NO. XXI.—ON THE GLACIAL PHENOMENA OF SCOTLAND, WITH
REFERENCE TO THE REPORTS OF THE BOULDER COMMITTEE
OF THE ROYAL SOCIETY OF EDINBURGH. BY DUGALD BELL.

[Read 12th May, 1887.]

THE Twelfth Volume of the *Proceedings of the Royal Society of Edinburgh*, which has lately been added to our Library, contains the "Tenth and Final Report of the Boulder Committee, with Appendix containing an Abstract of the information in the nine Annual Reports of the Committee, and a Summary of the principal points apparently established by the information so received." This bears to have been presented to the Society in July, 1884. The Committee was appointed in 1871, so that their labours extended over a period of thirteen years. The publication of this final *Report*, and of the Summary which it embodies, seems to furnish a favourable opportunity for reviewing the work of the Committee as a whole, and endeavouring to estimate and sum up the results.

These *Reports*, it is scarcely necessary to say, contain a great mass of interesting information regarding boulders and their distribution in all parts of our country; information which has never before been presented in so complete and authentic a form, and which cannot be overlooked by any future student of the subject. In collecting it, the Committee have certainly rendered an important service to Geological science. Special acknowledgments are due to the Convener, Dr. Milne-Home, for the energy and enthusiasm with which he threw himself into the work. As has been said elsewhere—"At a time of life and after services which might entitle him to leave such observations to younger men, he has gone over the greater part of Scotland, visiting every remarkable boulder he could hear of, measuring, sketching it, noting its position, and otherwise putting it on record." ("Among the Rocks round Glasgow," p. 162.) We repeat this tribute all the more readily that we shall have occasion to differ widely from Dr. Milne-Home with regard to the interpretation of many of the facts recorded, and with regard to his general theory of the glaciation of our country—a difference which we hope always to express in a manner consistent with the respect due to him as a gentleman of high reputation and a veteran promoter of the science.

One does not go far in these *Reports* without discovering that

the Convener is an out-and-out advocate of the "iceberg hypothesis;" glaciers or land ice he introduces as little as possible, apparently regarding bergs or floating ice as able to account for almost everything that is required. All the phenomena under notice he thinks may have been produced by icebergs carried by ocean currents from the north-west, and drifting over the country at a time when it was submerged to a depth of about 2,000 feet beneath its present level.

This theory he seems to have been led to adopt from his observations in the eastern and south-eastern parts of Scotland. Finding that it accounted fairly well, on a general view, for the facts observed in these districts, and also that it seemed to be consistent with those observed in the north-eastern counties of Elgin and Nairn, and parts of Ross-shire, he apparently came to the conclusion that it was universally applicable in Scotland, and did not see the difficulties, or rather, we should say, the insuperable objections, which attend it when viewed in relation to other parts of the country.

Thus we read in the 2nd *Report*—"On the mainland of Scotland, at least in its eastern half, the boulders appear to have come over a wide extent of country in one and the same direction, viz., from the north-west. This inference had been drawn years ago from such facts as the finding of granite and mica-schist boulders in the counties of Fife, East Lothian, and Berwick, which must have come from the Scottish Highlands. But further and more striking proofs of this great north-west movement are afforded by the large boulders in the counties of Elgin, Nairn, and Ross-shire," which, it is concluded, "must have been transported from mountains far to the north-west." (Vol. viii. p. 143.)* This, however, is by no means certain, as we learn afterwards they may have come from the sides of the Great Glen, in a direction from W.S.W. to E.N.E.

We need only select a few of the instances given:—

In Ayton Parish, Berwickshire, a boulder of same rock as Cockburn Iaw, 10 miles to W.N.W. Also, pieces of coal found in a bed of sand. Nearest place where coal strata occur is in Mid-Lothian, 30 miles to N.W.

In Coldingham Parish, rounded masses of hematite, from East

* The references throughout are to the volumes of the *Proceedings of the Royal Society of Edinburgh*.

Lothian, 30 miles to N.W. At Blackadder, a boulder of blue whin, from Hardens, 5 miles to N.W. In Hutton Parish, blocks of a peculiar greywaëke, from near Cockburn Law, 14 miles to N.W.

In the Parish of Dunse, a number of boulders, all carried in the same general direction from N.W. ; among them porphyries from hills 3 to 6 miles distant, mingled with pieces of sandstone from the immediate neighbourhood to the west.

At Linton, East Lothian, a greenstone boulder from the Garlton Hills, 6 miles W. by N.

On Craiglockhart Hill, near Edinburgh, boulders of various kinds from N.W.—some of sandstone from a distance of only about a mile. At Alnwick Hill, near Liberton, in excavations for water-works, a great variety of boulders from W. and N.W. ; some of red sandstone, such as occurs at Grange and Merchiston, others of marine limestone, similar to that in Linlithgowshire, others of quartz, etc., which probably came from the Silurian rocks near Callander. Boulders of mica-schist, doubtless from the last-named locality, also occur on the Pentlands.

Near Kirkliston, Linlithgowshire, a boulder of Old Red conglomerate, probably from the neighbourhood of Callander or Doune, N.W.

Further west, a considerable number of conglomerate boulders are found near Dunmore, Plean, Stirling, Bridge-of-Allan, and Doune, which, the Convener remarks, "are interesting, as the position of the parent rock is known, viz., that band of conglomerate which traverses the country at Callander." (See, *e.g.*, 4th and 5th Reports, vol. ix. pp. 677-683, and vol. x. pp. 591-605.) And so the conclusion is confirmed—"General movement from the north-west."

Of course those who hold the theory of a great development of glaciers or land ice as more consistent with the facts point out that all these instances, on which Dr. Milne-Home founds so much, occur on the *eastern side* of the principal mountain chains, from which it can be shown that the boulders and striæ radiated in *all* directions—N., N.W., W., S.W., and S., as well as E. or S.E. Whenever we come to the *western side* of these mountains, the marks of glaciation are in a different direction, indicating a movement on that side *westward* out to sea. On this point, we think a single day's excursion by Loch Earn or Loch Tay, up the valley of the Dochart to the neighbourhood of the Black Mount,

and then down Glen Etive or Glencoe on the other side, is sufficient to convince anyone who fairly observes the facts.

Similar facts are presented very clearly in the south of Scotland—a district too little noticed in connection with glacial phenomena. The high grounds of Carrick and Kirkcudbright have evidently formed a centre of dispersion, from which the striations and boulders can be traced extending outwards on every side—to the N. and W., as well as to the S. and E. This, of course, is quite inconsistent with the theory of an ocean-current and floating ice or bergs.

If there had been a general inflowing current, laden with ice and boulders, from N.W., its traces should be most distinct and abundant on the west coast, which was first exposed to its impact. But it is precisely there that the evidences of ice-movement in a different direction are most conspicuous and unmistakable. It is on this western side of the country that the difficulties in regard to the theory favoured in these *Reports* thicken and accumulate.

Thus—(1) In the neighbourhood of *Loch Glashan*, near *Loch Fyne*, the Convener writes—"I was rather surprised to see the hills which sloped down to N.E. well covered with boulders, and also striated rocks facing N.E. and N.N.E." Again, near *Port Sonachan*, on *Loch Awe*—"I was rather surprised to find the boulders in such positions as to indicate that they had come from N.N.E. . . . The rocks presented smoothings which faced N.N.E. . . . I felt surprised at this direction as, when last year in the Hebrides and the west coast of Argyleshire, I had been accustomed to see that N.W. was the usual direction both of boulder transport and of rock striations." (6th *Report*, vol. x.) This observation, it is right to state with all respect, is not concurred in generally by geologists.

It had been stated in a previous *Report* * that "when boulders are on hills, they evidently indicate a preference for the sides of those hills having a N.W. aspect—a fact which seems to indicate the existence and prevalence of some transporting agent which could be more frequently and effectively stopped by the sides of hills facing N.W." (2nd *Report*, vol. viii.) But here are some stubborn and intractable boulders "indicating a preference" for a different side of the hill altogether! There was no help for it but

* Although it is apparent that the Convener did a great part of the work of drawing up the *Reports*, we wish to adopt, as far as possible, an impersonal style of remark by referring simply to "the *Reports*."

to admit that "this N.N.E. direction of transport [*i.e.* from N.N.E.] appears to characterize all the boulders and the rock striations at the Gareloch, Loch Gair, and Loch Awe," evidently including Loch Long, Loch Fyne, and Loch Etive, and to suggest that "in this district there may have been a current of floating ice moving in a S.S.W. direction." Also, "that when the Highlands of Scotland were covered by the sea up to a height of, say, 2,000 feet, and when they presented an archipelago of islands, there may have been currents moving in different directions, and these directions changing as the sea fell from one level to another." (6th *Report*, vol. x.)

What is this but giving up entirely the "general movement from the N.W.," and virtually saying there is no order or certainty in the matter? But we shall refer to these "abnormal currents" further on.

(2) In noticing the *Crinan* district, which seems to us in itself decisive of the whole question of sea or land ice, the *Report* affirms that "there is nothing to suggest the action of a glacier," and adheres to the view that the phenomena in question were the work of sea and "floating ice." And Mr. Jamieson is referred to as having "stated in one of his papers in the *London Geological Society's Journal*, that he found "smoothed rocks on the east side of this valley, and striations running N.W. and S.E." The Reporter admits he "did not see these markings;" but he adds, "they are not inconsistent with the theory above suggested as to the transport of the boulders." (7th *Report*, vol. xi.)

This reference to Mr. Jamieson's paper ("On the Ice-Worn Rocks of Scotland," *Quar. Jour. Geol. Soc.*, vol. xviii. 1862), which is well known as one of the standard papers on the subject, is exceedingly inaccurate, and can only be accounted for by supposing that it was written from memory, under an erroneous impression, and without taking time to verify it. Mr. Jamieson distinctly states that the rocks of this district, presenting "some of the most beautiful examples of glaciation" he had ever seen, are marked with "long rectilinear grooves running uninterruptedly onwards from N.E. to S.W. for many yards, with all lesser degrees of scratching and polish." The ridges, he says, present invariably their rounded snouts to N.E., and their more rugged outlines to S.W.; and there is "evidence that the agent which impressed these furrows moved obliquely *uphill*, mounting a slope of 700 or

800 feet." In short, he finds "the glaciation of the rocks of Knapdale looks as if the stream of ice descending Upper Loch Fyne (so great had been its volume, and so immense the *vis à tergo* impelling it onwards) had gone right out, over hill and dale, into the Sound of Jura." And, in his view, nothing short of ice "filling the valleys up to the brim, and covering the whole country in one great winding-sheet, will meet the requirements of the case." All this notwithstanding, when the Reporter visits Loch Swin, on the western side of Knapdale, he still thinks that the striæ and boulders indicate a movement inwards from N.W. (*7th Report*, vol. xi.)

(3) Coming to Oban and its neighbourhood, we find the clearest evidence that the channel of Loch Etive was once filled by a great mass of land ice moving westward out to sea. The *Report* admits that the rocky knolls along Loch Etive are all smoothed from the east, "indicating apparently the action of a glacier which had moved down the valley now occupied by the loch." (*7th Report*, vol. xi.) It is to be borne in mind that the whole of the upper part of this valley, from Bonawe to the head of Glen Etive, is formed of granite of several varieties, grey and reddish. It is perfectly certain that the glacier which once occupied the valley must have borne thousands of granite boulders, and deposited them in various positions near its termination and along its sides. Yet, in discussing the origin of such boulders around Oban, this seems not to be taken into consideration at all, but attention is constantly turned away to N. and N.W.

For example, the boulders in the little valley on the south side of Oban Bay probably came "from some north-westerly point." (*7th Report*, vol. xi.) The large granite boulder at the north end of the bay, known as the "Dunolly boulder," has a position close to the cliff, which raises a presumption that it came from W. or N.W. (*4th Report*, vol. ix.) Boulders on north end of Kerrera "probably came from same quarter." (*Ib.*) Boulders in the Pass of Brander are probably from the north, or "floated by ice flowing from N.N.W." (*Ib.*) A large granite boulder on the hills between Oban and Connal Ferry suggests a look towards Ben Cruachan on the east; but "there seems to be nothing improbable in the supposition that it may have been brought by floating ice from the north." (*7th Report*, vol. xi.) At Lailt, about $6\frac{1}{2}$ miles from Oban, there is a perched boulder of considerable size. Judging from its

position, "the presumption is that it came from S.E., *i.e.* down the valley leading up to Loch Awe. *But it may have come from the N.W.*, as in that direction there is a valley by which it could have floated to its present position." (5th Report, vol. x.) Near it are fragments of another boulder, whose position also "suggested transport from S. or S.W. [S.E. ?], though the N.W. was not impossible." (*Ib.* p. 154.) Instead of carefully observing the facts of nature, and impartially recording what they seem to teach, does not this look like trying to bend these facts into conformity with a preconceived notion or theory?

(4) We pass on to what is said regarding the *Great Glen*, or line of the Caledonian Canal. The facts collected regarding the transport of boulders at the northern or Inverness end of it—in the work of collecting which our esteemed vice-president, Mr. Jolly, had a large share—leave no doubt that the general direction of movement in that quarter was from S.W. to N.E., *up* the line of the Great Glen, and out to sea by Forres and Elgin. Of course this is different from the standard normal direction—according to these Reports—from N.W. It is admitted that "the boulders whose parent rocks are situated on the hills forming the sides of the Great Glen have moved, not E.S.E. (the normal direction), but E.N.E." Some modification of the theory must, therefore, be made to meet these facts. "There can be no doubt," it is added, "that when the sea stood at a height of, say, 2,000 feet above the present level, and with a general oceanic current from W.N.W., the current in *Glen-na-Albin* itself—*i.e.* between the walls of the valley—would be in the line of that valley, forming a deep fissure across the country running about N.E., and that the force of this abnormal current would be sufficient to carry boulders transported through it in a north-easterly direction till it united with the more general stream from W.N.W." (5th Report, vol. x.)

Thus, the number of "abnormal currents" increases; one from N.E. to S.W. in the district embraced by Loch Long, Loch Fyne, and Loch Awe, and another from S.W. to N.E. through the Great Glen. It may be questioned indeed whether, if the line of the Great Glen caused any change in the direction of the "general oceanic current from N.W.," it was not far more likely to be from N.E. to S.W.—always towards the warmer latitude—than in the reverse direction, turning back to the colder conditions of N.E. Apart from this, we may note that the latter supposed "abnormal

current" is quite inconsistent with the northern and north-western origin, which these *Reports* so resolutely insist upon with regard to the boulders on Kerrera and elsewhere around Oban; for how could boulders reach that neighbourhood from N. or N.W. across a powerful current—"sufficient to carry boulders, etc."—sweeping past it from the S.W.? It is also contradicted by the fact that in the southern half of the Great Glen the direction of boulder transport has been clearly to S. and S.W.* In fact, this change in the direction of transport along the line of the Great Glen—to N.E. at one end of it and S.W. at the other—is one of the leading facts subversive of the ocean current theory; for, of course, no such diverging ocean currents could originate in the centre of the glen.

"My idea is," we read in the 5th *Report* (vol. x.) "that whilst in the *higher* parts of the country the general traces of a W.N.W. current are everywhere distinct, in *low-lying* valleys the direction of the current would change in correspondence with the axis of these valleys."

May we venture to ask, how could this be? The supposed general oceanic current from N.W. (or "W.N.W.") would sweep across the country by all the cols or passes that were submerged, the higher ones allowing a less, and the lower ones a greater depth and volume of it to pass through them. When by the gradual elevation of the land, the higher ones got above water, the current at these points would simply cease; there could be no current where there was no continuous passage across; these valleys, so far as they continued to be submerged, would then become bays or inlets of "still water;" another current there, with a different direction, was impossible. Moreover, the "low-lying valleys," which would be longest under water and most continuously subjected to this wide-spread N.W. current, should be the most unlikely places to indicate currents in other directions; there, the traces of the general or prevailing ocean current should be most distinct, persistent, and uniform.

But we may note in passing, that, on the theory of the great deep-enveloping sheet of land ice pressing out on all sides from the high lands of the country, these changes of direction become quite intel-

* See Mr. C. Livingstone's "Notes on Boulders to West of Fort-William" (8th *Report*, vol. xi.), in which he shows that some of these boulders have come from the upper part of Glen Nevis, and others he believes were brought down the Great Glen from the neighbourhood of Glen Spean, etc.

ligible. During its maximum, or period of greatest prevalence, this ice-sheet would maintain a uniform course over obstacles, which, as it shrank, would suffice to turn it aside. At length, when it existed as a number of separate local glaciers, these would "correspond with the axis of the valleys" in which they moved—east, west, north, or south, as the case might be, which cannot be imagined of oceanic currents.

Again, though at the time of greatest submergence the current might flow across the higher cols and passes of the country, its main draught and volume would be across the more open low-lying valleys; and this would undoubtedly determine the course of the icebergs. An iceberg differs so little in specific gravity from water that it has a very great proportional draught—seven parts of its bulk being under water to one above it. Its course, therefore, is determined not so much by winds, or eddies, or side currents, as by the main, deep, arterial oceanic currents. In these it sits, and to these it clings. It is extremely unlikely, therefore, that, even during the time of greatest submergence, icebergs would find their way into little side-valleys or land-locked inlets, where the water could only have escaped by a high and narrow neck or pass, and where, consequently, the draught must have been quite inconsiderable; they must have been mainly restricted to the broad open channels and sounds of the country. Yet it is precisely in these narrow valleys and land-locked inlets, and on the steep slopes of high mountains—in situations the most difficult to imagine that icebergs could ever have reached—that their supposed relics and evidences, in the shape of boulders, are most abundant!

But, not to dwell longer on such points, we submit further that—as appears to us—this theory of floating ice or icebergs, laden with fragments of rock, and borne across the submerged land by a general oceanic current from N.W., *will not account for the facts* which these *Reports* have recorded.

(a) With regard to the boulders themselves, looking only at those of the central valley in the meantime, the principal fact, of which an immense number of instances are given, is that boulders, more or less worn and rounded, from the mountains in the west are strewn over the country to the east; and these far-travelled boulders are mixed with pieces of local rocks—*i.e.* from nearer sources, but still from localities to the west. It is needless to repeat instances, we refer to those already given at p. 238-9.

We know that similar facts are common in our own neighbourhood—boulders of the same kind of rocks as the mountains to N. and W. being mixed up plentifully with fragments of rocks from the immediate neighbourhood, sandstone, trap, shale, limestone, ironstone, in varying proportions, according to the locality.

Now, our contention is that the theory of a berg-laden ocean-current from N.W. will not account for these facts ; and is, indeed, directly at variance with them. Let us consider the matter for a little. An iceberg is a portion of a glacier or sheet of land-ice which has broken off and floated away after the glacier or ice-sheet reached the sea. All the pieces of rock, large or small, which it bears on its surface, or more deeply embedded in its mass, fell upon it, or were enclosed by it, and borne away during its slow and gradual progress over the land. From the moment it reaches the sea, it gains nothing more, adds no more to its rocky burdens ; on the contrary, it immediately begins to lose, dropping these burdens over the sea-bottom at less or greater distances from their parent beds. Itself in a state of gradual dissolution, its freight also is always becoming smaller and smaller. It is more than likely that it loses the greater part of the rocky fragments attached to it, or embedded in it, at an early stage of its progress ; those fixed in its under surface, as it melts away, soon drop off ; those on the top also speedily slide off and disappear ; so that nearly all the icebergs that are recorded to have been seen at sea, have been remarkable for the absence of rocky fragments of any kind. Indeed, there can be little doubt that the ice-sheet parts with a large portion of its stony freight *before* any part of it breaks off to form a berg ; as it protrudes from the land out to sea, it must be constantly dropping the pebbles and boulders with which it is charged. (See Geikie's "Text-Book.") So long as it floats, it is plain that the iceberg cannot pick up any fresh fragments of rock ; what reason is there for supposing it can do so when it touches the ground ? When it grounds on the sea-bottom, on a bank or some sloping ridge of submerged land, is it conceivable that it can, so to speak, recoup its losses by laying hold of and carrying away masses of rock from the land on which it has touched ? It is surely unnecessary to point out that ice as it exists in an iceberg is hard and brittle, not a yielding or plastic mass into which substances can be dented by pressure, and to which they may thus be made to adhere

for a time. The shock of its stranding on the ground would tend to shatter it more or less, and to loosen from it any stones it was carrying, also to shatter any stones on which it struck ; but could not, in the least appreciable degree, enable it to pick up or attach fragments of rock to itself, far less so to incorporate them into its mass that it could carry them onwards to spots many miles distant from where it came into contact with them. The idea of icebergs thus acting as accommodating carriers and deliverers, not only laying down but taking up anew as they go along, really seems, the more we think of it, to be a wonderful hallucination. We may accept it as perfectly certain that, whatever icebergs may drop or leave behind them in their progress, they can take up nothing by the way ; their "bill of lading," so to speak, was made up and closed before their voyage began.

Accordingly, if these icebergs, which are supposed to have produced the effects under consideration, took their rise, as is alleged, in some distant tract of land to N.W., from which even the boulders on the West Coast of Scotland and on the Hebrides have been derived, it follows that none of these boulders found on the eastern parts of the country can really have been brought from the neighbourhood of Linlithgow, Doune, Callander, Kilpatrick, Inveraray, or Bonawe, as has been imagined ; they must all have come from that distant tract where the ice was formed. Their resemblance to the rocks around Callander, Kilpatrick, Inveraray, etc., is a mere coincidence ; there is not the least likelihood that (*by such means*) they ever came from these points. Their occurrence, also, in a certain definite order in relation to these rocks of the country is purely accidental ; and it must be allowed to be wonderful that icebergs should have dropped fragments of rocks in a distinct order and succession, in a certain uniform position always, with regard to the rocks existing away down in the sea-bottom over which they were being carried !

We may confidently affirm, then, that this feature of the boulders—the mingling of nearer and more distant kinds which characterizes them—could never have been produced by icebergs. And this conclusion would be very much strengthened by a reference to the Boulder-clay, and its changes in colour and composition from point to point, in conformity with the subjacent rocks, but on this we do not at present enlarge.

(b) Further, not only the nature of the boulders and their rela-

tion to neighbouring rocks, but also the *positions* in which they are found, are directly opposed to this iceberg hypothesis. These *Reports* frequently call attention to the fact, which is worthy of very close attention and consideration, that "another common feature presented by boulders is that when they are longer than they are broad, the longer axis is parallel with the direction in which the boulder had been transported. Very frequently, also, when one end is sharp and the other end broad, the former points towards the direction from which the boulder has come." (4th *Report*, vol. ix., p. 705.)

This last particular, however, as we shall see, is very open to question.

All through these nine *Reports*, the boulders are described with reference to these characteristics—"striae parallel to longer axis," and "sharpest end pointing" in such a direction. This fact regarding the position of boulders—so far at least as regards the correspondence of their longer axis with the direction in which they had been moved—may be taken as very generally established. But, accepting the fact, we entirely differ from these *Reports* in the conclusion drawn from it. "On the theory of icebergs or floating ice," it is said, "this feature is intelligible; on any glacier or ice-sheet theory it is not." (4th *Report*, vol. ix., p. 705.) We should say exactly the reverse; on the glacier or ice-sheet theory it is intelligible; on any theory of icebergs or floating ice it is not.

Can any one show that it is in the least degree likely that the stones dropped from, or left stranded by, icebergs, would correspond in the position of their longer axis with the direction in which the bergs were going? Manifestly, the stones would be dropped or left stranded in *all* positions; and it would be by the merest chance if one in a hundred corresponded with the direction of the berg's course. The berg itself may have changed its shape, its longer axis, even the side on which it floats, more than once during its voyage. What shadow of a probability is there that the stones it may deposit will have any uniformity of direction? None whatever.

This is so obvious that we begin to think the theory maintained in these *Reports* must be that the position of the boulders, as regards their longer axis, etc., is due to the subsequent action of the ocean currents. In the 2nd *Report* we read—"It was a remark of Hugh

Miller's that, on examining pebbles lying in the channel of a river, the great majority have their narrow ends pointing up stream. When once in that position, they retain the position longer than when lying broadside against the stream." (Vol. viii., p. 139.) Now, evidently this observation proves nothing with regard to boulders deposited by icebergs. The position of pebbles in a stream has really nothing to do with the matter—unless it be implied that these ice-borne boulders, many of them hundreds of tons in weight, and poised in precarious situations, had a certain uniform position imparted to them afterwards by the currents of the sea! This, we think, might be dismissed without further comment. But, first, we have grave doubts as to the accuracy of this observation, even though it be attributed to Hugh Miller. Masses of rock *in situ*, or that the stream could not move at all, may have their up-stream faces rounded or narrowed to some extent by the action of the current and the abrasion of sand and gravel, but that stones whose position *has been* affected by the current, could be arranged with their narrower and lighter ends pointing up-stream, seems impossible. We appeal to observation. In any movable body in a stream, if both ends be alike, will it not be rolled over and over, and if one end be sharper and lighter than the other, will not that end be wheeled round and point down stream? Then, secondly, we believe there is a mistake in the citation of the above passage. The reference is not given, and though we used to know Hugh Miller's writings tolerably well, we fail to remember in what part of them it occurs. We can, however, cite a passage from Hugh Miller of quite a different purport, and leave the reader to judge of the soundness of the reasoning, and its applicability to the point in hand.

In his well-known "Sketch-Book of Popular Geology," Miller refers to the fact of the pebbles and boulders of the Boulder-clay being scratched and grooved in the line of their longer axis, and argues that "the agent which produced such effects could not have been simply water, whether impelled by currents or in waves," for the smaller stones must have been *fastened* before they could have been scratched. "Even," he continues, "if the force of water could have scratched and furrowed them, it would not have scratched and furrowed them longitudinally, but across. Stones when carried adown a stream by the torrent, or propelled upward along a beach by the waves, present always their broader and

longer surfaces ; and the broader and longer these surfaces are, the further are the stones propelled. They are not *launched* forwards, as a sailor would say, *end on*, but *tumbled* forwards *broadside*. They come rolling down a river in flood, or upwards on the shore in a time of tempest, as a hogshead rolls down a declivity. In the boulder-clay, on the contrary, most of the pebbles that bear the mark of their transport at all were not *rolled*, but *slidden* forward in the line of their longer axis. They were launched, as ships are launched, in the line of least resistance, or as an arrow or javelin is sent on its course through the air. Water could not have been the agent here, nor yet an eruption of mud propelled along the surface by some wave of translation." (Pp. 31-2.)

So far Hugh Miller had got fully thirty years ago. This passage proves that in his opinion water, "whether impelled by currents or in waves," could not arrange these stones or boulders longitudinally, or give them those markings which show that they retained their longitudinal position even when rubbed and pressed against obstacles which they encountered in their way.

This very general uniformity, then, between the longer axis of the boulders and the directions in which these boulders have come—accepting it as a fact—we find (1) is quite inconsistent with the theory of the boulders having been dropped by icebergs ; and (2) could not have been imparted to them afterwards by ocean currents.

Accordingly, all the instances of uniformity or coincidence in this respect which have been brought together in these *Reports*, tell powerfully against the theory which these Reports adopt and defend. The same remark applies to the almost universal striation of the boulders in the line of their longer axis.

So also in regard to the instances which have been adduced of boulders having their "narrow ends" pointing backward in the direction *from* which they have come ; we are convinced that in at least some of these cases there has been—quite unintentionally of course—a mistake in recording the fact. So far as our own observation goes, nearly all the larger boulders, and those whose position we may suppose to be unaltered since they were laid down, have their narrow ends pointing forward in the direction *towards* which they were being transported. This is plainly, as Hugh Miller said, "the line of least resistance." We might instance the Inverbeg boulder on the western side of Loch Lomond,

north of Luss (see *Transactions*, vol. vii., p. 174), the large boulder on the hillside above Arden, the "Jim Crow" boulder near Kirn (which the Reporter is certainly in error in stating has its narrow end pointing up the loch), the "black boulder" near Ashton, Gourrock, and many others. So that the boulders alluded to as found on the western side of Knapdale, with their narrow ends pointing seaward, seem to us to prove exactly the reverse of what is concluded from them—not that they came in *from*, but that they were being moved out *towards* the western ocean. Take, for example, the "Dana boulder," a pear-shaped mass of from 70 to 80 tons weight, which rests on the shore of the island of Dana, on the northern side of Loch Sweyn. Here is a copy of the figure given of it in the Boulder Committee's *Report* (vol. xii., p. 781).

The supposition put forward in the *Report* regarding it is that it came in from the W. or S.W., and was progressing towards N.E. in that position! What natural agency could possibly incur such a waste of force as to move this mass *blunt end foremost*? Besides, what could be taking it to the north-east, in the face of a great "current" from that direction sweeping down Loch Fyne, as already admitted?

There is another point to which we wish to allude before closing. This is the frequent occurrence of boulders on the detached summits and steep slopes of hills, or on narrow ridges and knolls of rock, from which it would almost seem they could easily be dislodged—

"As if an infant's touch could urge
Their headlong passage down the verge."

Numerous such cases are described in the *Reports*, and some explanation is offered of their having been left in such apparently

precarious positions. In one instance, near Oban, the Reporter says "the sides of the valley where these boulders lie are at present exceedingly steep, and it seemed marvellous that if they fell on the surface where they now lie they did not roll to the bottom." (7th *Report*, vol. xi.) In another place, he alludes to the difficulty of believing that the boulder *fell* into such a position from the end of a glacier. We may remark, however, that it need not have "fallen" at all—in fact, its being in such a position is good proof that it did not fall. It may simply have taken the ground while the glacier was shrinking, and then the gradually-diminishing ice-sheet being unable to move it further, it was left as it grounded. This seems to account better for the boulders being left on such narrow projecting points, than the stranding of icebergs, with any rocks they bore, in all possible positions. For in the first place, many of these knolls and summits are in such situations that it is impossible to see how an iceberg, even under the most skilful navigation, could get to them—one near Tarbert, on Loch Fyne, for example, being on a detached eminence quite surrounded by hills. (Vol. xii., p. 775.) And then, secondly, supposing the iceberg did get there, it is difficult to believe that such an unstable mass could leave any part of its rocky burden, if it had any, so exactly poised in that position. So great is this difficulty, indeed, that the Convener, without abandoning his main theory, propounds a subsidiary one to get over it. He suggests that "probably when these boulders arrived, the valley was filled with gravel, and that as the gravel was scoured out by streams, the boulders slowly subsided to their present position." (Vol. xi., p. 249.) In a subsequent *Report* this is explained more fully, and illustrated by a wood-cut, which we shall reproduce.

We read—"It is also a puzzle to conceive what kind of agent it was which could set these boulders upon the steep slope of a hill so gently and exactly as enabled the boulders to occupy positions of stability. Having been brought from a distance on a glacier or on floating ice, a boulder must in either case [?] have fallen with such violence as, by the reaction of concussion, to have slipped down any slope, even though far from steep. The only rational way, as it appears, of accounting for the position of these boulders is to suppose that the valleys became filled with detritus when the land was submerged, and that floating ice dropped boulders on the detritus. If the boulders were dropped near the

side of a valley, the boulders, as the detritus was removed by rain and mountain streams, would slowly and gradually sink till they reached the slope of the hill, and would then find a resting-place. This view is indicated by the following diagram, where A represents a boulder in detritus, and B the same boulder when the detritus was nearly all removed from the valley." (*Ib.*, p. 783.)

Now, with all respect, we should say that, granting all the conditions required, this is a most unlikely thing to happen. A boulder protects in some degree that on which it rests; the detritus is gradually worn away on all sides of it by the denuding agencies, leaving it supported on a knoll or pillar of the detritus, like the earth-pillars at Botzen in the Tyrol, each with its stone a-top, as described and figured by Sir Charles Lyell. (*Principles*, vol. i., p. 337.) At length the pillar wearing away however slowly, becomes too slender to support its burden, and the stone falls, in a manner most unlikely to leave it nicely-poised on any narrow ledge underneath. Accordingly we find it stated by Sir Charles that "many of the fallen blocks, once the cappings of pillars which have disappeared, are now seen in the bed of the torrent." (*Ib.*, p. 338.) This is the natural history of a boulder resting on detritus; and, indeed, it is illustrated by an instance, given in the 6th *Report*, of a large boulder in the Inveraray district, north of Loch Fyne. "It was resting," says the writer, "on a knoll of gravel, at some distance from any hills. At first I was puzzled to account for its position being so exactly on the apex of the gravel knoll. It struck me eventually that its great size and weight had been the means of protecting by covering the knoll on which it originally had been dropped. The denuding agencies which could loosen and sweep away the gravel and sand in the surrounding parts of the valley, probably did not move the boulder, and so would leave it in its original position, or nearly so." (Vol. x., p. 581.) Exactly; but how is it to get down at length, gently and easily, to its

narrow ledge of rock? Again, as regards the conditions—not to enlarge on the enormous quantities of detritus required by the theory now under consideration, and the question whence and by what means these were accumulated (for mere submergence does not *create* detritus)—a fatal objection to the theory is that multitudes of the boulders whose position is sought to be explained are not in valleys at all, but on isolated hill-tops, on the shoulders of high mountains, and on steep slopes overlooking wide moorlands, or the sea; so that from every point of view the explanation is incredible, and starts greater difficulties than it was intended to remove.

We had some observations to offer on the N.W. current, and the submergence of the land, so frequently alluded to in these *Reports*, but must reserve these till another opportunity.

NO. XXII.—ON SOME BOULDERS NEAR ARDEN, LOCHLOMOND.
By DUGALD BELL. WITH A PLATE [No. V.].

[Read 12th January, 1888.]

THOUGH the Boulder Committee of the Royal Society of Edinburgh now rest from their labours, there are doubtless, in various parts of the country, many boulders which, notwithstanding all the care taken, that Committee have not included in their valuable *Reports*. One of these, a large and prominent mass of mica-schist on the hill above Inverbeg, near Luss, was brought under the notice of the Society a few years ago. (See *Transactions*, vol. vii., p. 172.) I wish now to call attention to one or two others, also on the western side of Lochlomond, but farther south, viz., on the hill known as *Gowk Hill* (called by the Ordnance Survey, *Cock Hill*), behind Arden.

At this point there is, as the Convener states in one of the *Reports*, “a low valley which runs up from the loch in a westerly direction. The summit level of the valley is about 150 feet above the sea. Along the south side of the valley,” he continues, “a number of boulders, chiefly of primitive rocks, have been deposited. They are at a height of about 94 feet above the sea. As usual, the most frequent position is here, as elsewhere, N.W. and S.E. for the longer axis, and the sharpest end towards the west.” (4th *Report*, vol. ix.) This last statement, we are convinced, is a