for signs, and for seasons, and for days, and for years. And so it may be that the *waters* above and below the firmament are the sun and moon and stars on the one hand, and the earth on the other, all these bodies being in fact covered with water in the first instance. But be this as it may, there is no room for doubt as to what was the condition of the earth at this time. Up to the third day this was that of a shoreless ocean. Morning twice dawned and evening twice fell upon a land-less waste of water. On the third day, and not until then, "the waters under the heavens were gathered together into one place, and the dry land appeared." As to the meaning of the words there can be no mistake.

"And the Spirit of God moved upon the face of the waters. And God said, Let there be light: and there was light. * * * And God divided the light from the

darkness. And God called the light Day, and the darkness Night. And the evening and the morning were the first day." The light was the light of day, for it was separated from the darkness, which was night, and therefore the earth must have been rotating on her axis from the first, light and motion, of this sort at least, then going hand in hand as now. Nor is there anything contradictory to this conclusion in the statement that the *lights* which were to divide the day from the night, and to be for signs, and for seasons, and for days, and for years, did not appear until the fourth day. The heavenly bodies were not created on this day: they appeared as *lights* on this day. Until then these *lights* may have been hidden by the mist "which went up from the earth and watered the whole face of the ground," as it did on the day when the earth was covered with vegetation, which day was the third day. Or the heat of the ring of subterranean fire which was then beginning to cause the earth to bulge out from the form of a sphere into that of an oblate spheroid flattened at the poles, and the land to rise above the waters, may have made the *lights* invisible by filling the air with dense clouds of watery vapour. There is nothing inconsequent in the story so far.

The separation of light from darkness is very significant. The light is the light of day, the darkness is night. The earth is evidently rotating on her axis at the time. The light is connected with motion, and it may also be connected with electricity and magnetism and heat, the connection being that which is set forth so clearly in the Doctrine of the Correlation of the Physical

Forces. And in the rising of the land above the water which soon follows upon the creation of light it is also possible to find further evidence of the connection between light and heat, and between light and another manifestation of motion, for, according to the premisses, this rising is caused by the heat of the ring of subterranean fire of which the development is concurrent with that of light. Indeed, there is nothing in these statements which is inconsistent with the belief that the universe was cold and dark and motionless before the day when the earth and heavenly bodies were formed and placed in position, and when they, *as bodies*, began to interact electrically as they do now. And without irreverence it is even possible to go further and believe that the physical way in which the Spirit of God moved upon the waters may be no other than that which may be said to be shadowed forth in the Doctrine of the Correlation of the Physical Forces.

Plants of all kinds are created before animals, and animals of all kinds before man, and of them it is said, "and God saw everything that He had made, and, behold, it was *very good*." There is nothing to support the doctrine of evolution from a state of imperfection, with one very rudimentary form as the starting-point for all plants and animals and man, or with a few such points. Creation is carried out speedily, and in a given order, and why not? Innumerable ages are not wanted for the purposes of creation. Indeed, if there be room for wonder, the wonder is that the Creator, with whom a thousand years are as one day, and one day as a thousand years, should have spent so long

a time as six days in doing what is said to have been done on these days. But it does not follow that the living creatures which thus sprang into existence at the word of command on the days of creation should be unaffected by evolution. On the contrary it may be allowed that they respond to the modifying action of natural causes to a very considerable extent, though not to the extent which would satisfy an ultra-evolutionist. It is certain that many plants and animals now existing differ very decidedly from the fossil plants and animals of which they are the lineal descendants. It is highly probable that important *varieties* have been produced in the course of time, and that "the struggle for existence" and "the survival of the fittest" may have largely contributed to this result. But nowhere do I find any sufficient reason for believing that variations in any one species have ever been carried so far as to obliterate the essential differences between it and any other species, or genus, or order, or class. Nor do I find anything unscientific in what is said about the work done on the days of creation, or about the vegetable diet of man and animals in the first instance, or about the pre-eminence of man. The arrangement of the fossils in the fossiliferous rocks, be that what it may, can, as I believe, have nothing to do with the days of creation, and, therefore, this to me does not supply a reason for saying that the work said to be done on the days of creation was not done on these days. Indeed, so far as I know, there is no reason why the account of this work may not be taken simply as a narrative which has nothing to do with science. And it is not altogether

incredible that man himself and all animals should have found their food in the plant-world in the first instance, for the very existence of herbivorous animals is in itself a sufficient proof that the materials necessary for the formation of their tissues are met with within this world. And, as is well known, there is much to be said in favour of the pre-eminence of man—but this topic I must pass by as one which is too large to be dealt with incidentally in this place.

And with the same key it is also possible to pass on far enough to be able to see in the story of the deluge a continuation of the story of the creation. How strangely circumstantial is this story! This catastrophe is slow and orderly, as if the causes at work were natural causes: it was also predicted—as an eclipse might have been. "In the second month, the seventeenth day of the month, the same day were all the fountains of the great deep broken up, and the windows of heaven were opened. And the rain was upon the earth forty days and forty nights. * * * And the flood was forty days upon the earth. * * * And the waters prevailed exceedingly upon the earth: and all the high hills, that were under the whole heaven, were covered. Fifteen cubits upward did the waters prevail; and the mountains were covered. * * * And the waters prevailed upon the earth an hundred and fifty days. * * * And the waters assuaged. The fountains also of the deep and the windows of heaven were stopped, and the rain from heaven was restrained. And the waters returned from off the earth continually: and after the end of the hundred and fifty days the waters were abated. And

the ark rested in the seventh month, on the seventeenth day of the month, upon the mountains of Ararat. And the waters decreased continually until the tenth month; in the tenth month, on the first day of the month, were the tops of the mountains seen. * * * And in the second month, on the seven and twentieth day of the month, was the earth dried."

And so it may well be if the deluge was brought about in the way to which I have directed attention, that is, by the axis of the earth having been moved from a position in which it was perpendicular to the plane of the ecliptic into the position it now occupies, and by the changes in the development of subterranean heat consequent thereon. The case is indeed that upon which I have already dilated sufficiently (p. 75).

The fountains of the great deep are broken up and the windows of heaven opened on the seventeenth day of the second month, that is—the Jewish year beginning on the 20th of September—on the 6th of November. For forty days and forty nights, that is, until the 16th of December, which is very near the time of the solstice, the waters go on rising, and then, after prevailing for one hundred and fifty days so as to cover the highest hills to the depth of fifteen cubits, they begin to subside. The ark grounded on the top of Ararat on the seventeenth day of the seventh month, that is on the 27th of March: the tops of the mountains are seen on the first day of the tenth month, that is, on the 20th of June, the day before the solstice: the earth is dried on the 27th day of the second month, which is the 16th of November. The catastrophe occupies very

nearly an exact year, beginning and ending in November at dates only ten days apart.

The fountains of the great deep are broken up and the windows of heaven opened on the 6th of November, and rain pours down in torrents for forty days and forty nights. The words do not merely point to the land as being flooded under a mighty downpour of rain: they also point to a breaking up of the fountains of the great deep, which may mean that the ancient land was mainly submerged by a raising of the bed of the ancient sea, which raising began on the 6th of November, and went on for a year, and which, in accordance with the premisses, may have been caused by the axis of the earth having been moved on this day from its original position into that it now occupies.

The rain was upon the earth forty days and forty nights. It would seem as if, in causing the deluge, the rain played an insignificant part in comparison with that played by the raising of the bed of the sea. It would even seem as if the rain, which went on for forty days and forty nights, ceased at the end of this time. And why? Did the sea overlying the rising land during these forty days and forty nights, simmer under the heat proceeding from the land, and may rain have fallen in torrents during this time in consequence of the sky being overloaded with steam? Did the rain cease at the end of these forty days and forty nights, because, owing to the land being then wholly or in great measure under water, the reign of ice had commenced by which the ice-caps of the poles were eventually drawn over the earth until their edges were on the point of meeting at the

line—a state of things which might very well prevent the formation of rain. I see no reason why it may not have been so; indeed, after what has been said, this conclusion would almost seem to be that which follows naturally from the premisses.

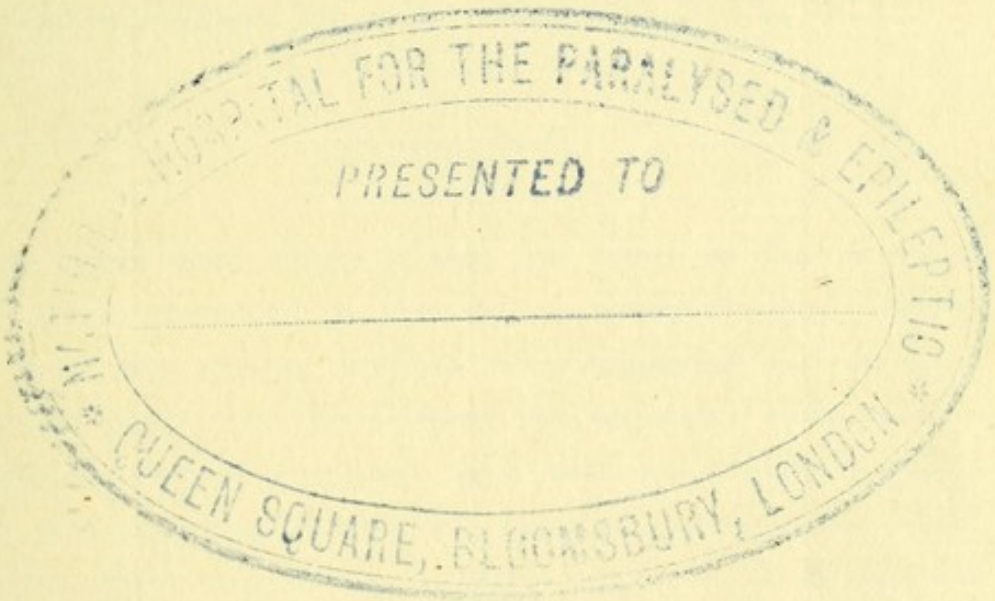
Nor is there anything inconsequent in the remainder of the story of the deluge. On the contrary, after what has been said, it is easy to go on far enough to see, not only that this story is consistent with itself from beginning to end, but also that, in addition to describing the deluge, it may also describe how the bed of the ancient sea was converted into the present land. Nay it is not difficult to find what would seem to be a crucial reason for believing at once in the truth of the biblical story of the deluge and in the truth of the premisses in the fact that the time of which the story takes account, a year only, is the very time which is required by the premisses to bring about the same result.

And it may also be quite in order that there should have been no rain and no rainbow before the deluge. Before the deluge there may have been no land to speak of except in equatorial regions. Before the deluge the land may have been rainless and bow-less, because it was always exceedingly hot. Before the deluge, indeed, the vapour arising from the hot land, which now may fall as rain close at hand, might rise into the air high enough to meet with the currents which would carry it polewards, where, whether it fell as rain or hail or snow, it would fall far out of sight of land. Nor is a fatal objection to this view to be found in the "rain-prints" left on the slab of arenaceous

green shale which was found at Cape Breton in Nova Scotia—a slab which belongs to the carboniferous system of rocks. It is certain that this slab was formed long before the revolution which converted the bed of the ancient sea into the present land: it is not certain that the rain-prints so-called were really caused by rain. Along with these prints are marks which are evidently worm-tracks. The slab, almost as evidently, was once a muddy shore where the worms which left these tracks had their dwelling, and who can say with certainty that the rain-prints so-called may not have been caused by splashes from a breaking wave, or waterfall? There would be no rainbow proper in this case. At all events, it is not possible to say that the absence of rain and rainbow in antediluvian times is altogether inconsistent with the premisses.

What then? Am I at liberty to accept the biblical history of the earth as the actual history? Am I to believe that the elder stratified rocks were formed in the comparatively short interval between the creation and the deluge? Am I to believe that in the great revolution in which the bed of the ancient sea was lifted up until it took the place of the present land there was an intermediate stage in which this deluge happened, as a matter of course, in the way pointed out? Am I to believe that the signs of glacial action, including the great denudations of the chalk and the tertiary strata, together with the scooping out of wide and deep valleys, point to a comparatively short period in which the earth was ground down by the tremendous glaciers

which were called into existence by the cold of the deluge, and which continued to act until the melting of the ice over the new land led to the introduction of the present state of things as to climate? Whether I am right in thinking, as I do, that affirmative answers ought to be returned to these questions remains to be seen. I cannot venture to think that I have proved my case. I only venture to hope that I have been dealing with a working hypothesis of which I need not be ashamed.



WORKS BY THE SAME AUTHOR.

VITAL MOTION

AS A MODE OF PHYSICAL MOTION.

BY

CHARLES BLAND RADCLIFFE,

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"Le nom de *Galvani* ne périra point ; les siècles futurs profiteront de sa déeouverte, et, comme le dit *Brandes*, 'ils reconnaîtront que la physiologie doit à *Galvani* et à *Harvey* ses deux bases principales.'"

A. VON HUMBOLDT.

Second Edition, 8*vo.*, 7/6. *Macmillan & Co.*, 1882.

SUMMARY.

The view here taken is that the coats of the muscular fibres while alive and at rest are so many charged Leyden jars, that this charge inhibits or antagonizes the state of muscular contraction by keeping the muscular molecules in a state of mutual repulsion, and that contraction happens when this charge is discharged, because the muscular fibres are then given over to the attractive force which is inherent in the physical constitution of the muscular molecules. The discharge, analogous to that of the Torpedo, which accompanies muscular contraction, and which appears to be only the faradaic current which attends upon any sudden movement of electricity, and any other form of electrical

discharge, are supposed to produce muscular contraction, not by acting as a *stimulus* to a vital property of contractility, but simply by discharging the charge which inhibits contraction, and which returns the moment the muscle is left to itself. This is the supposition respecting ordinary muscular contraction but not respecting rigor mortis, for here the muscle is supposed to stiffen slowly and permanently, simply because the charge which keeps up the state of muscular relaxation has departed slowly and permanently. And all that may be said about muscle may also be said about nerve, except this, that in nerve, probably for the same reason as that which puts an end to amœboid movements in granular amœbæ, the state of action is not accompanied by contraction. The case indeed is altogether opposed to the notion that muscle and nerve are endued with a vital property of irritability, and that muscular contraction and nervous action are brought about by the excitation or stimulation of this property, for everything goes to show that muscular contraction and nervous action are inhibited by electrical charge and produced by electrical discharge,—that the workings of nervous influence and blood and other so-called stimuli upon muscle and nerve may be resolved into those of electrical charge and discharge,—that involuntary or voluntary muscular contraction or sensation in excess is produced, not by the over-action of a "discharging nerve-centre," but mainly by the *inaction* of certain nerve-centres of which the action is to inhibit muscular contraction and sensation,—that convulsion and spasm and tremor and neuralgic trouble betoken, not vital intensification, but a state the very opposite to this, even a step towards actual death,—and that, in fact, a great revolution is needed in the treatment of all the disorders in which the muscles and nerves are prone to pass from the state of rest into that of action

PROTEUS,

OR

UNITY IN NATURE.

„Die Geisterwelt ist nicht verschlossen;
Dein Sinn ist zu, dein Herz ist todt!
Auf, bade, Schüler, unverdrossen
Die ird'sche Brust im Morgenrothe."

Goethe (Faust).

„Ich nicht auf Menschen baue, sondern auf den Gott in mir und über mich."

Jean Paul (Titan).

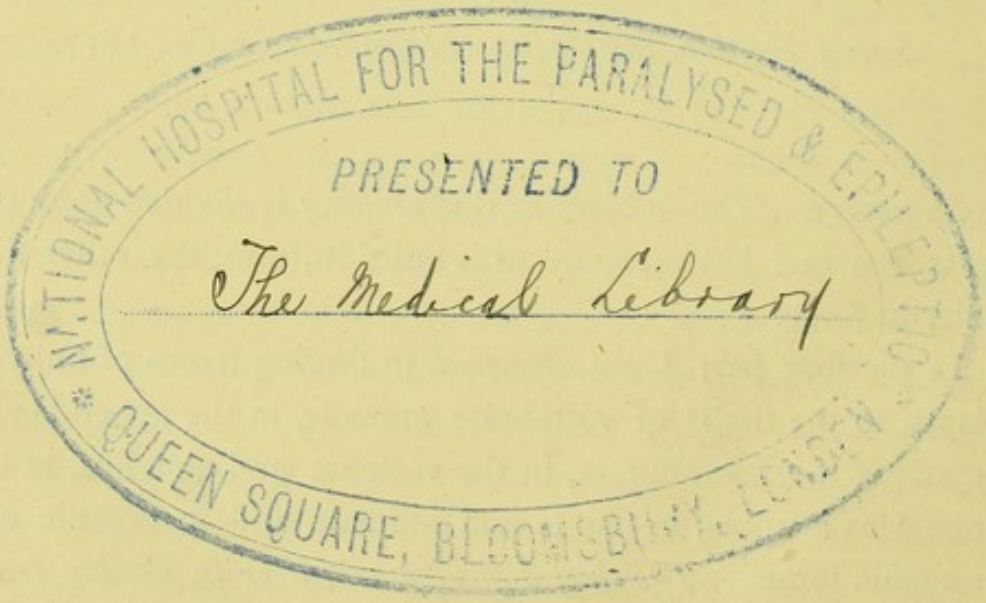
Second Edition, 8*vo*., 7/6. *Macmillan & Co.*, 1877.

Summary. The subject in this enquiry is divided into two parts, the one having to do with unity in form, the other with unity in force.

In the first part I am occupied in finding traces of unity in plants, in the limbs of vertebrate animals, in the appendicular organs of the invertebrata, in the vertebra and annellus, in the animal as a whole, in plants and animals, and in organic and inorganic forms. *The chief difficulty, which is in finding traces of unity in the vertebra and annellus, is met by regarding the body of the vertebra, not as something single and central but as a com-*

pound structure formed by the coalescence of two lateral elements, each forming part of a bony ring or annellus, which part is a rudiment of some limb or other appendicular organ, the process at work in forming the body being analogous to that which brings together two lateral columns into a single cerebro-spinal axis, or two lateral vessels into a single aorta.

In the second part of the enquiry I occupy myself in seeking traces of unity in various modes of physical force, in vital motion, in the vivifying power of light and heat, in the phenomena of instinct, and memory, and imagination and volition and intelligence, and soul, the side taken when the enquiry becomes metaphysical being that which would be taken, not by the many who now-a-days listen to the opinions of Aristotle as incontrovertible, but by the few who take Plato for their master. In a word, every step in the latter part of the enquiry makes me less ready to agree with those who believe in matter, or ὕλη, as the basis of being, and who, by pinning their faith to the doctrine of evolution, see chaos rather than cosmos in the present order of nature.



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