SIR CHARLES LYELL

AND

THE GLACIAL THEORY

OF

LAKE-BASINS.

BY

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EXTRACTED FROM THE PHILOSOPHICAL MAGAZINE FOR APRIL 1865.

LONDON:

PRINTED BY TAYLOR AND FRANCIS,

RED LION COURT, FLEET STREET.

1865.

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GLACIAL THEORY OF LAKE-BASINS.

IN Sir Charles Lyell's new edition of the 'Elements of Geology,' he has devoted several pages to the discussion of the theory of the "connexion of the predominance of lakes with glacial action," and he does me the honour, by a number of counter arguments, to combat the views I advanced in 1862*. In the opening passage he adopts that part of my reasoning in which I first insisted, as a significant fact, on the connexion of multitudes of lakes with regions high and low in all latitudes that have been thoroughly subjected to ice-action, and their comparative rarity in countries where the signs of glacial action have not been observed†. It is with satisfaction that I now see

* "On the Glacial Origin of certain Lakes," &c., Journal of the Geological Society, vol. xviii. p. 185 (Proc. March 5, 1862).

† On this subject Sir Charles observes, "It has been truly remarked that lakes are very common in those countries where erratics, striated boulders, and rock-surfaces, with other signs of glaciation, abound, and that they are comparatively rare in tropical and subtropical regions. When travelling over some of the lower lands in Sweden, far from mountains, as well as over the coast-region of Maine in the United States, and other districts in North America, I was much struck with the innumerable ponds and small lakes, of which counterparts are described as equally characteristic of Finland, Canada, and the Hudson's Bay territories." These are the very regions to which I directed attention in my Memoir of 1862; and my attention having been directed by a geologist of distinction to the passage quoted, I was led to infer that by accident I had done injustice to the published views of Sir Charles Lyell. It was therefore with a sense of relief that, on referring to his two journeys in North America and other writings, I failed to find any allusion to the subject. I mention it now lest others should draw the same inference that I did. As far as I am aware, I first drew special attention to the fact in connexion with the Glacial Theory.

this fact stated as a piece of common knowledge in a manual so popular as Sir Charles Lyell's is sure to be. Some physical geologists may doubtless marvel that Sir Charles, writing of lakes that "run in great rents and faults," is still of opinion that the existence of such rents and faults in connexion with valleys "is no more than may be said of most of the longitudinal and transverse valleys in every mountain-chain;" but I will not argue that point, and I may forget the assumption when I find it coupled with the admission of the truth of the principle I endeavoured to establish, that mountain-lakes do not lie in gaping fractures, and that fractures, wherever we know them by eye, are almost always close*. Sir Charles, I am glad to see, also approves of my argument to show that the Alpine and other lake-basins are not the result of special subsidences; and the admission of all these points by him will help no doubt by-and-by to procure the adhesion of readers who do not think or have no opportunity of observing for themselves. Those who go so far take so many steps in the right direction—steps, I think, which may in the long run lead them to accept my theory altogether.

But though there is this partial agreement in some details, including the direct power of ice "in scooping out shallow basins where the rocks are of unequal hardness" (Antiquity of Man), Sir Charles does his utmost to disprove the possibility of glaciers on a great scale having been the means of scooping out by slow erosion large lake-basins, such as those of the Alps, Scotland, Sweden, or North America; and I now propose, as briefly as I can, to examine some of the arguments to which he seems to attach the greatest weight.

The erosion, then, of large rock-basins is untenable because even if ice, in descending a steep slope, "scoop out one of those cavities called tarns," yet we must suppose that it loses all power of extending the cavity, being unable to cut *a gorge* through the lower margin of the tarn; and "this diminished force of erosion, wherever the ice has to ascend a slope, *or to move horizontally,* seems adverse to the hypothesis of the formation of lakes of considerable length and depth by glaciers." In my last paper, published in the Philosophical Magazine for October 1864, some months before the appearance of the 'Elements,' I discussed for the second time what I believe to have been the peculiar scooping effects of huge glaciers that issued from the slopes of great, yet comparatively narrow valleys into the wider plains that they overspread, or into flats near the

* Attributed by Sir Charles to Mr. Jukes, who in an admirable article in the 'Reader,' March 12th, 1864, used in my favour, and with new illustrations, the arguments which I employed in my original memoir.

mouths of the valleys themselves, and still within their bounds And though Sir Charles has not met the arguments urged either in my first or second paper, except by implication, I am constrained by the circumstances of the case to repeat them in a manner that I hope cannot be misunderstood. This may be done in a very few words. Every physicist knows that when such a body as glacier-ice descends a slope, the direct vertical pressure of the ice will be proportional to its thickness and weight and the angle of the slope over which it flows. If the angle be 5°, the weight and erosive power of a given thickness of ice will be so much, if 10° so much less, if 20° less still, till at length, if we may imagine the fall to be over a vertical wall of rock, the pressure against the wall (except accidentally) will be *nil*. But when the same vast body of ice has reached the plain, then motion and erosion would cease, were it not for pressure from behind (excepting what little motion forward and sidewards might be due to its own weight). This pressure, however, must have been constant as long as supplies of snow fell on the mountains, and therefore the inert mass in the plain was constantly urged onwards; and because of its vertical pressure its direct erosive power would necessarily be proportional to its thickness, and greater than when it lay on a slope; for it would grate across the rocks, as it were, unwillingly and by compulsion, instead of finding its way onwards more or less by virtue of gravity. Indeed the idea is forced on the mind, that the sluggish ice would have a tendency to heap itself up just outside the mouth of the valley and there attain an unusual thickness, thus exercising, after its descent, an extra erosive power. Further, as I have said elsewhere, when the glacier spread well out upon the plain, far beyond the mouth of the valley, it would of necessity thin more and more by melting; and this seems to me a very obvious reason why, its weight being lessened, the waste of underlying matter by erosion would decrease towards what are now the mouths of those lake-basins which Sir Charles, following the supposition of the late Dr. Falconer, allows were filled with ice during the glacial period. These propositions seem to me so obvious, that I should scarcely have thought it necessary to restate them; but if they be mere fallacies, it is singular that no one has yet thought it worth while to refute them. Sir Charles himself seems to allow that the ice may have had "to ascend a slope"*.

The remark that in a "part of a valley from which a glacier has retreated in historical times, no basin-shaped hollows are conspicuous," is met, if we think of it, by the foregoing

* To discuss the details of this subject would involve a repetition of what I have already printed, and this I must necessarily avoid. Geological Journal, *l. c.*; Phil. Mag. October 1864, &c.

observations; for the extreme end of the petty glaciers of to-day have only a small erosive power, and not one that I know in the Alps has ever in historical times been protruded in mass *on and well over a plain.* When a glacier lies on a slope, it needs little reflection to show that its tendency will by no means be to produce "cup-and-saucer-shaped cavities"*.

The argument that the constant occurrence of transverse rents in the same part of a glacier proves that the ice "cannot saw through and get rid of the obstacles which impede the freedom of its onward march," reads strangely after the admission that "everywhere we behold proofs that the glacier, by the aid of sand and pebbles, can grind down, polish, and plane the bottom." Neither the mountains that bound nor the valleys under the glaciers can possibly preserve the same relative details of feature till all shall be worn away; and the position of the obstacles as constants can no more be considered indestructible than the Falls of Niagara, which Sir Charles Lyell has long ago shown may retreat till Lake Erie itself shall be drained.

Another point seems to require explanation. At the beginning of the subject (p. 168) it is stated that lakes are *exceedingly common in all regions* that have been glaciated, and rare in tropical and subtropical regions; and at page 170 it is observed that "such basins, large and small, are met with *in all latitudes.*" Now I have specially guarded myself against being supposed to assert that all lake-basins have been formed by ice; but if "such basins, large and small, are met with in all latitudes," which I doubt (except among mountains which maintain or have maintained glaciers), their relative proportions in different latitudes deprives the argument of much or of any value; and for other reasons the same may be said of the remark "that there are lacustrine deposits of *all geological epochs*, attesting the existence of lakes at times when no one is disposed to attribute them to the agency of ice." There may have been lakes of all geological epochs; but I should like to see proofs adduced; and very few of them are mentioned in the 'Elements.' Where are the lakes of the Silurian strata, which themselves embrace more epochs than one, if unconformities constitute epochs? And though Sir Charles compares the Old Red Sandstone fish to

* See my paper "On the Erosion of Valleys and Lakes," Phil. Mag. October 1864. I have often thought that the absence or scarcity of lakes on the southern flanks of the Himalayah is due to the well-known steepness of the valleys, and their occurrence in numbers on the north is owing to the opposite circumstance. Tarns I know there are on fragmentary flats on the mountains on the south side. But I can only judge from maps and descriptions, and therefore dare not positively assert it. After the publication of my first memoir, Dr. Hooker wrote me a letter confirmatory of these views, which were till then new to him.

living genera in African and American rivers, is he prepared to follow Mr. Godwin-Austen's opinion, that much of the Old Red Sandstone, as distinct from Devonian rocks, is a lacustrine deposit? With respect to the Coal-measure strata, constant reference is made in the 'Elements' to their formation in deltas or lagoons; but no mention is made of great deep inland lakes. Indeed the word *lake* is only once used in the description of this formation, and it is immediately qualified by the word *lagoon*. Has any one yet described Permian lakes? though I believe they will be found. And even in his account of rock-salt, Sir Charles does not assert that the salt of the Trias was formed in far inland continental lakes supersaturated with salt, though he refers to those of Asia; and he again insists rather on lagoons, as in the Runn of Cutch or the Bahr Assal, near the Abyssinian frontier, or the possibility of salt now forming in the Red Sea. Are there any Liassic, Oolitic, or Cretaceous lakes described? On the contrary, all their freshwater formations are either said to be deltoid, or the manner of their formation is left in the dark. It is true that lakes have been described of very late Eocene (?) and of Miocene age; and there the record of them begins and ends till we come to post-pliocene and recent times. It is therefore by no means yet a piece of common knowledge "that there are lacustrine deposits of all geological epochs."

But if "lacustrine deposits" are "of all geological epochs," has no one spoken of "the agency of ice" in past times? or has no one written of anything that might suggest that idea to an unbiased mind? Let us look to this. Mr. John Carrick Moore has described conglomerates in the Lower Silurian rocks of Wigtonshire, which might well be called boulder-beds; for a prodigious number of the enclosed masses of gneiss and granite (which Mr. Moore has pointed out to me on the ground) range from a foot up even to six feet in diameter, and all of them have been derived from ancient strata (perhaps Laurentian) of a region now unknown.

The conglomerate of the Old Red Sandstone of several parts of Scotland and the North of England is so like the "Upper boulder-drift" of many parts of Britain, though consolidated, that other geologists besides myself have spent hours in searching it for scratched stones; but, for chemical reasons connected with pressure, which Mr. Sorby will appreciate, none have yet been found, if they ever existed there*.

Mr. Godwin-Austen has been so bold as to attribute the transport of blocks in the French carboniferous rocks to floating ice; and I invite any one to examine the ice-scratched erratics

* The Rev. J. G. Cumming long ago suggested the glacial origin of the Old Red conglomerates of the Isle of Man.

of the Permian strata in the Jermyn Street Museum, and then to judge if the subject as described by me does not, to say the least, deserve the measure of attention which it has received in the Manuals of Professor Phillips*, Professor Jukes, and Mr. Page.

I feel convinced that the same conclusions which I drew for the Rothliegende of part of England will yet be extended to much of that of Northern Germany; for though marls and gravels are interstratified with it, these, as in our post-pliocene drift, are exceptional, and the main characteristic of this vast formation (2600 feet thick) in the Thüringerwald is the flattened and subangular nature of its blocks, some of which are of large size. Similar erratic deposits are now forming in Baffin's Bay and the Western Atlantic.

Mr. Godwin-Austen long ago suggested the ice-borne character of great blocks in the New Red Sandstone of Devonshire; and the Oolitic strata of the east of Scotland contain such numbers of huge angular blocks, that their possibly though scarcely probably glacial origin constantly suggested itself to my mind when I noted the facts during a journey with Sir Roderick Murchison in 1859. The local character of the blocks, chiefly but not entirely Oolitic, is adverse to the view; but the *smashed* condition of many of the shells in the interstratified oolitic clays is analogous to the state of the shells in the upper drifts all over Britain.

It may not be generally known that Escher von der Linth is aware of boulders in the cretaceous strata of the Alps, and Godwin-Austen has suggested a similar origin for boulders sometimes found in the British chalk; and surely, though unnoticed by him in the 'Elements,' Sir Charles is conversant with the clear-sighted observations of Gastaldi, who attributes the formation of certain conglomerates (with scratched stones), and the transport of the huge boulders that lie in them, to the agency of floating icebergs that, descending into a miocene sea, broke from Alpine glaciers, and carried their freights to the neighbourhood of what is now Turin, from the far-off region where the Lago Maggiore at present lies.

Geologists, then, some of them of the highest eminence, have actually written of "the agency of ice" in several geological epochs; and, whether in these epochs or in others mentioned above, it is clear that erratic- and boulder-phenomena not easily to be accounted for exist in many formations, these phenomena being not unlike those that are brought about by floating ice in the present day. The subject of the ancient agency of glaciers and floating ice is indeed far too prominent to be disposed

* Professor Phillips does not agree with me, but still in a note he takes care to notice my opinion.

of without examination, and rejected, for all but post-pliocene time, in half a dozen lines, as if indeed, even in a Manual of stratigraphical geology, the older strata exhibited no perplexing phenomena that might induce anything to be said on the subject worth attention. In the case of the Miocene ice-work of the Alps, which, having seen it with Gastaldi, I have long considered to have been *proved* by him, some persons may consider it suggestive *that lacustrine phenomena do occur* on the flanks of the Alps in the same formation; and if any of the boulder-conglomerates of the Old Red Sandstone be ice-formed, and Mr. Godwin-Austen's suggestion be true, the conjunction occurs again. "It would, indeed, be the most perplexing of all enigmas," says Sir Charles, "if we did not find that lake-basins were now, *and had been at all times,* a normal feature in the physiognomy of the earth's surface, since we know that unequal movements of upheaval and subsidence are now in progress, and were going on at all former geological epochs." Here again we find the assumption of "lake-basins at all times," just as if it were a fact familiar to geologists that such lake-basins had always existed, whereas, eliminating lagoons, the statement seems to be only derived from two or three circumstances relating to strata of tertiary times. Traces of estuarine beds are more frequent; but this is another matter altogether. To me the absence of lake-deposits is not at all perplexing,—first, because the preservation of all superficial terrestrial phenomena (as opposed to marine) has been, for obvious reasons, rare in the world's history, except in strata of very late date; and secondly, because I believe that the conditions for the formation of innumerable lakes like those of North America, Scandinavia, the Highlands, the Alps, and other glacier mountain-regions, were probably comparatively rare in the earlier history of the earth. That accidental lakes, due to volcanos, and a few of them perhaps to unequal movements of upheaval and subsidence, may have existed at all times is perhaps certain; and it would do no harm to my theory were I to concede that all the known and accepted lakes of Miocene and Eocene (?) times, and older ones if they existed, were formed by the processes to which Sir Charles adheres.

These preliminary points regarding past times being stated lead, in the 'Elements,' to the special discussion of Sir Charles Lyell's proposition as to what was in his opinion the real cause of the formation of the larger lakes that flank the Alps; for, except in a vague manner, he does not grapple with the origin of the unnumbered lake-basins that are strewn over the face of such a country as North America. "We need but little reflection," he remarks, "to discover that when changes of level are in progress, some of the principal valleys can hardly fail to be

converted in some parts of their course into lakes of considerable magnitude," because otherwise we should have to assume "that the greatest elevatory movement always conforms to the central axis of every chain," or to that "of every watershed." "But sometimes upheaval will be in excess in the lower part of the valley, and at other times (which would equally produce lake-basins) there would be an excess of subsidence in the higher regions, the alluvial plains below sinking at a less rapid rate, or being, perhaps, stationary." And here I must be allowed to remark that these considerations did not escape me when I wrote my memoir "On the Glacial Origin of certain Lakes"; but I rejected them (I now see, unwisely) as random surmises, not comparable in value to the various hypotheses I discussed, and as I believe disproved, viz., that the great Alpine lakes "lie in simple synclinal troughs," or in "areas of mere watery erosion," or in mere "lines of dislocation," or "in areas of special subsidence." I shall now show why I rejected and still reject both of the above suppositions proposed in the 'Elements.'

Referring to 'The Antiquity of Man,' Sir Charles very properly assumes that the large valleys of the Alps were of preglacial origin,—a good and a sound assumption, founded on definite proofs as old as the days of Charpentier, if not older, and one I had occasion in this Magazine to show may have originated in subaërial actions that have been going on ever since the close of Miocene times*. No one, therefore, ever dreamed that "the rivers had been idle for a million of years or more, leaving to glaciers the task of doing, in comparatively modern times, the whole work of excavation." But the question he proposes to solve is, how, controverting my proposition of glacier-erosion, parts of these valleys may have been converted into lakes. Let us take the Lago Maggiore as an example.

If in this case upheaval was "in excess in the lower part of the valley," what would the result be?

From the deepest part of the lake to its efflux is a distance of about twelve miles, and the average angle from the deepest part to the efflux is 2° 21′, and, giving every advantage to any one who prefers upheaval "in excess in the lower part of the valley," I will assume that the axis of movement lay on a line coincident with the deepest part of the lake, or, in other words, that the hinge, so to speak, of the movement lay there. Before the upward movement began, the whole slope must have been downward towards the valley of the Po, otherwise the drainage would have been dammed up; and it needs little reflection to see in that case that the point which is now the efflux of the lake must have been 2625 feet, or say 2650 feet lower than at present, so

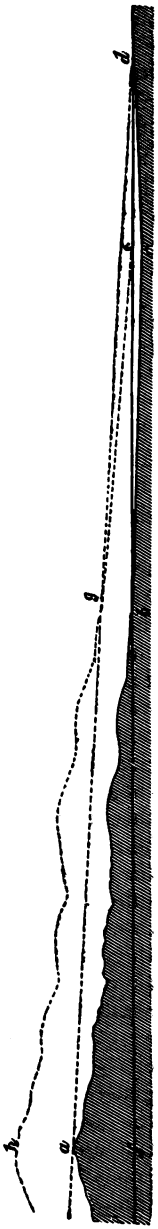
* Phil. Mag. November 1862.

as to bring it at the least to a level a little lower than what is now the deepest part of the lake. Without this the onward southern flow of the water could not have been established. But in that case the plain of the Po outside the present efflux of the lake must also have been at the least 2650 feet lower than at present, that is to say, before the tilting began; in which case the plain must have been about 2000 feet below the level of the present sea, and liable to be covered with deposits of that post-pliocene epoch. But no trace of these deposits exists, and they have never even been imagined; for the post-pliocene deposits of the valley of the Po are older than the ancient glaciers. The only escape from this is to suppose that when these movements took place the whole region lay so high (from 2000 to 3000 feet higher than at present) that elevations and depressions had no immediate relation to existing sea-levels.

But though Sir Charles alludes to upheaval "in excess in the lower part of the valleys," he rejects it in the case of the Alpine valleys, and prefers another hypothesis to account for the actual existence of the lakes as they now stand; and this I shall now examine.

"The Alps," he observes, "are from 80 to 100 miles across. Let us suppose a central depression in this chain at the rate of 5 feet in a century, while the intensity of the movement gradually diminishes as it approaches the outskirts of the chain, till at length it dies out in the surrounding lower region." Thus in time the valley-slopes that originally all declined outwards and downwards from the central elevations of the mountains, would in the lower regions, by depression of the central ridges, by degrees acquire a reverse slope, that is to say, towards their ends they would slope inwards to the mountains, and by this process the drainage would become dammed up by rock, and lake-basins would be formed. Now, to test this idea, we must take the distance between the efflux of lake and lake on the opposite sides of the chain. From the outflow of the Lago Maggiore to that of the lake at Lucerne, the distance is roughly about ninety-three miles in a straight line; and if we measure another line as far as the north end of the Lake of Zurich, the distance is about 112 miles. It will make no material difference in my argument which line I take; but let us take the latter, for it is clear that the rule of subsidence ought to apply to the lakes in general on both sides of the Alps. A point halfway between the outflow of the Lake of Zurich and that of Maggiore lies near the upper end of the valley of the Rhone, that is to say, just about the main centre of the Alpine chain. The distance, then, from the south end of the Lago Maggiore to the central point of subsidence in the Alps was about fifty-six miles. Now the

Lago Maggiore from end to end is about thirty-three miles long, if we disregard the curve of the lake, and from its efflux to the Borromean Isles, where it is deepest, is twelve or thirteen miles, and the average upward slope of the bottom of the lake to its outflow from that point is about 2° 21′. If, then, the chief line of depression lay in the centre of the Alps, and if that depression was the cause of the formation of the lake, then it is evident that, before the bottom of the lake assumed its present form, the whole region, from its outflow to the centre of the Alps, must have been so tilted, that the present upward slope from the Borromean Isles to the efflux must have sloped the other way (viz. south) at some angle, however small. And here I must have recourse to a diagram; for experience has shown me that many admirable geologists are yet exceedingly apt to exaggerate or else to neglect their angles. It is, as near as may be, on a true scale. Let *a* be the crest of the Alps, say 14,000 feet high, *b* the northern end of the lake, *c* its bottom, 2625 feet deep, *d* its efflux, and *b d c* an angle of 2° 21′, viz. the slope of the lake from *c* to *d*. What we have got to do is to alter the general levels of the country by a maximum upheaval at *a*, so that the line *c d*, instead of sloping upwards from *c* to *d*, shall slope downwards a little in the opposite direction, viz. from *e* to *d*. The depth at *c e* is 2625 feet; and to give the argument every point against me, let the axis of the movement lie at *d*. There the actual movement will be *nil*; and for every mile you proceed towards *a* the amount of upheaval will increase. To restore the country to its original form, as supposed by Sir Charles, let the point *c* be raised 2625 feet because of a general tilt of the solid country comprised between the lines *a d c f*, so as to raise the triangle *b c d* into the position of the triangle *g e d*. Then it so happens that the line *d f* at *f* will be raised as near as may be to the point *a*; or, in other words, the crest of the Alps would be raised to *h*, and the whole range in this neighbourhood, at the period



before depression began, must have been 28,000 or 30,000 feet high during the greatest extension of the glaciers. If we make an allowance for denudation, of course the Alps were still higher. Depress the central ridge *h* till it attains the height of *a*, the axis of movement or hinge being at *d*, and the triangle *g e d*, previously filled with ice, would assume the position of the triangle *b c d*; and when the glacier melted, the hollow became filled with water*. This is asking a good deal; and if it were necessary to account for the greater snowfall of the old glacier-epoch in the Alps by increased height of the mountains, which it is not, though we might be inclined to grant Charpentier his 3000 feet, the difficulty increases when we are asked to grant an elevation five times as much; nor indeed is the question likely to be raised by any one who measures his angles and calculates his numbers. If we remove the point where the angular movement ceased out into the alluvial plain of the Po, the difficulty simply increases for every yard we carry it in that direction; and a little reflection will show that at no great distance the angular movement necessary to drain the lake would raise the Central Alps to a height of 60,000 instead of 30,000 feet†.

Neither have we any special reason to suppose that any of those oscillatory movements have frequently taken place in the Alps, such as are common accompaniments of earthquakes in volcanic areas; and the trifling instances Sir Charles gives of these in Cook's Strait, and of another gradual movement in Finmark of 135 feet, though they have some relation to the subject, yet cease to have any probable significance when we consider the magnitude of the movement needed in our case, and also that it is not only necessary to apply it for the formation of the rock-bound lake-basins of the Alps, but also to numerous other cases on the flanks of many mountain-chains, and not there alone, but to the widely glaciated undulations of North America. Has Dr. Julius Haast also been mistaken when, adopting generally my views, he accounts for the excavation of the rock-bound lake-basins of New Zealand by glaciers? or was there a central depres-

* For the sake of simplicity, I make no allowance for curvature, which would be very small even if the problem were reduced to extreme accuracy. To attempt this would be merely pedantic.

† If, however, we are seriously asked to grant the probability of such movements having taken place in the Alps at a geological period so late, it is difficult to see why Sir Charles Lyell should object to Professor Heer's hypothesis, that Europe and America were joined across the Atlantic when the Miocene flora grew in Europe. The depth of the Atlantic is not so great but it would be easy to carry a line across it in soundings not greater than the oscillations of level I have indicated the Alps are required to have undergone during and since glacial times. If any one can grant this for times so recent, it is easier to grant it for times so old as the Miocene epoch.

sion of the mountains there also? Does this theory of depression apply to the Scandinavian chain and the Swedish lakes, upon which Dr. Torrel told me my theory threw so much light? Is Sir Charles prepared, if necessary, to apply it to the Vosges and the Black Forest? Will it meet the case of the lakes of the Pyrenees, which, I am informed, quite conform to my views? and were the greater rock-basins of Cumberland formed by a depression of the centre of that cluster of mountains, so that, instead of the lakes ranging on either side of a line of strike, they all radiate outwards from a centre? How will it suit Loch Lomond, Loch Katrine, and Loch Awe in Scotland, or, better still, Loch Ericht, Loch Rannoch, and Loch Lydach, which stand towards each other something like the legs of an old Isle of Man penny? and what of all the other myriad lakes of the Highlands, which trend east and west, south-east and north-west, north and south, and to every point of the compass, in accordance with the run of the valleys that gave a direction to the flow of the old glaciers? Were the marine lochs—once glacier-filled land-valleys—that open south into the Clyde, west into the Atlantic, and north into the North Sea, and which are generally deepest (like Loch Lomond) towards their heads—were they also tilted up at their ends by depression of the inland mountains? for no one who studies them is likely to assert that they are shallower nearer their mouths by mere gathering of sediment. And what about the lakes on the north watershed of the Himalayah? Is there no risk that we may be obliged to add 15,000 or 20,000 feet to the stature of that gigantic range to meet the exigencies of the case? When we come to the mountains and the rolling undulations of North America, where vast tracts are covered with unnumbered lakes, many of which are as large as those of Switzerland, what a variety of tiltings in all directions must have been produced to dam up basins, the outflows of which run towards every point of the compass! It must be remembered, too, that Sir Charles's supposition may be applied equally to these tracts, whether the lakes are entirely enclosed by rocks, or were dammed up by moraines and drift after the disappearance of the glaciers.

If, however, it be objected that the tilting that produced the great lakes on the south side of the Alps was not the result of a special sinking of the centre of the chain, let us take another supposition, viz. that the main line of depression lay on an east and west line, on the parallel of the north end of the Lago Maggiore and the Lago di Como; then, if the hinge or axis of movement lay east and west on a line at or near the south end of these lakes, the amount of depression for the north end of Maggiore would be about 7000 to 8000 feet, and that of Como

would approach the lowest of these numbers; while if we shorten the line still more, and place it at the Borromean Islands, we then get merely a special sinking of the bottom of the lake, —that for Maggiore, making no allowance for sediment, being 2625 feet. Considering, however, that the depression of the chain, according to Sir Charles Lyell's hypothesis, *ought to have been the means of forming the rock-basins on both sides of the Alps,* it is difficult to get rid of the idea that a great central depression was the cause, if there be any ground for the idea at all. In that case Charpentier's 3000 feet come very far below the mark. Indeed, if we must allow 14,000 feet of depression to form the lake-basins (still full of ice), one cannot see why there are not a great many more lakes than we find; for they ought also to occur in other valleys that run north and south of the central chain and open on the plains, but which are merely river-courses.

Again, how do the existing lakes conform to the regulation? Certainly those of Geneva and Neuchâtel do not in their trend agree with a supposed depression of the Central Alps; for their lengths and outflows are, roughly, at right angles to those of the other great lakes of Como, Lugano, Maggiore, Orta, Varese, Garda, Thun, Lucerne, Zug, Sempach, Zurich, and Constance; and to dam up the lakes of Geneva and Neuchâtel, we should require a central depression running north-west between them at right angles to the chain of the Alps, and quite across the Miocene rocks. For this we need a special proof, which has never even been attempted; and I do not see but that to produce the whole of the lakes by depression, the supposed great movement must merely resolve itself into a number of minor ones. A better supposition than this seems to me to be a special dislocation, or a special depression for each lake, which I have elsewhere attempted to disprove.

And now, to sum up the matter, let us see what I am expected to allow if I am forced to accept as proved the adverse points that have been raised against my theory. The Alps "*may have been* at the time of intense cold 3000 feet higher than they are now. They *may also* have been lower again." "The repetition of such unequal movements *may*, in a time geologically brief, turn parts of a valley into a lake." "*If there be no ice* during the movement, &c.," and "*should the movement* be very slow," "the river *may* afterwards fill up the cavity," and "*it may* afterwards cut through the new stony barrier." "*If the change* takes place in a glacial period, the thickness of the ice will augment from century to century, not in consequence of erosion, but simply because the contour of the valley is becoming gradually more basin-shaped. The mere

occupancy, *therefore*, of cavities by ice, by preventing fluviatile and lacustrine deposition, *is one cause* of the abundance of lakes which will come into existence whenever the climate changes and the ice melts." After so many *ifs*, and *shoulds*, and *may be's*, I submit that *may be one cause*, instead of *is one cause*, would have been a more appropriate mode of expression. These suppositions are backed by the additional statement, that if "we observe a capricious distribution of lake-basins, we have no reason to feel surprise, so long as we conceive the origin of such basins to be due to subterranean movements in the earth's crust; for these *may be* partial in their extent, or *may vary* in their direction in a manner which has no relation to the course of the valleys." I prefer to so many surmises the simplicity of my hypothesis, that as glacier-ice does erode the rocky floor over which it passes, and as it can, under certain circumstances, move up slopes, the nature of that erosion will be, and was dependent,

1. On the angles of the slopes over which it passed, when these slopes were seriously appreciable.

2. On the fact that the glaciers sometimes passed from these slopes into low grounds, into which the great old glacier-valleys opened.

3. That at the mouths of these great old valleys, and sometimes near their mouths, where two or more great glaciers met, the downward pressure of all the accumulated ice of all the tributary valleys would be greatest.

4. For, because of its inertness in such flat ground, the grinding-power of the ice urged on from behind would be greatest, in accordance with all known physical laws; and

5. That, as it progressed and melted, the ice must have been thinner, and must have exercised less erosive power than where it was thick, whence the gradual slope of the bottom of these lakes towards their outflows.

Sir Charles does not deny that glacier-ice may move up a slope. His idea of tilting supposes it, for the lake-hollows were not filled up with sediments, because they were filled with ice; but, apart from this, on one side there is immense complication of phenomena, which, to meet his case, must be applied to all the mountain-chains and clusters I have ever seen, and, as far as I know, to all of them I have ever read about, and, besides, to all the length and breadth of the northern half of North America, while on my side there is, at all events, simplicity.

As for the surmise that icebergs are likely to hollow out lake-basins of any importance in solid rocks, I have already discussed the subject; and it is so immaterial to the main argument, and seems to me so utterly improbable, that I will not at present renew the discussion.