DO GLACIERS EXCAVATE?

ALL geologists agree that ice plays a part in sculpturing the features of the earth, but they differ as to the extent and importance of its work. For instance, some believe that the "northern heights of London" once formed the southern limit of an ice-sheet, which began its journey in the mountain districts of Scotland and of Scandinavia; some, that glaciers excavated, not only the tarns in corries, but even the great sub-Alpine lakes, while others restrict the area occupied by continuous ice and minimise its erosive powers. For full thirty years, since the publication of the late Sir A. Ramsay's memorable paper,* the origin of lake-basins has been a standing dish in geological controversy. He attributed them to the erosive action of ice, and his hypothesis, at first, won many adherents. Then it seemed to be losing favour, till at the end of last year its defence was undertaken by Dr. A. R. Wallace.† Against a champion so formidable I should be afraid to enter the lists did I not remember that his laurels were won on fields where heat is the rule, and cold the exception. Of this I think some signs appear in his ingenious apology for the efficacy of glaciers. That reads like the plea of a skilful advocate, who has mastered his brief and read up his subject, and yet fails to feel the force of either a difficulty or a general argument quite so readily and so keenly as a man whose knowledge has been gained by personal experience.

Be this as it may, we are indebted to Dr. Wallace for defending with remarkable force and acumen the claim of glaciers to be the excavators of the great sub-Alpine lakes. Nevertheless, his argument occasionally appears to me inconclusive, and so, as my name

* *Quarterly Journal of the Geological Society*, vol. xviii. p. 185.
† *Fortnightly Review*, Nov. and Dec. 1893.

occurs several times in the course of his article, I venture to offer some criticisms on it, though conscious of the difficulty of dealing with so complicated a subject in the space of a few pages. Throughout I shall speak for myself—fellow-workers with whose views on glacial geology I generally agree may differ from me as to particular points, or I from them.

Accordingly, to prevent misunderstanding, and to clear the ground, I must once more repeat that I have never refused to accept certain rock-basins as the work of glaciers. These, however, I believe to be rather small and shallow, and generally in somewhat exceptional situations, like the tarns in corries. Some of the lakelets so plentiful in districts like Sweden and Finland may have a similar origin, though with these difficulties begin to arise. The origin of many of the Cambrian, Cumbrian and Highland lakes seems to me a moot question, and one where the shield sometimes may have two sides. But I maintain that glaciers have had practically nothing to do with the formation of the larger Alpine lake-basins. Thus the question between us is one of degree, of where the line is to be drawn between the work of glaciers and the results of other agents.

Again, Dr. Wallace appears to me sometimes to fail to distinguish between *abrasion* and *erosion*, as when he challenges me either to deny that the major part of the North German drift comes from Scandinavia or to admit that the erosive action of glaciers is great. Surely I might grant the one and yet deny the other: for we could obtain the same quantity of débris from a block of stone, either by chiselling off one of its edges or by chipping a hollow in one of its faces. But he also falls into the common error of supposing that the quantity of débris extruded from beneath a glacier is a measure of its excavatory (erosive) power. In disproof of a statement "that the action of glaciers is entirely superficial and that they actually preserve the surfaces they cover from denudation" * he refers to Dr. Penck's estimate of the quantity of mud brought down by the torrent from the Aar glacier as a proof of the amount of rock which is being removed from its bed. But he forgets some important facts. From the crags on either side earth and dust fall upon the surface of the glacier, part of which ultimately makes its way to the bottom; débris, fine and coarse, is hurried down by streams from lateral snowbeds and glaciers, and is carried at last beneath the ice into the main torrent. The very stones which do the work of grinding the bed of the glacier must be also worn away, sometimes, perhaps, quite as fast as the rock itself.† Again, when a glacier first invades a new territory,

* As this statement is coupled with my name I may say that it is inexact as a summary of my views.

† Does not the fact that the stones which have travelled beneath ice are so often sub-angular indicate that usually the amount lost by them has not been very great, and if so, the effect produced on the bed of the glacier has been on a similar scale?

it must often find the surface covered with loose débris, large and small, and this, if it is to begin rubbing away the subjacent rocks, must first be swept away and carried on by the advancing ice. It is only when the whole region has been buried for long beneath an ice-sheet that the mud in its effluent streams approaches to being a measure of sub-glacier abrasion or erosion. To some extent, indeed, Dr. Wallace has receded from this position,* but I think that had he been familiar with glaciers from personal experience it would not have been left for a friend to indicate the unsoundness of the argument.

As I have not seen the North German boulder clay, I will say no more on the question of its origin than that I do not feel bound to admit that, though this drift may be a result of ice-action, it has been deposited by an ice-sheet; for it not seldom happens that I am unable to understand how certain glacialists have arrived at their conclusions concerning facts with which I also am acquainted. I shall, therefore, restrict myself to discussing the boulder-clay of England, which deposit seems to be regarded by Dr. Wallace as a proof that a very large part of this country was buried beneath a moving sheet of ice. That the condition of some parts of Great Britain was once generally similar to that of the more southern portion of Greenland I admit, but the real question at issue is this—How far did the British glaciers, even if they sometimes became confluent, advance beyond the mountainous regions into the lowlands, and what deposits are directly due to them? Owing to the calls of other work I have never found leisure to piece together the observations made from time to time, during a period of some twenty-five years, and to track the ice continuously from the "gates of the hills" over the lower ground; hence I cannot venture to fix the exact limit of the land-ice at any given epoch, and must deal with the question by selecting certain places at which the detrital material, as it seems to me, cannot have been deposited by an ice-sheet.

Before doing this I must call attention to the following important facts which, as he makes no reference to them, I think must have escaped Dr. Wallace's notice. In Arctic regions, at the present day, great quantities of débris, and even large masses of rock, fall every winter from the cliffs on to the ice-foot, and even on to the edge of floes. Besides this, boulders, shingle, and other material on the shore are frozen into the ice, and this is worked up and down by the action of tide or wind, so that pebbles are striated and other effects of glaciers imitated. Thus, a boulder-clay in itself is no proof of the former presence of an ice-sheet. Other considerations must also be taken into account before the question can be decided. The "foot" and the floe ice, at the break-up of the winter, get loose and drift

* See his letter in *Nature* (January 4, p. 221). Here also the confusion already noticed is exhibited in the words "the clayey element in it [the drift from Scandinavia] would be due to *erosion*." It might be equally due to *abrasion*.

away from land, often bearing with them in-frozen boulders and even the material which has slipped from the cliff. There is no evidence of which I am aware to prove that an ice-sheet, under any circumstances, ever uproots considerable masses of rock. Its erosive action, if such there be, must be mainly a process of rubbing and scraping. When large masses have been transported by it they must have fallen from crags on to its surface—that is, they belong to glaciers rather than to an ice-sheet proper, which buried the greater part of the region.

Proceeding then to English examples, let us glance first at the noted instance of the "till" and "contorted drift" at Cromer. The one is a mass of more or less sandy clay, full of rounded and sub-angular pebbles of chalk, of less worn fragments of flint, with other stones and boulders, some of which have travelled far, perhaps even from Scandinavia; the latter consists of stratified sand and gravel, often exhibiting strange contortions, in which are embedded huge masses of chalk, both solid and *remanié*, and of gravel, presumably frozen when it was transported. These probably have not travelled far, and might be easily detached from cliffs by the action of frost, as described above; but if they were torn up by an ice-sheet it must have rooted like a pig. The sand and gravel, with which they are almost exclusively associated, precisely resemble those usually referred to the action of currents. At first, however, these must have often enclosed very large masses of ice—rafts which have foundered with their load of boulders—and as this ice slowly melted, its disappearance would cause flexures, distortion, and slipping in the stratified gravel. There is nothing in the structure of the till itself adverse to the idea that it has been deposited under water. It rests on a light-coloured stratified sand, which is practically undisturbed, and the change from this to the clay is not more rapid than is often seen in ordinary bedded rocks. Occasionally also the till and the overlying sand occur in very regular and thin alternating bands. To attribute the Cromer drifts to an ice-sheet constantly leads to contradictory conclusions; for this would oblige us to assume, in the same cliff, now that the ice ploughed up loose materials, and now that it passed over them without producing any disturbance.

In the neighbourhood of Wellington (Shropshire) I have seen boulder-clay resting on and even interlaminated with an undisturbed stratified sand, which contains marine shells, more or less broken. In Leicestershire boulder-clay rests, now on Keuper marl, now on various hard rocks; but the former seems undisturbed, the latter are not rounded. Yet, though I have never seen a characteristic ice-worn rock in all the Charnwood district, boulders from it have been distributed, especially towards the south and south-west, as far as twenty miles away.

Dr. Wallace refers to the great streams of erratics which can be

traced from certain centres over various parts of Britain. But to attribute these to land-ice involves us in constant difficulties. One example may suffice to indicate the general character of these. Boulders of granite from Shapfell are found on the eastern coast, between Scarborough and the Humber, and are scattered over the vale of York. How they managed to cross Stainmoor, which is in places more than a hundred feet above the highest outcrop of the granite, is a puzzle on any hypothesis; but if we invoke the aid of an ice-sheet, we must suppose this to have travelled roughly from west to east right across the path of the still larger mass which was coming from the north.

The boulder-clays not unfrequently contain foraminifers and marine shells; but the latter, as they are commonly broken, are supposed to have been caught up and transported by the advancing ice from the sea-bed (possibly then dry land). If so, we must concede that moving ice does very frequently erode soft materials. Of late years the same explanation has been applied to certain shell-bearing gravels which are closely associated with boulder-clays. One of these, mentioned by Dr. Wallace, at Moel Tryfaen, is over 1300 feet above the sea. Here the stratified sand and gravel is overlain by boulder-clay. I know the place well, and stood for some time last September on the summit, trying to understand, but without success, how such a mass of material could be carried uphill from the sea by even the biggest of ice-sheets; how it either retained or re-acquired stratification, by what path the ice came and what caused it to move uphill.* Moreover, notwithstanding what Dr. Wallace says, I remained convinced that the ice from the North Sea would have been kept at bay by the native ice and snow of Wales.† But the case of Gloppa, over which Dr. Wallace passes lightly, is yet more difficult. Here is an extensive mass of stratified gravel, over sixty feet thick, more than thirty miles from the sea and 1100 feet above it, containing at least sixty species of shells, many of them uninjured. Other instances might easily be given, such as that at Wellington, though this, perhaps, is the strongest; but we may add that the frequent intercalation of bedded sands, &c., in the British boulder-clays, is not easily explained on the hypothesis of land-ice. Is it not also somewhat inconsistent to claim a considerable elevation of the land in early glacial times and yet to dispute a submergence during them, as if it involved an *a priori* improbability; especially when it is generally admitted that,

* Dr. Wallace seems to think it has been denied that ice can *under any circumstances* move uphill. Certainly I have never said this. What I assert is that there must be an adequate cause, and this, in many cases where such motion is alleged, appears to me to be wanting.

† Dr. Wallace states that a riebeckite rock from Ailsa Craig has been found at Moel Tryfaen. What I found appears to me more like that of Mynydd Mawr, in the immediate neighbourhood. But the presence of the former is as explicable on the one hypothesis as on the other.

even in Britain, there has been subsequently some upheaval, and this, in other countries, has often reached 600 feet and sometimes twice that amount.

But the argument on which, perhaps, Dr. Wallace chiefly relies is that lakes, of a particular kind, are abundant in mountain regions which have been glaciated, but not in others. This argument was, indeed, employed in Sir A. Ramsay's original paper, but it has never before been presented in such detail or with so much force.

Before discussing it, I may remark that Dr. Wallace errs in supposing that I have asserted the Alpine lake-basins to be older than the glacial epoch, for, so far as I can remember, I have never made any such statement, and for several years have entertained strong suspicions that they are not so. But, as the expression of this opinion, in the present state of the evidence, would have made an excellent "red herring" in the controversy, I have carefully abstained from committing myself to either view. Another minor point in his argument is also an assumption—viz., that the movements of the earth's crust during mountain-making are exceedingly slow. As I am no "convulsionist," I am willing to admit that this is possible; but, so far as the evidence goes, it points rather in the opposite direction—viz., that ages of comparatively rapid upheaval have alternated with periods either of repose, or more often of slow movement in the opposite direction.

Proceeding, then, to Dr. Wallace's main argument, we find that the production of a lake-basin is attributed to a combination of favourable conditions, so that the absence of it may be due to various causes. For instance, if the slope of a valley be uniform, then this will be equally deepened; but when it becomes nearly level, at the foot of a more marked descent, then excavation commences. In this statement there is, I believe, a certain truth; but, if that be the explanation of lake-basins, they should be much more abundant, for similar changes of level are by no means uncommon features higher up the valleys. To the general question of the amount of abrasion which must be assumed we shall presently return.

We pass, then, to a more direct argument, which is thus stated: "If we look at the valley-lakes of our own country and of Switzerland the first thing that strikes us is their great length and their situation, usually at the lower end of the valley, where it emerges from the higher mountains into comparatively low country." Of these statements the first is generally true, though it is not without exceptions, such as Orta and Zug. Moreover, if the larger lakes, as I suppose, have been formed by flexures in the beds of pre-existing valleys, I expect to find them generally long in proportion to their breadth.

The difficulty, why each of the great Alpine valleys is not provided with a lake, since each was traversed by a great glacier, is thus met.

Since ice is not a very plastic substance, it may not always touch the bed of a narrow valley, but may rest upon the sides, and form a kind of arch. It is quite true that ice, as a rule, seems to succeed in bridging over a very narrow ravine, but, after careful study of this question, I have no hesitation in saying that a glacier has usually reached the bottom of any ordinary V-shaped valley, even when its sides are pretty steep. To this matter I shall return, for it has an important bearing on the general question of the effect of glaciers. In the next place, Dr. Wallace explains the absence of a lake-basin at the opening of the Dora Baltea valley by saying that they formerly existed at Aosta and Verrex. But this does not apply to the valleys of the Stura and the Adige, and further, seeing that a branch of the Rhine glacier could erode the Wallen See, before proceeding to help in making the lake of Zurich, we can hardly hold that some youthful energy on the part of the Dora Baltea glacier is an excuse for senile inactivity. Moreover, these early efforts in basin-making (I know the district), if they existed, were comparatively unimportant.

Dr. Wallace, as it seems to me, really fails to meet the difficulty which the singular form of the Lake of Lugano presents to a glacial erosionist. The size of its drainage area is of little importance: the watershed to the north is no doubt "moderately high"—sufficiently so to stop the inroads of ice from the main chain, but not high enough to give rise to local glaciers of great magnitude on its southern slopes. Suppose, however, we admit that an offshoot of the Como glacier did trespass on the area of the lake *viâ* Porlezza—a route which would demand considerable plasticity in the ice, as a glance at a map will show—must we also assume that it bent back northwards round the headland of Morcote? Did it then descend the Tresa, or did an offshoot from the Maggiore glacier come up that valley to help it in grubbing out the basin west of the Monte Salvatore? The ice of the glaciers which excavated the basins of wriggling Lugano and forked Como must at any rate have been a tolerably plastic substance.

It is urged, further, that the greatest lakes, as Geneva, Constance, and Maggiore, lie in the paths of the greatest valleys—*i.e.*, of the largest ice-streams. To the last-named lake Dr. Wallace attributes a maximum depth of 2500 feet,* and says: "Geologists will probably not think 30,000 years an extravagant estimate for the duration of the glacial period" (it may be remembered that these lake-basins were covered only during the epoch when the glaciers attained their largest dimensions), "in which case an erosion of only an inch in a year would be sufficient." I think Dr. Wallace would find it very difficult to prove that a glacier would remove the ordinary Alpine rocks at such a rate as this; but letting that pass, I must remark

* Possibly Dr. Wallace has consulted some recent authority which I have not come across. Those with which I am acquainted give the maximum depth at only about half this amount.

that if the glaciers were at work for 30,000 years, and could erode, under circumstances of moderate provocation, an inch in a year, they must have produced very striking effects on the upper valleys of the Alps. How far they have so done we shall presently see.

Dr. Wallace then calls attention to three criteria by which basins of glacial erosion are distinguished from ordinary valleys. In the first place, they never present those peculiarities of contour which are not infrequent in mountain valleys and never exhibit either submerged ravines or those jutting rocky promontories which are so common a feature in hilly districts. But what are we to say to the rocky headland of Sermione with its steep scarp looking up the Lake of Garda? I could mention other cases. But ought lakes to show submerged ravines? Apart from the likelihood of these becoming filled up with mud more rapidly than the wider part of the basin and being thus obliterated, this structure—sloping sides descending to a ravine—is always a great rarity when a valley has begun to open out, and so is the last form to be expected in the beds of lakes near "the gates of the hills." But I am surprised to find Dr. Wallace referring to M. Delebecque's *Atlas des Lacs Français* in support of his views. These maps show that the contours of the surrounding hills are generally repeated beneath the water; but for this, if the basins be scooped out by ice, there seems no reason. For instance, the bed of the Lake of Geneva descends rapidly beneath the steep mountains at its upper end, and continues deep, though with a more gentle gradient, opposite to the more open region around Lausanne. In the Lac de Bourget the contour lines are crowded together beneath the steep slopes of the Mont du Chat. In the lake of Annécy a headland and island occur at Duingt right in the path of the ice-stream, and a buried steep-sided hill, about 160 feet high, at the Crêt de Châtillon. Besides these, how could a glacier excavate that extraordinary round hole, about the same depth below the general level of the lake-bed, at Bourbioz, near Annécy? If this is a "giant's kettle," it is indeed a monster. Then what shall we say to the strangely irregular form of the Lac d'Aiguebelette. Its broadest part is interrupted by the shoal carrying the Grande and the Petit Ile. From this its bed descends—apparently in the most open part of the lake—to a depth of 233 feet. Look also at the snakelike form of the Lac des Brenets, about 100 feet deep, with the curious bifurcation at its upper end; through one arm of which the channel of the Doubs can be traced into the deeper part, separating a bed generally about 15. feet below the surface, while the other arm forms a backwater. This single sheet of the Atlas shows what varied forms these lakes can assume, and the work as a whole presents to us a number of basins, some lying in the path of the great ice-streams, others quite out of it, others again in regions which only can have been invaded very incompletely

or for a short time by a glacier, and of these certain lie transverse to its path and parallel with protecting ridges.

This is the second critical character: that "Alpine lake-bottoms, whether large or small, frequently consist of two or more distinct basins, a feature which could not occur in lakes due to submergence unless there were two or more points of flexure for each depression, a thing highly improbable even in the larger lakes, and almost impossible in the smaller." These undulations in many cases only amount to a very few yards and may be due to the unequal deposit of débris from retreating glaciers, but, in the more important cases of the larger lakes, what is there wonderful in finding undulations in a line of general flexure? These are very common in regions where rocks have been bent. Is Dr. Wallace's hypothesis free from difficulties? Suppose the Lake of Lucerne to have been scooped out by glaciers, we may fairly ask for an explanation of the Küssnacht arm. Granted that the moderate ice-contingent from the direction of the Brunig Pass may have produced some deflection in the Reuss glacier (which must have taken the same path as the river), this could hardly have forced the latter to send out an offshoot almost at right angles to its general course. We are thus landed in the old difficulty about the origin of the Lake of Zug, 650 feet deep. Did the aforesaid offshoot descend from the top of the "Hollow Lane," or was the work done by an arm, sent off at Brunnen, which first scooped out the Lake of Lowertz? Here is a basin, deep for its size, just at a place where we should expect the ice to be least active!

The third characteristic assigned to lakes of erosion is this; the contour lines in most river-valleys run up the tributaries for a certain distance, so that on taking them at heights of "two or three, or five hundred feet above the river," these would "form a series of notches or loops of greater or less depth at every tributary stream with its entering valley or deeply cut ravine," but in the lakes of glaciated districts, the water never forms inlets up the inflowing streams, but "all of them, without exception, form an even junction with the lake margin just as they would do if flowing into a river." As an illustration diagrams are given of the Dart and the valley of the Tweed, (contour line of 700 feet) on the one side, and of Ulleswater and Como on the other. Quite so, but Dr. Wallace forgets that mountain streams bring down great quantities of débris into the lakes in question. The deltas from these have made the shores generally even, nay, sometimes have trespassed upon the lake. But on examining the contour lines on the hill-sides, let us say up to 700 feet above the water, we should find the usual loops. Perhaps it may be said that these curves are above the water-line; yes, but apart from the fact that some of the lakes once reached a higher level, can we suppose that such huge moving masses of ice restricted their energies only to the area now covered by water and produced no effect on either side?

Lastly, we come to the general argument founded on the occurrence of these valley-lakes in the marginal zones of glaciated mountain regions. Here, notwithstanding the ingenuity of his arguments, Dr. Wallace seems to me to have failed in discovering characteristics peculiar to the larger lakes in glaciated regions and distinguishing them from lakes which are not claimed as products of ice-erosion. In the Alps, for instance, Zug and Orta seem specifically identical with the Egeri See and the Lago di Ritom, and are comparatively short lakes. It is the long valley-lakes which appear to be less common, so far as I know, in other regions; but it is singular that those lakelets which we should agree in attributing to ice more usually are roughly ovoid in form. But if we argue from shape, why should we not claim the Lake of Capernaum and the Dead Sea as the work of the Lebanon glaciers? The former is short, and the latter in olden time was long enough, and both are not ill situated for the action of ice, if only it could be got into the Jordan valley! Or where are we to draw the line in North America, where the lakes are of all shapes? Reduce the scale of Erie or of Ontario, or even of Michigan, and they would compare well with some of the Alpine lakes. Glacialists, indeed, have cast longing eyes upon the region of the great American lakes, but this has been shown by Professor Spencer and others to be a submerged river system, in which differential movements have occurred at comparatively late dates. I am surprised that Dr. Wallace has not referred to this question.

Again, though it is a fact that lakes are common in certain glaciated regions, this is not conclusive. There are curious tarns on the uplands of Brazil where are no traces of glaciation, and lakes in parts of India, according to Mr. R. D. Oldham,* out of the reach of glaciers. They are by no means universal in glaciated regions, for if we rule the Pyrenean glaciers out of court as inefficient to erode—though one of them was forty-four miles long, and they came down to the lowland in places very suitable for digging—what can we say to those lakes which either lie out of the paths of great glaciers, or where these can have had little influence? Can the puny local ice-streams of the Jura, if such there were, have dug basins, when their larger brethren in the Pyrenees were helpless? But why not attribute Nicaragua and Titicaca, and the lakes in San Domingo and Porto Rico, in Celebes and Tasmania, to glacial action? I know these only from maps and descriptions, but I cannot see how they differ in shape from Alpine lakes. In the last island, glaciers have existed, but they never can have been very important.† I must continue to plead that I cannot distinguish, except for size, the lakes of Africa and the basin of the Sea of Marmora, from some of the greater Alpine lakes.

* *Nature*, vol. xlix. (1893) p. 30, *cf. ibid.* p. 197.

† On this point see Sir H. Howorth's letter, *ibid.* p. 30.

Lastly, I repeat my statement that in districts which have been abraded by ice we find no evidence that very great quantities of rock have been removed, even in valleys which have been traversed by the largest ice-streams. I know from personal examination almost every important valley in the Alps, and could fill, were it necessary, pages with extracts and diagrams from my note-books to justify this statement; but as this would be wearisome, I content myself with repeating, if possibly with greater emphasis, for I have made further studies, the words used in my lecture last spring.* After describing the valley of the Aar above the Lake of Brienz, I continue:

"Perhaps no district in the Alps exhibits the traces of ice-action on a grander scale, yet these have been only superimposed upon and modify the features of fluviatile erosion. Yet the contours of ice-action . . . . can be traced almost down to the very surface of the torrents. But the Haslithal is not at all an exceptional case. I have examined almost every important valley which leads up into one of the greater groups of crystalline peaks in the Alps, with the same result—namely, that the major features, whether in crag, rock, slope, or ridge, are those of the ordinary processes of meteoric and fluviatile erosion, the minor only being due to glacial action. Hence it follows that when the ice first emerged from the fastnesses of the central peaks, it descended valleys corresponding in their main outlines with those which still exist, say nearly identical in depth and breadth; but at the same time every crag was rough, every ridge was sharp or serrate. The ice took possession of the region. It rasped and rubbed, and when it finally disappeared the rock surfaces were worn and defaced like the sculpture of some bas-relief which has been trodden under foot till only the main outlines of its design can be distinguished. The Val Bregaglia, the Val Mastalone, the Val Anzasca, the valley of the Dranse, and many others have afforded me the clearest proofs that the ice has occupied without materially deepening, excavating, or modifying the glens. Crags which as it advanced must have risen up like peel-towers from the floor of the valley have been buried deep below the frozen mass, and have emerged, worn, rounded, scored, but only so far changed as to have become humps."

Hence, notwithstanding Dr. Wallace's ingenious advocacy of the erosive power of glaciers and ice-sheets, I maintain that these can excavate only under the most favourable conditions, and then but to a limited extent, and that they are proved by a close study of the Alpine peaks and valleys to have been incapable of hollowing out the great lakes of that chain. In effect he asks us to believe that an agent which has failed to do more than modify the physical features over which it has passed, even when concentrated between the mountain slopes, which has failed to give any signs of excavating power except under circumstances peculiarly favourable, should be able, when it has reached a milder climate, is moving down the gentlest slopes, and is beginning to spread out laterally, to excavate basins in solid rock, not a few miles in length and hundreds of feet in depth. Truly this would be like an old man's effort to make up for the lost opportunities of a wasted life.

T. G. BONNEY.

* *The Geographical Journal*, June 1893.