IV. *On the Revolutions of the Earth's Surface.* By Sir JAMES
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PART I.

[*Read March 16. 1812.*]

WE are never more disposed to give credit to a philosophical system, than when we meet with a case of its successful application, unknown to the author, or containing circumstances which he had not taken into account when he formed that system.

The facts brought forward in the following paper, which, according to my view of the subject, clearly indicate the operation of immense torrents, can be accounted for, I think, in a very satisfactory manner, by the Huttonian Theory, and consequently afford some very powerful arguments in support of it.

But such was by no means the view taken of this subject by Dr HUTTON himself, or by Mr PLAYFAIR, who, since his death,

has so ably illustrated and maintained his geological opinions. These gentlemen have expressed themselves, on all occasions, in a manner peculiarly hostile to the employment of such torrents as geological agents, believing that all the phenomena may be traced to the influence of diurnal causes only.

I have no hesitation in declaring my hearty concurrence in what I consider as the essence of the Huttonian Theory ; I mean as to all that relates to the influence of internal heat in the formation of our rocks and mountains : But I could never help differing from Dr HUTTON, as to the particular mode in which he conceived our continents to have risen from the bottom of the sea, by a motion so gentle, as to leave no trace of the event, and so as to have had no share in producing the present state of the Earth's surface.

At an early period of life, while I imbibed from the delightful conversation of my worthy friend Dr HUTTON, the spirit of his geological views, I retained my attachment to opinions suggested by M. DE SAUSSURE's observations, which, at a still earlier period, I had acquired in the Alps, and which had been rivetted in my mind by the sight of the phenomena from which they have been inferred. The facts also observed in the Russian empire, and brought forward by Professor PALLAS, relative to the productions of the tropics, which are found upon the banks of the Frozen Sea, appeared to me of sufficient force to justify his belief, that, at some remote period, a torrent of water had swept across the continent of Asia. I have, therefore, been always disposed to combine the doctrines of HUTTON with those professed by the gentlemen just named, relative to marine inundations ; and a number of facts which I have observed in this country, will contribute, I hope, to throw some additional light upon this difficult subject.

Before

Before mentioning my own observations, I beg leave to lay before the Society the state in which I found the question. The central ridge of the Alps, of which Mont Blanc is the principal mountain, consists, as is well known, of Granite. Nearly parallel to it, and at a distance of fifty-four miles to the north-west (by Sir GEORGE SHUCKBURGH'S measurement) lies the mountainous ridge of Jura, fifty or sixty miles in length, composed entirely of calcareous matter. Between these two ridges are interposed the valley and lake of Geneva, and other valleys and inferior hills. The native place of the granite is confined to the central ridge; but over the surface just described, innumerable detached granitic blocks are very irregularly scattered, which have originated, as we must presume, in that central ridge, and have been transported to their present position by some mechanical power.

Upon the southern side of the Baltic, a multitude of similar blocks are found, also scattered irregularly over a sandy district, of which a particular and detailed account is given in M. DE LUC'S Geological Travels, vol. i.

The origin and history of such blocks, which occur in various other places, have given rise to considerable discussion; and the question is inseparably connected with other points of magnitude in geology,—such as the formation of valleys and lakes, and the distribution and arrangement of various beds of clay and gravel, and of all the loose and alluvial assemblages which occur under various forms in all parts of the globe, and constitute its most valuable districts in point of fertility.

M. DE SAUSSURE ascribes the transportation of the granitic blocks on the Alps to the action of an immense torrent of water, or *Debacle*, as he terms it, which at some remote period, flowing over the summit of the Alps, had carried these blocks along with it.

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Dr HUTTON and Mr PLAYFAIR, as I have already said, deny the necessity of introducing such an agent, since the circumstances, as they conceive, might have been produced by the usual action of rivers. But this simple view seems to be excluded, when we consider both the magnitude and the positions of these blocks. Their size, in some cases, amounts, as in the valley of Monetier upon Saleve, to 1200 cubic feet, and in the case of those on the Coteau de Boisy, to 2250, and even to 10,296 cubic feet, which is the measure of the block called Pierre à Martin.

To move a mass of granite of even fifty or sixty cubic feet, and to carry it a few yards, would require the utmost efforts of the Rhone or the Arve, as they flow near Geneva, in their highest floods, but that such blocks could be conveyed by one of them along its whole course, is contrary, I conceive, to all experience, and still more when we consider that these rivers are divided at their source from beneath the Glaciers into forty or fifty small streams. Yet from the Glaciers, these blocks must have come; and when we take into account the magnitude of some of the granitic masses, it is clear that the task is beyond the power of any river that flows on the surface of the earth; nay, it seems more than water, under any predicament, could accomplish, and more than could be expected from the *Debauche* itself, however extravagant its magnitude may appear: but we shall again return to the subject, and shew in what manner this difficulty may be explained.

These stones do not lie merely in the beds of rivers, but occur all over the country, and on the summits of mountains, where rivers could least be conceived to have flowed; nor are they confined to that side of the country, or to the side of the lake of Geneva which lies next the Alps; for we find them in particular on the face of Jura, which fronts the central ridge
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and the lake, at an elevation of 2000 feet above the latter. A set of low hills also intervene, which occasionally hide that central ridge from the view ; and it is principally where the snowy summits are visible from the face of Jura, by means of some depression in these intervening hills, that we find those travelled masses ; as I remember well to have witnessed, at some of the places which SAUSSURE has pointed out, where, in high situations, on the face of Jura, I rode through great assemblages of granitic blocks, three or four feet in diameter.

The force of this fact is admitted, but an attempt is made *, even under that admission, to refer the whole to diurnal actions, by supposing, that from the spot where these blocks lie, up to the summit of Mont Blanc, one continued solid plane has ascended, along which, on a declivity computed at one in thirty, these blocks may have been hurried by a stream of water ; and that subsequently, in the course of ages, all the intervening mass had been washed away, so as to reduce the country to its present situation. But this hypothesis removes the difficulty of the intervening valleys only ; for the transportation in this case would be scarcely less difficult than it would be to Geneva, as matters stand at this day ;—and a circumstance occurs, founded upon the observations of Sir GEORGE SHUCKBURGH, which seems entirely to preclude this hypothesis.

According to his measurement and scale, as given in the Philosophical Transactions, vol. lxvii. the height of Mont Blanc is 14,432 feet above the lake, and the distance in a straight line from Jura to Mont Blanc, I find, by his scale, to be nearly fifty-four miles, and 2000 feet is the height at which these granite blocks occur on Jura. The slope, therefore, along which these blocks must have descended, would be nearly that of one in

* *Illustrations*, art. 345, page 385.

in twenty-three. But without inquiring whether or not this declivity would be sufficient for the purpose, it is of consequence to attend to another circumstance pointed out in these measurements. The line above which snow lies perpetually, during all summer, is there noted, and lies by the scale at 7500 feet perpendicular below the summit of Mont Blanc. It is easy, then, to calculate to what horizontal distance from the centre of the ridge, this limit of perpetual snow would extend, and we thus find it to be 32 miles *. But SAUSSURE has found the junction of the granite with the surrounding strata at the *Buet*, and by the same scale, I find the distance of this mountain from Mont Blanc is about 10 miles. It is obvious, then, that in the supposed situation of the Alps, in which the granitic

* Let AD be Jura, and BC Mont-Blanc. Let D be the place on Jura where the blocks lie; the line DC will be the outline of the supposed surface. Let AB be drawn horizontally at the level of the surface of the lake, and DE at a level 2000 feet higher, meeting BC in E. We have then $AB = DE = 54$ miles $= 285120$ feet; and $CE = CB - EB = 14432 - 2000 = 12432$ feet. The measure of this declivity along CD may therefore be easily obtained. Thus, $CE : ED :: 12432 \text{ feet} : 285120 :: 1 : 22.9$, nearly one in twenty-three. Mr PLAYFAIR states it at one in thirty; but he has reckoned to the summit of Jura; whereas the blocks under consideration lie at some distance below that summit.

Let CF be equal to 7500 feet, and let FG be drawn horizontally; then G will be the lowest point of perpetual snow upon the supposed surface. The horizontal distance of G from the centre will thus be obtained, $CE : CF :: ED : FG$, or $12432 : 7500 :: 54 \text{ miles} : 32\frac{1}{2} \text{ miles}$. Let H be the extreme point of the granitic mass at the *Buet*, and let HK be drawn vertically; the point K will denote the limit of the granite upon the supposed surface.

nitic mass could not be conceived to extend farther than it now does from the middle, that the whole granitic surface must not only have been buried in everlasting snow, but that the extent of this snow must have been three times that of the granite. Now there is every reason to suppose, since the temperature diminishes as the height increases, that within this upper third, the temperature has never, in the hottest day in summer, reached so high as 32° ; that is to say, that water has never there existed in a liquid form. We cannot, then, conceive any block, however small, to have been conveyed from thence by means of water, acting by the usual diurnal laws.

M. DE LUC admits the reality of M. DE SAUSSURE's debacle, and accounts for it in the same manner; but he does not ascribe to it, either in Germany or in the Alps, the transportation of the granitic blocks, the presence of which he accounts for very differently. He supposes that these blocks have nowhere migrated along the surface, but have been ejected from below, at the places where they now lie. This ejection he produces by a very extraordinary hypothesis *, founded entirely on gratuitous suppositions, and which affords no plausible solution of the difficulty, by which we might be tempted to adopt it.

M. DE LUC mentions a theory formed by Mr WREDE, a Professor of Berlin, to account for the same blocks. He embraces a belief which seems to have been in considerable favour, that the level of the Baltic Sea has been sinking for ages past, and he extends his belief of the same change taking place in the

VOL. VII.

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* He supposes, that below the firm crust of the earth, then lying horizontally at the bottom of the sea, a set of caverns once existed, filled with peculiar elastic fluids. That at a certain period, this crust breaking to pieces, fell with great violence into these caverns, and drove out the elastic fluids contained in them with such impetuosity, that the blocks of granite under consideration, which then constituted portions of the lower part of the crust, were forced upwards into the air, and fell back into their present position.

whole of the North Sea. His idea is, that a change has been gradually going forward in the position of the centre of gravity of the earth, which has been moving to the southward, and that an equivalent rise is experienced in the southern hemisphere. That when the North Sea stood at its highest level, these granitic blocks had been transported across the Baltic, by means of the winds, on floats of ice, and settling in their present places, had been left by the retiring waters.

M. DE LUC, in a very satisfactory manner, in my opinion, refutes one essential part of this system, by shewing, from the form of the new lands, and from other circumstances, that no change in level whatever has taken place on the surface of that sea for many ages back, and, indeed, ever since the surrounding country has possessed its present form. As to the other part of his system, which relates to the transportation of these blocks by means of ice, we shall soon have occasion to resume its consideration.

Besides the consequences resulting from these particular phenomena, there are other material branches of Dr HUTTON's theory connected with the same views, which in my opinion require to be carefully revised. The theory which he has advanced respecting the formation of valleys, by mere diurnal actions, appears to me liable to great objections, and I cannot help agreeing with M. DE LUC, in much that he has urged against Mr PLAYFAIR on this point. I also concur with my friend Sir GEORGE MACKENZIE, in some general views, suggested to him by the aspect of certain rocks in Iceland, and in rejecting the slow operations of the atmosphere (*Travels in Iceland*, p. 39.)

The difficulty is peculiarly great where a lake occupies the bottom of the valley, and is very conspicuous in the case of the lake of Geneva lately mentioned. For, granting all that has
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been advanced in support of the diurnal formation of the valley in which this lake lies, granting that it has been excavated by the diurnal actions, since these granitic blocks were deposited upon Mount Jura; it still remains to be explained, how the lake of Geneva itself was formed, the depth of which amounts in some places (SAUSSURE, art. 44.) to 950 feet. Now, as has very fairly been stated, this lake is constantly receiving all the spoils of the district which lies above it, called the Val-lais, yet delivers nothing at Geneva, situated below it, but pure water. It is evident, then, that every known diurnal action tends to fill up this lake, and none to excavate it. This is therefore admitted by Mr PLAYFAIR, to be a case "in which hypothetical reasonings are warranted by the strictest rules of philosophical investigation, and where we must therefore have recourse to an agent that is invisible." *Illustrations of the Huttonian Theory*, p. 366.

This acknowledged difficulty seems calculated to entitle the following speculations to a patient hearing from this gentleman; and he has already, in another part of the same work, expressed himself with regard to them, and other kindred opinions, in a manner highly encouraging to discussion, and which affords one of the most striking examples of candour that has ever occurred.

"These arguments," Mr PLAYFAIR says, (referring to those he had just been stating) art. 367. page 412. "appear to me conclusive against the necessity of supposing the action of sudden and irregular causes on the surface of the earth. In this, however, perhaps I am deceived, neither PALLAS nor SAUSSURE, nor DOLOMIEU, nor any author who has espoused the hypothesis of such causes, has explained his notions with any precision; on the contrary, they have all spoken with such reserve and mystery, as seems to betray the weak-

“ness, but may have concealed the strength of their cause. I have, therefore, been combating an enemy that was in some respects unknown, and I may have supposed him dislodged, only because I could not find his strong holds.”

The charge of obscurity here brought forward, is very applicable to the diluvian system of SAUSSURE and DE LUC, in what regards the origin of the torrent, or the disposal of the water after it had overwhelmed the land. The same charge seems not, however, to apply to the suggestion of Professor PALLAS, who, in an early work, entitled, *Observations sur la Formation des Montagnes*, first published in 1777, has very explicitly ascribed the inundation by which he conceives the continent of Asia to have been overwhelmed, to the action of volcanoes rising in the Indian Sea, and forming the Moluccas, the Philippines, and other islands, known or supposed to be volcanic.

It must be admitted, that such an event may occasionally have happened. But the occurrence of similar catastrophes may be inferred in a manner still more general and unequivocal, from those Plutonic * revolutions the reality of which has been established by Dr HUTTON's observations. According to his system, all our strata once lay at the bottom of the sea, and have been raised into their present situation by the subterranean and submarine exertions of heat, similar to that which appears externally in the volcanoes. And the angular fractures exhibited by these beds on many occasions, prove that this elevation was performed in such cases when the mass was in a hard state. It is obvious, then, that the same principle which I lately attempted to apply (page 87. of this volume) to the volcanic phenomena, as exhibited in the *Atrio del Cavallo*, will apply to those Plutonic revolutions; and we are authorized

* I need scarcely say, that by the term *Plutonic*, I mean to characterize that geological system in which the principal agent is heat acting under *Compression*.

sed on these principles to expect, that the liquid substances of basalt and of granite, in their progress through the rents of our strata, have been exposed to congelation, like the lava in the rents of Mount Vesuvius; that their progress has been arrested, and the protruding energy accumulated in a similar manner, till it acquired sufficient power to break through that obstacle, or through some other opposed to it; and it is manifest, however gradual and uniform the propelling force may have been previous to its accumulation, that the ultimate laceration must have been performed, by a sudden and violent motion, producing an earthquake at the surface, and thus affording a more extensive and more satisfactory solution of that tremendous Phenomenon that is furnished by the steam of Mr MITCHELL; though I am ready to admit, that in the volcanic actions, the production and condensation of steam, on many occasions, has produced very powerful effects. In the Plutonic regions, as the restraining mass was beyond all comparison stronger, and thicker and heavier, than in the most violent volcanic action, it must have exerted a power of resistance greater in the same proportion; consequently, the time of that constraint must have been of incomparably longer duration, and the violence of the shock, when the fracture did take place, though no less sudden, must have been incomparably more powerful.

Thus, as Vesuvius, in the course of the middle ages, was once at rest during several centuries, we have reason to presume that the Plutonic action, after being suspended for several thousand years *, should rush forward with a degree of violence proportioned

* This observation meets an objection urged against Dr HUTTON by M. DE BREISLAC, a gentleman to whom I feel indebted for the handsome manner in which he has mentioned the result of my experiments, and who, on most occasions, has treated our views with peculiar fairness, but who seems in this important

portioned to the time of its previous constraint, and capable of fulfilling all the conditions of SAUSSURE's *debacle*, or the wave of PALLAS; and the existence of these tremendous events is thus indicated *à priori* by the Huttonian principles.

I shall now consider, whether such traces have not been actually left by these waves, as to place the reality of their existence beyond all doubt. We have already alluded to some of these, and shall have occasion to mention some others; but in a case of this sort, no historical, nor even traditional authority need be expected; for though no limit could well be assigned to the magnitude which such a wave might actually reach, there is a decided limit to the magnitude of one that could be recorded; since, by exterminating all witnesses, every wave beyond a certain size would infallibly be the cause of its own oblivion. Those of a moderate extent are most likely to have been recorded by man, having sufficient power to produce the most awful impressions, and yet sparing a sufficient number of witnesses to transmit the event to future generations.

In our attempts to elucidate this subject, we shall refer, in the first place, to specimens of this tremendous phenomenon in still lower stages of its power, since they alone have been recorded in an authentic and intelligible form, I mean, by referring to those agitations of the sea which have accompanied most of the great Earthquakes in our own times, as we learn from the following regular statements.

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portant article to have altogether misapprehended the system proposed. In his late work, (*Introduction à la Géologie, tr. Fr.* p. 115.) he urges as subversive of Dr HUTTON's views, some thermometrical observations in which the bottom of the sea was found to be as cold as its surface. To urge this as an objection, implies that the heat employed in this system is supposed to be for ever, and everywhere, acting upon the sand of the sea; whereas it is well known that Dr HUTTON conceived that the subterranean heat, as well as that of the volcanoes, was subject to short fits of activity, with ages of intermission.

In the *Philosophical Transactions*, vol. XLIX. p. 424. we have an account of the earthquake at Cadiz, by Mr B. BEWICKE, merchant there, 1st November 1755, in which it is said, “ an hour after” (the first shock) “ looking out to sea, we saw a
“ wave coming, at eight miles distance, which was at least six-
“ ty feet higher than common. Every body began to tremble ;
“ the centinels left their posts, as well they did ; it came
“ against the west part of the town, which is very rocky ; the
“ rocks abated a great deal of its force ; at last it came upon
“ the walls, and beat in the breast-work, and carried pieces of
“ eight or ten tons weight, forty or fifty yards from the walls,”
&c.

We have another account of the same by Don ANTONIO DE ULLOA, *Phil. Trans.* vol. XLIX. p. 427. who describes the wave as having returned five times after the first *. And similar facts are stated of other scenes of this sort.

One circumstance in addition to those mentioned, which has accompanied all these great events, and which seems at first sight to contradict our explanation of them is, that in all the agitations of the water, the first event has been a retreat of the sea.

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* He says, “ the inhabitants had scarcely begun to recover from their first
“ terror, when they saw themselves plunged into new alarms ; at ten minutes
“ past eleven, they saw, rolling towards the city, a tide of the sea, which passed
“ over the parapet of sixty feet, above the ordinary level of the water ; at thirty
“ minutes past eleven came a second tide, and these two were followed by four
“ others of the same kind, at fifty minutes past eleven ; at twelve o'clock, thirty
“ minutes ; one o'clock, ten minutes ; one o'clock, fifty minutes. The tides
“ continued, with some intervals, till the evening, but lessening. They have
“ ruined one hundred toises of the rampart, part of which, of three toises length,
“ and of their whole thickness, were carried by the torrent fifty paces.” It is
remarkable, that in these six waves, the two first intervals of time are exactly of
twenty minutes each, and the three others of forty. It is probable, that they
have all resulted from one great impulse.

Mr MITCHELL says, *Philosophical Transactions*, vol. LI. p. 566. 611. speaking of the earthquake at Lisbon in 1755, "the bar (at the mouth of the Tagus) was seen dry from shore to shore, then suddenly the sea, like a mountain, came rolling in. This phenomenon accompanied the same earthquake at the island of Madeira, where we are told, that at the city of Funchal, the sea, which was quite calm, was observed to retire suddenly some paces, then rising with a great swell, without the least noise, and as suddenly advancing, entered the city*." He further states, in the same page, that "in the northern part of the island the inundation was more violent, the sea retiring there above one hundred paces at first, and suddenly returning, overflowed the shore, forcing open doors," &c. Again, in page 466. he says, "The great earthquake that destroyed Lima and Callao in 1746, seems also to have come from the sea; for several of the ports on the coast were overwhelmed by a great wave, which did not arrive till four or five minutes after the earthquake began, and which was preceded by a retreat of the waters, as well as that at Lisbon."

Looking into the original account of these calamities in DON ANTONIO DE ULLOA's *Travels into South America*, French edition, vol. i. p. 467. I find that he mentions two events of this sort, which took place at Callao; one in 1687, in which the sea first retreated, and then returned with such force as to overwhelm Callao and other places. And again in 1746, when, in the course of twenty-four hours, two hundred shocks were felt: on this occasion, the sea retiring as it had done formerly, on similar occasions, returned furiously, and overwhelmed Cal-
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* *Philosophical Transactions*, vol. XLIX. p. 433. The retreat is here said to have begun an hour and a half after the shock.

lao so completely, that nothing remained of the whole town but a remnant of Fort Santa Cruz. Out of twenty-three vessels at anchor in the harbour, nineteen were sunk, and the four others, one of which was a frigate, were carried to a great distance on shore, and left on dry land. At Callao, out of four thousand inhabitants, only two hundred were saved.

Mr MITCHELL endeavours to account for this, by his favourite doctrine of steam, according to which he conceives, that the strata at the bottom of the sea have been raised as a roof; he then continues, (p. 613.) “ now while the roof is raising, the
“ waters of the ocean, over it, retreat, and flow from thence
“ every way; this, however, being brought about slowly, they
“ will have time to retreat so gently, as to occasion no great
“ disturbance; but as soon as some part of the roof falls in, the
“ cold water contained in its fissures mixing with the steam,
“ will immediately produce a vacuum, in the same manner as
“ the water injected into the cylinder of a steam-engine, and
“ the earth subsiding, and leaving a hollow place above, the
“ waters will flow every way towards it, and cause a retreat of
“ the sea on all the shores round about.” But the gradual elevation of the roof, keeping tight all the while, which is here assumed as the steam was collecting, can by no means be admitted; since, as we have shewn, a mass such as this, when forced upwards, would have yielded suddenly.

This phenomenon, however, along with all the rest mentioned, may, I conceive, be traced as consequences of the simple, but rapid, elevation of the bottom, which we have ascribed to the forcible introduction from below of stone in liquid fusion. Water being almost incompressible, or elastic in an extreme degree, it is obvious, that an elevation of that portion of the fluid which lay immediately upon the part of the bottom raised, would take place almost simultaneously. This

elevation would of course be the greatest in the middle, the quantity of matter in the vertical line being the least ; but there is no doubt that an action of the same kind, diminishing in a high ratio, as the distance from the centre increased, would be extended all round ; the surface of the fluid being thrown into the form of some species of conchoid, produced by the revolution round the vertical axis cb of the line dce , (Plate VI. fig. 1.) the highest point of which is at c , with a contrary flexure at f and h , and going off beyond these points, in such a manner as to have the original level surface of the water dbc for an asymptote. This conchoidal elevation, produced in the first moment, being just equal in bulk to the quantity of water displaced by the solid mass (aa) elevated at the bottom.

If this mass stopped suddenly, as would naturally happen, when, in consequence of the rupture, the protruding liquid stone has found a vent ; it is evident, that every part of the water put in motion during the first moment, would tend, according to the first law of motion, still to continue moving during the next and following moments ; but this tendency could not be everywhere effectual ; because, if all the water that rose in the first moment were to rise also in the second, a vacuum would be the consequence. In the second moment, then, a struggle must take place between the different portions of the fluid ; the middle portion, which had acquired the greatest velocity, would continue to flow upwards, and its place not being supplied from below, as, during the first moment, a suction would be exerted downwards, upon all the surrounding fluid. The consequence must be, that the rise which each particle would have performed by its separate momentum, during this second moment, would be counteracted by the pressure of the atmosphere, and entirely done away at a certain distance from the centre, as at f and h , fig. 2. ; beyond that limit the water would
sink

sink below the medium level, as at *k* and *i*. An annular depression would thus be produced, surrounding the central elevation, vertical sections of which are exhibited by *dkf* and *hie*; that depression below the medium level being exactly equal in bulk to the additional elevation at the centre, occasioned by the momentum, the vertical section of which is *fghc*. The middle elevation, and the annular depression, would continue to increase during a period, and to an amount regulated by that class of laws, which determines the magnitude and duration of waves, as relative to the wind which produces them; and the water at the end of that period would be thrown into the position denoted by the dotted line in fig. 2., and by the single continued line in fig. 3., indicated in both figures by the letters *dkfghie*.

During a third period, the water flowing from without the annular depression, to fill it up, a wave of depression would be propagated outwards in all directions, and would visibly reach to places where the elevation produced by the direct impulse in the first moment, had not been at all perceptible, in consequence of their distance from the centre. It is owing to this cause, I am convinced, that the catastrophes at Lisbon, at Cadiz, at Madeira, and at Lima, in so far as the position of the surface of the sea was concerned, were all first announced by a retreat of the sea, as shewn at *m*, in fig. 4.

During another successive period, the undulatory motion continuing, the wave of elevation would advance towards the shore at *m*, and being met by the retiring water, would produce the curling form called the breaking of a wave, as shewn at *m* in figure 5. At that instant the calamitous effects described in all the great earthquakes, as resulting from the sea, have been produced.

I have made a few experiments lately with explosions of some pounds of gunpowder under water, in order to try the effect of sudden impulse. In every case, a very manifest heave of the surface was produced at the instant of the explosion ; and at that same instant, a very smart percussion was felt. This was always followed in two or three seconds by a distinct and separate agitation of the water, occasioned by the rising of the permanently elastic gases produced in the explosion. The form given to the wave in fig. 1. has been sketched from the recollection of that first momentary impulse.

We have thus a theory of those waves which seem almost invariably to have accompanied the great earthquakes ; supposing these to have been occasioned by submarine elevations, and to form an intermediate step between the Volcanic eruptions, and the truly Plutonic operations by which our continents have been elevated.

But, to return to the alpine scenes with which we set out : If the phenomena on the banks of the lake of Geneva, to which we have alluded, were really occasioned by a torrent of water, its magnitude must have been such as to leave few vestiges of the human race, in Europe at least, to record the circumstance ; and we can only expect to find proofs of its reality in geological facts. The events of Lisbon and of Callao, though on a scale comparatively diminutive, help to lead our imagination to the conception of this colossal disaster. It may still, however, be alleged, as I have already hinted, that it would be impossible for water of any depth whatever, or moving with any velocity, to carry blocks of such magnitude to such situations ; and the consideration is of so great importance, that I am induced, in attempting to unite the ideas of SAUSSURE with those of HURTON, to retain part of the system
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proposed by M. WREDE, in so far as to consider the granitic blocks as having been made to float, by means of a mass of ice attached to each.

A circumstance happened lately in this country which tends to encourage that idea. Upon the coast of the Murray Frith, a large block of stone, four or five feet in diameter, lying within high-water mark, and well known as having served to denote the boundary between two estates, was, during a stormy night in winter, transported to the distance of ninety yards, and the persons upon the spot were convinced, that this migration was performed by means of a large cake of ice, formed round the stone, and attached to it; and that the whole had been lifted and carried forward by the rising tide. The course of this stone was marked upon the sand below, by a deep and broad furrow, which remained visible for a long time afterwards, as I have been informed by several members of this Society, who saw it after an interval of more than a year.

By supposing that an agent of this sort acted in concert with our diluvian wave, the difficulty with regard to the transportation of the granitic blocks, seems to be in a great measure removed; and this explanation happens to be peculiarly applicable to the case in point; the native place of these blocks being covered profusely with ice, in the state best calculated for fulfilling the office here assigned to it. M. DE SAUSSURE has given the following clear account of the formation of the Glaciers. In any region whose temperature is such that the heat of summer is not sufficiently powerful to melt all the snow of winter, the remaining snow, which, by partial thawing, and subsequent freezing, is turned into ice, must accumulate year after year to an unbounded degree, so that the mass must become at last so lofty, as to be unable to bear its own weight, and must produce a motion downwards, which will be greatly assisted

assisted by fractures, and by the sliding of the ice upon the ground. In this manner the glaciers are produced, which consist of an assemblage of great blocks of ice, each as big as a house, which, sliding and rolling upon each other during summer, make their way from the high frozen valleys where they were formed, through narrow glens, into the rich and warm vales below, in which they remain a considerable time, presenting a curious spectacle among the meadows and woods, some of which they overtop. These masses, however, melt at last, in this new situation, and leave vast assemblages of stones, which had been attached to them, forming a ridge called the *Marene* of the Glacier.

It is obvious, then, that a wave washing over these high alpine valleys in summer, would float and carry off all the ice in the glaciers, and accumulated in the higher valleys, and, along with the ice, all the blocks of stone imbedded in it, or attached to it in any way. The stream, with this load, would find its way through every opening, and would in a particular manner flow through those depressions, which at this day, as we have said, afford a view of the snowy summit of Mont Blanc, from certain places on the face of Jura where these blocks abound.

The enormous masses already mentioned, which are found near Geneva, and at the Coteau de Boisy, may now be accounted for ; and the same system will apply also to the blocks upon the Baltic, which may have been brought to their present place, not by a permanent and steady position of the ocean, varying by slow degrees, as has been alleged by M. WREDE, but by a sudden diluvian wave washing over some district, situated either at a sufficiently high level, or near enough to the pole to be the seat of glaciers. I am not at present acquainted with any facts by which the native place of these
blocks

blocks can be traced ; but I trust we shall not long remain in that state of uncertainty, since there are means by which that point will be found of very easy decision, as I shall endeavour to shew in a subsequent part of this paper, by examples in the neighbourhood of Edinburgh.

It is well known, that granite is found native in Sweden ; so these blocks may have been carried across the Baltic, as those of Mont Blanc have crossed the valley of Geneva. It is possible also, that they may have been transported by the help of ice from the Alps across Germany, by the very same torrent we have been considering, and which had left a portion of its load behind it on Mount Jura.

This view would afford a natural account of the production of Holland, and of a great part of that quarter of Europe, which consists entirely of sand, and whose magnitude appears to me very far to surpass any deposition that could reasonably be ascribed to the present rivers. All this sand may be conceived to have been hurried along by that mighty stream, and deposited when the torrent began to spread, and lose its force by diffusion. It is likely, too, that an immense quantity of this sand would be carried far into the ocean, and its deposition being there modified in various ways, by local tides and currents, might assume the character of horizontal strata, so as to lay the foundation for future productions of freestone or of killas. We might thus, by the help of this diluvian agent, complete the great circle of events, so elegantly pointed out by Dr HUTTON, but which the diurnal agents seem quite insufficient to fulfil.

As the sand was depositing upon the low countries, the blocks of granite, with their accompanying ice, from whatever quarter they originated, would still keep floating, and thus account for a striking fact stated by M. DE LUC. He observes, that

that the granitic blocks lying in the district between Berlin and the Baltic Sea, occur frequently, and almost constantly, in very numerous assemblages, upon the summits of the sandy hills with which that country is interspersed, whilst none are to be met with in the intervening valleys. That they also abound on the islands of the Baltic; and these blocks shew themselves upon the beach only in those places where the sea reaches the base of some of the hills on which they lie.

The present theory affords an easy solution of this fact. In the descending stream, these hills, constituting shallow sand-banks, would afford the first resting place for the floating blocks of ice, which, grounding upon them, would accumulate to very numerous assemblages, and there deposit their granitic charge, while all the other similar blocks flowed onwards, in the deep water between *.

We are thus enabled to give a tolerable account of the granite blocks; but the formation of valleys, and the excavation of lakes, in particular of the lake of Geneva, remain to be explained.

* This aid, by which our diluvian wave is so much assisted, can be of no such service to M. DE SAUSSURE's hypothesis, as he has originally framed it. He conceives (art. 210, p. 141. vol. i.) that, at a period when the mountains were covered with water, the crust was broken which defended certain caverns then void, and the waters rushing into them with violence, left the mountains in their present state, and, by their retreat, produced the diluvian torrent. It is plain that such a torrent could derive no assistance from the blocks of ice. For when this water, in its previously stagnated state, touched the solid earth, if any blocks of ice were there, they would adhere to that solid mass, and would not be carried off by the retiring waters; and where this water lay deep upon the solid mass, its ice, if it had any, must have been floating at the surface, and could not be attached to blocks of stone, lying of course at the bottom.

It is obvious, from the mode in which this topic is introduced by M. DE SAUSSURE, (which he does, as he expressly says, to meet the impatience of his reader,) that he is not satisfied with it himself, nor can it bear the slightest examination.

plained. I trust, however, that the same principles will extricate us from this difficulty also.

If my view is correct, as to the violent manner in which our continents have risen from the bottom of the sea, to their utmost elevation in the atmosphere, it is quite obvious, that the cold and hard external crust, while it communicated such shocks to the ocean, must itself have undergone the greatest agitation, and must have been rent and broken in every conceivable mode. The stone, in liquid fusion, introduced into the rents, would assist the elevation, in so far as it tended to facilitate the shifts, by enabling one mass to slide on the other ; but forcing its way upwards, this liquid would at last reach a temperature, in which, as we have said, it would congeal ; its further progress in that direction, with respect to the neighbouring substance, being thus effectually stopt, and the propelling force from below continuing to act, the local elevation would be converted into a more general one, either where the strata were in a flexible state, by means of an horizontal thrust, producing an elevation along with the convolution of the strata, (in a manner lately pointed out in this Society), or, where the neighbouring substances were inflexible, by a penetration of the liquid through the mass, giving rise also to a vertical heave of the whole. In either case, a number of rents would be formed, in the hard outer crust, which would widen upwards as the heave advanced *, thus forming the rudiments of valleys

VOL. VII.

X

of

* This elevation may be illustrated, by the familiar example of what happens in the act of digging a piece of firm soil. The gardener first thrusts his spade into the unbroken ground, then leaning on the handle, gradually forces the flat iron upwards ; by this means, a heave of the soil takes place ; the middle part being raised, while the sides remain more or less attached to the firm soil. A separation between the two is the consequence ; and a formation of rents open upwards, which gradually become wider and wider as the spade rises.

of every degree of wideness, from the narrowest ravine to the most open vale.

All this progress, or at least great part of it, being performed by successive starts, each of which, while under water, producing a tremendous wave; the mass, in rising, would be repeatedly exposed to these diluvian torrents, some formed by itself, and some by neighbouring elevations, so as at last, after long exposure to agents, partly diluvian and partly diurnal, it would arrive at its present situation and condition.

In consequence of the diversity of these elevations in place and in magnitude, acting upon substances of every sort, an endless diversity of effect would be the result. In some cases a rent, in consequence of the locality of the heave, would be rendered both large and deep in the middle, while it remained nearly closed at both ends. Water, therefore, could not flow through it without stagnating by the way, and thus a lake would be formed, the depth of which might be very great in the middle, though its extremities were shallow. This applies to the formation of the lake of Geneva, either before or since the passage of the granitic blocks. It is applicable also to all lakes which occur in alpine or rocky districts. It will appear in what follows, that an account no less satisfactory may be given on other principles of those which belong to alluvial countries.

An example of the relative changes among rocks, produced by motions of this sort, occurs in the mass on the coast of Berwickshire, with which we have been so much occupied in a former meeting, as affording a display of the convolutions of the strata. Upon this coast, the killas and sandstone meet on the East near Eyemouth, and on the West in the parish of Cockburnspath, at the Siccar Point, where the junction is beautifully

ly displayed. In the neighbourhood of both junctions, the sandstone strata, laid open by the shore, present to view a striking picture of former revolutions, as I have endeavoured to represent by two sketches taken upon the spot. One is at the Hallelahole near the Press, between Berwick and Eyemouth (Plate VII.) on the east; and the other at the Coveshore (Plate VIII) in the parish of Cockburnspath on the west.

In both, the sandstone, remote from the junction, is seen in an horizontal position, which seems to be its general character, and which upon the West it maintains to a great extent; but in the neighbourhood of the junction, the beds become absolutely perpendicular, in a rock which, in both cases, rises to the height of thirty or forty feet. This change of position takes place differently in the two cases. That upon the East is sudden and immediate, the whole change being performed in a single cliff, at the bottom of which the beds are horizontal, and at its summit vertical, nay, leaning over upon themselves; on the other it is gradual, the strata being seen in succession to pass through every intermediate declivity, the change occupying more than a mile of coast.

This local elevation seems unequivocally to denote a great perpendicular shift. Either the sandstone mass has sunk and left the killas behind it, or the killas has risen from below, leaving the sandstone, and dragging its contiguous beds partly up along with it. As we know that the whole once lay deep below the sea, and that it is now raised, it seems more simple to ascribe these changes to one action, and to believe that the last supposition is the true one.

A fact which I witnessed in Calabria, not long after it occurred, belongs to the same class, and may bear a similar interpretation. What is called the Plain of Calabria is an alluvial

district, situated along the western coast, behind which there rises a ridge of primitive mountains. When the terrible earthquake of 1783 took place, a very extraordinary circumstance happened, the remains of which I went to see in 1785, and found it corresponding to DOLOMIEU's description. A naked scar of red earth, facing the plain, made its appearance upon the primitive mass, all along the line of junction between the two; this scar being ten or fifteen feet high, and appearing almost universally in a tract of many miles. DOLOMIEU has given an ingenious theory of this phenomenon, which appeared to me satisfactory at the time. He supposes, that in consequence of the agitation of the alluvial mass by the earthquake, it had undergone a subsidence, or *tassement* (as it is expressed in French), similar to what happens when a bushel of wheat is struck by the hand; and that in consequence of the subsidence of the loose country, the firm country had been left behind. I am now, however, inclined to suggest a different explanation of the same fact, and to connect it, by analogy, with some of those lately mentioned, by conceiving that the middle, or rocky mass, may have been raised, leaving the alluvial mass behind it, and that its motion has been the proximate cause of the earthquake. It may further be observed, in favour of this view, that if the alluvial mass did subside in the manner pointed out by DOLOMIEU, the districts in the neighbourhood of the sea must have been submerged. It would have been easy, if such a conjecture had occurred on the spot, to bring it to the test of observation; for the opposite side of the coast of the peninsula must have furnished some facts either in confirmation or refutation of the idea. I mention it here, as a hint to future travellers, in tracing the result of earthquakes.

I cannot too strongly recommend to geological travellers, the examination of those scenes where earthquakes, and their attendant

pendant waves, have exerted their influence, and which have probably left behind them very striking monuments of their power. I conceive that in this way, the doctrines already advanced in this paper, and which I am now about more fully to illustrate, will be submitted to the test of direct observation.

The fact which I have met with most strictly in point, occurs in HUMBOLDT'S Account of Mexico, *Eng. Trans.* London, 1811, vol. ii. p. 212. At San Pedro de Jerullo, "in the month of June 1759, a subterraneous noise was heard. Hollow noises of a most alarming nature (*bramidos*) were accompanied by frequent earthquakes, which succeeded one another from fifty to sixty days, to the great consternation of the inhabitants of the Hacienda. From the beginning of September, every thing seemed to announce the complete re-establishment of tranquillity, when, in the night between the 28th and 29th, the horrible subterraneous noise recommenced. The affrighted inhabitants fled to the mountains of Aquasarco. A tract of ground, from three to four square miles in extent, which goes by the name of the Malpays, rose up in the shape of a bladder. The bounds of this convulsion are still distinguishable in the fractured strata. The Malpays near its edges is only twelve metres (thirty-nine feet) above the old level of the plain, called Playas de Jerullo; but the convexity of the ground thus thrown up, increases progressively towards the centre, to an elevation of one hundred and sixty metres (five hundred and twenty-four feet)."

In this most striking and interesting scene, we have an actual specimen of those violent and sudden operations by which our continents have been raised to their present position, according

according to my view of Dr HUTTON's Theory ; and had this event come to his knowledge, which happened in his day, it might have induced him to admit the probability of those sudden elevations, indicated by so many facts.

The process of elevation, whether performed gently or rapidly, is free from a difficulty to which the systems of both SAUSSURE and WERNER are exposed : both of these geologists conceive that our continents and rocky districts were once covered with water, which has since flowed away, these rocks maintaining their original position ; now, to lay a rock bare in this manner, we must dispose not only of the water which covered the immediate mass of rock, but also of that body of the same fluid which occupied an equal level all over the globe. This difficulty was strongly felt by Professor PALLAS, who says, (*Observations sur la Formation des Montagnes*, p. 79.), that were the summits of the mountains supposed to have been covered, the mass of water required to equal and surmount them round all the globe could not be disposed of within the earth, even were its inside made up of caverns : On that account, he denies that the summit of the hills has been covered. He burdens himself, however, with a very considerable share of the same difficulty, by supposing that the sea had stood at such a level as to submerge hills of 100 fathoms high.

According to our theory, there is no such embarrassment. We suppose these low hills, as well as the high ones, to have been raised from the bottom of the sea, which need not be considered as ever having stood above its present level. And I think myself authorised by the facts stated in the course of this paper, in deviating so far from the Huttonian hypothesis, as to believe that the elevation of the land was performed

formed by successive starts, similar to volcanic eruptions, though far more rare and more powerful; and that the percussions impressed by these starts upon the waters of the ocean, were such as to form waves, sometimes of a moderate force, as those at Lisbon or Callao; sometimes of overwhelming magnitude, and capable of producing the effects described in the Alps, in Germany, and in Russia.

As the inferences derived from these distant facts are called in question by some gentlemen of the highest authority in this Society, I am happy to have it in my power to produce a set of observations made in this immediate neighbourhood, which seem in a manner no less satisfactory, to lead to similar conclusions. These are the subject of the second part of this communication.

PART II.

Fig 1.

Fig 2.

Fig 3.

Fig 4.

Fig 5.

PLATE VII.

HALTACHOLIE.

Engr'd by R. A. D. Isaac's Phil.

COVE SHORE.