

TRANSACTIONS
OF THE
EDINBURGH GEOLOGICAL SOCIETY.
SESSION 1873-74.

Thursday, 6th November 1873.

The Society held its Fortieth Anniversary Meeting in its rooms, No. 5 St Andrew Square, ARCHIBALD GEIKIE, Esq., LL.D., F.R.S., F.G.S., Professor of Geology in the University of Edinburgh, and Director of the Geological Survey of Scotland, the *President of the Society*, in the Chair. Professor GEIKIE delivered the following

INTRODUCTORY ADDRESS.

Among the questions which may be treated as matters of strict science, and which yet cannot be wholly divested of the strong human, one might almost say personal, interest which belongs to them, is the birth of mountains and valleys. The familiar outlines of his dwelling-place have fixed the attention of man from the infancy of the race up to the present day. Long before science arose to deal with them they had become inwoven with his history, his habits, and his creed. The great mountains had been to him emblems of majesty and eternity, lifting up their fronts to heaven as they had done from the beginning, and would no doubt do to the end. They rose before him as monuments of the power of that great Being who had heaved them out of chaos. It was enough for him in that early time to feel their mighty influences; he had then no questions or doubts as to how or when they first appeared upon the earth.

Happily, in spite of questioning, exacting science, these first natural and instinctive feelings are not yet dead within us. A knowledge even of all the laws of mountain-making cannot, if our minds are healthy and our hearts beat true, deprive us wholly of that first genuine child-like awe and wonder in pre-

sence of noble mountains. Crag and cliff sweeping in rugged and colossal massiveness above dark waves of pine far into the keen and clear blue air; the vast mantle of snow, so cloud like in its brightness, yet thrown in many a solid fold over crest and shoulder; the dark spires and splintered peaks, half snow, half stone, rising into the sky like very pillars of heaven; and then the verdure of the valleys below, the dash of waterfalls, the plenteous gush of springs, the laugh and dance of brook and river, as they one and all hurry down to the plains,—who can see these things for the first time, nay, for the hundredth time, without at least some sparkle of the simple, childlike emotion of the olden time, or without appreciating, even if he cannot fully share, the feeling of the poet to whom they brought “dim eyes suffused with tears?”

These great dominant features of the land must, indeed, ever rivet our imagination, and yet when the questioning spirit of modern science asks to know how they came into being, we are no longer permitted to content ourselves with the early belief that they were but parts of the primeval outlines of the earth. The progress of inquiry and knowledge has destroyed that belief. We find, too, that both labour and patience are needed ere we can understand what has been put in its place. But the toil of learning this is well repaid. However grandly the mountains rose when they were gazed at only in awe and wonder, they gain an added sublimity when the eyes which look upon them can trace some of the steps whereby their grim magnificence has been achieved.

We naturally associate the more lofty and rugged parts of the land with the operations of former earthquakes and convulsions, by which the solid earth has been broken and ridged into these picturesque forms. This obvious inference was early adopted in geology, and though in many cases a mere belief rather than a legitimate deduction from observations, and springing from a conviction of what ought to be rather than what has been proved to be the case, it has sturdily maintained its hold alike on the popular mind, and also, to a very considerable extent, as an article in the orthodox geological creed.

Towards the end of last century, however, Hutton and Playfair, names never to be mentioned in Edinburgh without gratitude and pride, proclaimed views of a very different character. They maintained that the rocks of the land, originally accumulated under the sea, have been upheaved by underground movements, and without pretending to know in what external forms these rocks first appeared above the sea, they contended that the present contours of the land had arisen in great measure from a process of sculpture, the valleys having been carved out by rains, streams, and other superficial agents, while the hills were left standing up as ridges between. So satisfied were these bold

and clear-sighted men that their idea was essentially true, that they gave themselves no concern in gathering detailed proofs in its support. They were content with general appeals to the face of nature everywhere as their best and irrefragable witness. But, as events proved, they were in advance of their time.* The views which they promulgated on this subject were first opposed, then laid aside and forgotten. In the subsequent literature of the science for fully half a century they almost wholly disappear. An occasional reference to them may be met with, where, however, they are perhaps cited only to be dismissed, as if the writer seemed hardly able to restrain some expression of his wonder that men could ever have been found so Quixotic as to vent such notions, or that others could have been so gullable as to believe them.

Apart altogether from the truth or error of the Huttonian teaching regarding the origin of the earth's superficial features, no one who has the progress of geology at heart can regard without regret this almost contemptuous dismissal of the question from the range of scientific inquiry. For together with that teaching went all interest in, and even all intelligent appreciation of the problem which Hutton had set himself to solve. Men turned back to vague notions about cataclysms, earthquakes, subterranean convulsions, and fractures, with which they dealt, and sometimes still deal, with a boldness in inverse proportion to their knowledge of the actual conditions of the problem. They studied with praiseworthy assiduity and success the working of the various natural agents whereby the surface of the land is affected, but it was with the view rather of showing how the materials of new continents are gathered together than of learning how the outlines of existing continents have been produced. The study of the origin of mountain and valley went out of fashion, and from the time of Playfair's illustrations, published at the beginning of this century,† received in this country but scant and haphazard attention, until in recent years the subject has gradually revived, and has become one of the most prominent and interesting subjects of geological research.

It is not my purpose to give any historical sketch of the progress of inquiry on this question, although I ought not even to refer to it without an allusion to the names of Scrope, Ramsay, Jukes, Ruskin, Dana, Topley, Foster, Whitaker, Greenwood, the Duke of Argyll, Mackintosh, and others, who, though often differing widely in their views, have done so much to renew an interest in what will probably always prove one of the most alluring

* See in particular Hutton's original paper (1785) published in the first volume of the "Transactions of the Royal Society of Edinburgh," and afterwards expanded by him into the "Theory of the Earth," of which only two volumes were (1795) published.

† Illustrations of the Huttonian Theory. Edinb. 1802.

aspects of geology. Thoroughly convinced of the essential truth on which the Huttonian doctrines were based, I wish, on the present occasion, first to define and illustrate some of the leading features of these doctrines as I hold them myself, and as I believe them to be held by the great body of active field geologists in Britain; and, secondly, to review certain objections which have recently been reiterated against them.

At the outset it is necessary to ascertain what relation the internal arrangements of the rocks bear to the external forms of the land, in other words, the influence of what is called geological structure. It is obvious, as Hutton showed, that since the rocks have been formed as a whole under the sea, they must have been raised out of that original position into land, so that the first point we settle beyond dispute is that the mass of the land owes its existence to upheaval from below. But though we fix securely enough this starting-point in our inquiry, it by no means follows that we thereby settle what was the original outline of the land so upheaved. The non-recognition of this fact has involved not a few of the writers on this subject in great confusion and error.

Among the geologists of the present day there is a growing conviction, that upheaval and subsidence are concomitant phenomena, and that viewed broadly, they both arise from the effects of the secular cooling and consequent contraction of the mass of the earth. The contraction has not been uniform, as if the globe had been a cooling ball of solid iron. On the contrary, owing to very great differences in the nature and condition of the various parts of our planet, and perhaps to features of the interior with which we are yet but imperfectly acquainted, some portions have sunk much more than others. These having to accommodate themselves into smaller dimensions, would undergo vast compression, and exert an enormous pressure on the more stable tracts which bounded them. It could not but happen, that after long intervals of strain, some portions of the squeezed crust would at length find relief from this pressure by rising along some line of weakness to a greater or less height according to their extent and the amount of force from which they sought to escape. These upraised areas would no doubt tend to occur in bands or lines across the direction of the pressure, much as the folds we produce in the sheets of an unbound book are more or less nearly parallel with the two sides from which we squeeze the paper. They would sometimes be broad folds—huge wide swellings of the earth's surface. At other times they might be long, lofty, and comparatively sharp ridges. In the one case they would give rise to high plateaux or table-lands; in the other they would be recognised as mountain chains.

This is a rough untechnical statement of what seems the probable explanation of the origin of the elevated tracts upon the

earth's surface. It is evident that the pressure would be vastly greater a few hundreds or thousands of feet underground than at the surface, and hence, that though the rocks deep down might be squeezed and crumpled, as we could crumple brown paper, yet that at the surface they might show little or no contortion. Certainly, without further proof, we could never affirm that a contorted mass of rock, which now forms the surface of the ground, rose as part of the surface during the time of upheaval and contortion. Intensely crumpled rocks would rather suggest a deeper position, with the subsequent removal of the rocks under which they originally lay.

As the earth has been cooling and contracting ever since it had a separate existence as a planet, its surface must have been exposed to a long series of such shrinkage movements as those we are considering. Apart, therefore, from local evidence, we should expect that ridges and depressions must have been impressed upon that surface in a long succession from the earliest periods downwards, and hence that the present mountain-chains and basins of the earth must be of many different ages. We cannot tell what the first mountains were made of, nor where they lay, although some of the existing ridges of the earth's surface are undoubtedly, even in a geological sense, very old. In not a few cases the same mountain-chain can be shown, from its internal structure, to be of many successive dates, as if it lay along a line of weakness which had served again and again as a line of relief from the severe earth-pressure.

These questions have been treated with much ability by Constant Prevost,* Dana,† Mallet,‡ and others, to whose writings I refer for details. In stating them in this general way, my object is to show that those geologists who, like myself, believe in the truth of the Huttonian doctrines of denudation, are most unfairly represented when they are said to ignore the influence of subterranean forces upon the exterior of the earth. None can recognise more clearly than they do how entirely have the great surface outlines of the globe been dependent upon the action of these forces, that is, upon the results which flow from the contraction of the planet, and from the reaction of the heated interior upon the surface.

But a block of marble is not a statue. Nor would a part of the earth's crust heaved up into land form at once such a surface

* Constant Prevost. *Théorie des Soulèvements*, &c.; *Bulletin de la Société Géologique de France*, xi. (1839-40) p. 183. *Comptes Rendus*, vol. xxxi. part ii. (1850).

† J. D. Dana. *On the Origin of Continents*, &c.; *American Journal of Science and Art*, 2d ser. ii. iii. and iv., 1846-47. A further statement and development of the author's views may be found in a series of admirable and suggestive papers reprinted in the *Phil. Mag.* for July-November of the present year (1873).

‡ Mallet. Introduction to English edition of Palmieri's Report on the Vesuvian Eruption of 1872. *On Volcanic Energy*, *Phil. Trans.* for 1873, p. 147. See also a paper by the Rev. O. Fisher, *Cambridge Trans.* vol. xi. p. iii. (1868).

of ridge and valley, and nicely-adjusted water system, as any country of which we know anything on the face of the globe. In each case it is a process of sculpture; and the result varies not only with the tools, but with the materials on which they are used. You would not expect the same kind of carving upon granite as upon marble. And so, too, in the great process of earth-sculpture each chief class of rock has its own characteristic style. The tools by which this great work has been done are of the simplest and most every-day order—the air, rain, frost, springs, brooks, rivers, glaciers, icebergs, and the sea. These tools have been at work from the earliest times of which any geological record has been preserved. Indeed, it is out of the accumulated chips and dust which they have made, afterwards hardened into solid rock and upheaved, that the very framework of our continents has been formed. The thickness of these consolidated materials is to be measured not by feet merely, but by miles. If the removed materials are so thick, they show what a vast mass of rock must have been carried away. And even before knowing anything of the way in which the various tools are used, we should be justified in holding it to be at the least extremely improbable that any land surface would long retain its original contour, or even any trace of it.

But when we come to watch with attention how the tools really do their work, this improbability increases enormously. Adopting a method of inquiry suggested by Mr Croll, I have elsewhere shown that even at their present rate of progress the amount of geological change which they would accomplish in a comparatively small number of ages is almost incredible.* On a moderate computation they would reduce the general mass of the British Islands down to the level of the sea in five or six millions of years, and might carve out valleys a thousand feet deep in a fourth part of that time. It is evident that though the upheaval of some parts of the continents may go back into the remotest geological antiquity, the forming of the present surface must be, comparatively speaking, modern.

There is reason to believe that many, if not most, of the great mountain-chains of the globe are, in a geological sense, of recent origin. The Alps, for example, though they may have undergone many earlier movements, were ridged up into their existing mass long after the soft clays were laid down which cover so large an area of the low lands in the south of England, and on which London is built. It would require far more detailed work than has ever been bestowed upon these mountains to enable us even to approximate to what was the original form of the surface just after the upheaval, and before the array of sculpture-

* See an Essay on Modern Denudation, "Trans. Geol. Soc. Glasgow," vol. iii. p. 153. Reprinted in the 3d edition of "Jukes' Student's Manual of Geology," chap. xxv.

tools began their busy and ceaseless task upon these great masses of rock. We may believe that a series of huge parallel folds of curved and broken rock rose for thousands of feet into the air, that when, after the earth-throes had ceased, rain and snow and frost first laid their fingers on the new-born summits, these agents of destruction would have a most uneven surface to work upon, and would necessarily be guided by it in their working, and hence that some, at least, of the dominant earliest ridges and hollows would tend to be perpetuated. Such a belief would carry probability in its favour, but it would certainly not amount to a proof of the supposed perpetuation. That would require to be corroborated by the internal and external evidence of the mountains themselves. In some tracts, as, for instance, among the singularly symmetrical ridges and furrows of the Jura, it would not be difficult to restore the original outline, and to fix exactly how far the subterranean movements had determined the present external forms of the ground, though even there, where this connection is so clear, we should see at the same time how greatly the tops and sides of the long saddle-shaped arches of rock have suffered from subsequent waste. But among the contorted, inverted, and broken rocks of the Central Alps the task would be infinitely more difficult.

We could not advance far, however, in such a quest before observing that one feature stands out conspicuously enough among the mountains, viz., that whatever might have been their original outlines, these were most certainly not the same as those which we see to-day. No part of the history of the ground can be made more self-evident than that since the birth of these mountains, millions upon millions of cubic yards of rock have been worn off their crests and ridges, and carved out of their sides. There is not a cliff, crag, or valley along the whole chain of the Alps which does not bear witness to this great truth.

If, then, even when dealing with the young Alps, we cannot be quite sure what were their first or infant features, how impossible must it be to decide as to the early outlines of such immensely more ancient uplands as those which date from Palæozoic times? For evidently the higher their antiquity, and the longer, therefore, their exposure to ceaseless waste, the more must these outlines be changed. The general mass of land might still remain land, but trenched and furrowed and worn down, as the Alps are now suffering, until not a single vestige or indication of its first contour survived, the remaining portions being, as it were, merely the stump or base of what once was.

Now, this is the position in which the question presents itself in Britain. The hills of the Highlands and southern uplands of Scotland, of the Lake district, and of Wales, are not mountains in the same sense as the Alps or Pyrenees, or other great con-

tinental mountain chains. However much these latter long lines of elevated ground may have had their outlines modified by the universal waste of the earth's surface, their linear character, the general parallelism of their component ridges, the undulations of the strata along their flanks, as well as their internal geological structure, bear witness to the fact that they are but huge wrinkles upon the shrivelled globe—tracts which have been thrust up while the neighbouring regions have sunk down. But in Britain these characteristic features are wanting. In all probability there never was any true mountain-chain in our region. There is good reason to believe that in very ancient times, that is to say, previous to the Old Red Sandstone, a wide plateau-like mass of land was upraised on the north-west of Europe, surviving portions of it being represented by the detached hilly regions of Britain and the great table-land of Scandinavia. The rocks underlying this upheaved tract underwent at the time of elevation enormous compression and consequent contortion. This could not happen without an infinite amount of resistance. The heat thus evolved among the grinding masses may have been amply sufficient even to melt them in part. And no doubt it was during this process that they became crystalline over such wide areas, and were injected with granite and other melted products. But all this had been wholly, or almost wholly completed before the time of the Old Red Sandstone, for the deposits of that geological system are formed out of the older altered rocks, and lie undisturbed upon them. Even now, in spite of all the subsequent denudation, the patches of Old Red Conglomerate which remain show to what an extent the older rocks had been buried under it, for they are found rising here and there to a height of two thousand or three thousand feet above the sea. But they prove further, not only that the contortion of the underlying rocks preceded the Old Red Sandstone, but that these rocks had suffered a vast extent of waste at the surface before even the oldest visible parts of the conglomerate were deposited upon them.

This waste has been in progress ever since. We need not, therefore, hope to discover any vestige of the aboriginal surface. A geological section drawn across any part of the hills proves beyond question that the general surface of the country has had hundreds or even thousands of feet of solid rock worn away from it. Such a section shows, moreover, that our present valleys are not mere folds due to underground movements, but are really trenches out of which the solid rock has been carried away.

So far this is a question of simple fact, and not merely of opinion. The language of Hutton may be literally true of Britain:—"The mountains have been formed by the hollowing out of the valleys, and the valleys have been hollowed out by the attrition of hard materials coming from the mountains." Our

British hills, unlike the chains of the Jura and the Alps, are simply irregular ridges, depending for their shape and trend upon the direction taken by the separating valleys. The varying textures of the rocks, their arrangements with relation to each other, their foldings and fractures, and the other phenomena comprehended under what is termed "geological structure," have greatly modified this result, but the process has nevertheless, as I believe, been one of superficial sculpturing, and not of subterranean commotion and upheaval. On the details of this process it is not needful to dwell.

From these cursory statements, which express, I believe, the general concurrent opinion of the modern Huttonian school, it should be clear how far that school must be from ignoring the influence of subterranean forces. Hutton himself never did so, and his followers now know far more of these forces than he did. But, on the other hand, they claim for the surface agents in geology a potency great enough to cut down table-lands into mountain ridges and glens, to carve out the surface of the land into systems of valleys, and, in the end, to waste a continent down to the level of the sea.

You are aware that the revival of the half-forgotten doctrines of the early Scottish school of geology has not been without vehement protest on the part of the older geologists, who have been inclined to treat them rather as novelties and departures from the older and purer faith. No one resisted them more determinedly than my much-missed friend and benefactor, the late Sir Roderick Murchison. He looked with regret, and even perhaps sometimes with a little alarm, upon their advance, and to the last he battled against them. He was indeed in this country the leader of his party, which has been called the "Convulsionist School," and his death has doubtless been a severe blow to that school, as it has been a loss to all who admired a straightforward, courteous, and undaunted antagonist.

Other members of the party have, however, in more or less direct ways, lifted up their voices of protest. I select this evening one of these antagonists, partly because he has spoken more, and more energetically than any other, and partly because a good deal of his speaking has been directed against myself. And here I am sorry that I must begin by a reference to a matter of personal history. In the summer of 1865 I published a little volume on "The Scenery of Scotland viewed in connection with its Physical Geology." The object of that work was to show how completely the Huttonian doctrine of earth-sculpture was borne out by the mountains and valleys of the northern part of this island. I distinctly disclaimed any novelty or originality on my own part in the broad doctrine which I tried to enforce. My veneration for Hutton and Playfair had been from boyhood pro-

found; again and again in the pages of my book I quoted them, and spoke of them as the founders of the school to which I professed myself a loyal adherent, and in which I could boast such friends and colleagues as Jukes and Ramsay.

I was well aware, and stated in the preface that the views to which I had been led, "ran counter to what are still the prevailing impressions on this subject," and that I was prepared to find them disputed or thrown aside. Convinced, however, of their essential truth, I looked forward to a time when what might then be regarded as mere dreaming, would be established as a recognised part of the ground-work of geology. The views put forward in the volume, however, met with an amount of general acceptance which I could hardly have anticipated. But, at length, the expected opposition made its appearance.

On February the 5th, 1868, his Grace the Duke of Argyll read to the Geological Society of London, a paper entitled, "On the Physical Geography of Argyllshire in connection with its Geological Structure."* Although that title was chosen, the paper proved really to be from beginning to end a criticism of my little book, which indeed the author candidly acknowledged to have served him as "the best text he could find."

To that paper I made no reply. It seemed to me that the noble author had failed to perceive the bearing of the whole argument from geological waste as proved by geological structure. His objections being already, in my opinion, anticipated in the book which had called them forth, I did not see how I could make my case plainer by any amount of additional argument. But, further, his Grace had begun his communication with a sentence, in which he stated that the views set forth by me "seemed to be gaining ground with the younger school of geologists"—fatal admission as it occurred to me; for I felt that what was called the younger school must eventually take the place of the older, and that if it remained true to its belief, the views which were now called in question would carry the day without any battling of mine. Every month shows more fully the justice of this anticipation.

I was content to let the matter rest. Nor would I recur to it now, but for the following reasons. Since that time the Duke of Argyll has become president of the Geological Society of London. In his recent address,† and in a subsequent communication‡ to the Society, he has returned to the subject of the origin of the present features of the land, referring to his former paper

* Quart. Journ. Geol. Soc., xxiv. 255.

† Ibid., xxix. p. li.

‡ On Six Lake Basins in Argyllshire, *ibid.*, p. 508. This paper had not been published at the time when the present address was written, and the meagre account of it in the abstracts of the Society afforded no material for proper criticism and reply. I have referred to it in a note appended to this address.

as "an argument which had not been met by any answer in detail," and adhering therefore to the views there expressed. As to the non-appearance of any "answer in detail" from myself, I can give no other explanation than that I considered my little book sufficiently detailed for its purpose, and believed that it already anticipated and answered the objections of my opponent. That is still my belief.

But a broad challenge, addressed to the general body of geologists by the president in the official address which he annually gives to the Society and the world, is not the same thing as a criticism from one member of the Society upon the work of another member. In the interests of science, therefore, it seems to me that some protest is now called for against doctrines promulgated at this late date in the century from so high and honourable a position as the chair of the Geological Society of London; and as I have been especially singled out for attack, it appears to me to be only an act of duty to vindicate not my own position merely, but the reputation of that "younger school" which is accused of seeking to pervert the geological mind from the ancient and true creed. If those doctrines maintained by the president were to become generally diffused, which, happily, is now impossible, they would suffice, I believe, to paralyse research in one important branch of the science, for, as far as relates to the history of the configuration of the land, they would assuredly bring down upon us again the pre-Huttonian darkness.

No one whom the Geological Society of London has chosen as its president can fail to command the respectful attention of geologists all over the world. And while I gladly acknowledge this right, I would also express the gratification which is widespread among the brethren of the hammer in this country, that the Duke of Argyll, in the midst of so many and so onerous, as well as honourable duties, should find time to take a lively and active interest in the progress of geology. I admire, too, the vigour with which he wields his pen, and the boldness with which he gives his judgment among disputed questions. He has once more thrown down his geological gauntlet, and if I venture to take it up and accept his battle, it is in the full consciousness of the presence of an adversary who, while dealing hard blows himself, will take in good part such buffets as the fortune of war may bring to him.

I have already alluded to the natural impression, that when we look at a region of rough mountains formed out of hardened and contorted rocks, we behold in the external outlines the direct results of the subterranean force by which the rocks were altered and crumpled. This obvious inference is far older than the days of geological inquiry. But surely its mere obviousness is no argument for its truth, any more than the rising and setting

of the sun prove the earth to be the centre of the universe. In the volume already referred to I spoke of it as "dealing with that dreamland of conjecture and speculation lying far beyond the pathways of science, where one has no need of facts for either the foundation or superstructure of his theory. It thus requires no scientific knowledge or training: it can be appreciated by all, and may be applied to the history of a mountain chain by one to whom the very name of geology is unknown."* But to recognise that this common and instinctive notion is yet a misleading one requires an acquaintance with geological structure, which comparatively few have an opportunity of obtaining, and which appears to be not always readily acquired at second-hand. I have watched the current geological literature on this question during the last decennium, and the result has been to convince me that the notion, or rather prejudice, which I am combating is in some minds so deeply rooted, that it cannot be got rid of by the reading of any number of books or treatises, and, of course, still less by the writing of them. Simple as may be the statement of the leading principles and facts relative to that waste of the earth's surface to which the term denudation is applied, there is yet, I firmly believe, no part of geology more difficult adequately to realise. So striking are the difference and contrast between the magnitude of the results adduced, and the apparent insignificance and impotence of the forces which are alleged to have produced them, that the mind not unnaturally hesitates to associate the one with the other in the relation of cause and effect. And yet it is only in proportion as one is enabled to master this subject that he is prepared to understand, far less to discuss the origin of the present contours of the land.

In the volume which the Duke of Argyll has singled out to bear the brunt of his attack, I carefully stated at the commencement that I proposed to consider the problem only "in so far as it relates to the history of the scenery of Scotland."† I laid down no universal law or dogma by which the hills and valleys of every other part of the world were to be explained. I knew the mountains and glens of Scotland; I had wandered over them and studied them from boyhood; trained in the severe and laborious school of the Geological Survey, I had mapped many hundreds of square miles of their surface across some of the most complicated pieces of geological structure in the kingdom. It was not, therefore, in any spirit of rashness, or novelty, or dogmatism, but with the growing conviction of many years of experience, and in the belief that a service to the cause of geological inquiry in this country could be done, that I ventured to launch my little book upon the world. I was well aware that

* *Op. cit.* p. 4.

† *Op. cit.* p. 6.

other regions exhibited features not seen here, and that for these other explanations might require to be found. But it was then no part of my subject to travel beyond my own domain. When the principles for which, in common with my able colleagues in the Survey, I contended were firmly established in relation to the scenery of this country, it would then be time to consider how far they were applicable elsewhere. That they would be found to be not merely of local but of wide general import, I then held to be probable, and I now know to be profoundly true.

One main object of my chapters was to show how the present hills and valleys of Scotland had come into existence gradually one by one, during an enormously protracted period of geological waste, in the manner to which I have already referred this evening. I adduced copious proofs from all parts of the kingdom in support of this view, similar proofs having been already triumphantly accumulated by Mr Jukes in Ireland, and by Professor Ramsay and others in England.

Far from ignoring the influence of geological structure upon external form, I might even have been charged with having brought forward a needlessly ample accumulation of evidence to show how constantly the resulting contours of the country have been determined by the arrangement of the rocks. I showed how ancient, in a geological sense, the denudation of the country had been, and how thoroughly it had done its work upon the surface, no matter whether the rocks had been formed originally as mere soft mud or had been once in actual fusion. I dwelt on the remarkable fact, that as a rule the valleys do not run along lines of fracture, and quoted in support of this assertion the published maps of the Geological Survey of the three kingdoms. To these and similar statements of sober fact, which are now part of the common stock of geological knowledge, His Grace opposes such phrases as these—"the facts assumed are, in my opinion, to a large extent purely hypothetical"—"this assertion is erroneous"—"extravagant demands"—"inventions and imaginations"—and so on.*

I do not consider it necessary to defend my facts. They are familiar enough to the geologists of this country as displayed more or less plainly in every district of our island. I am at present concerned with the counter-statements which the Duke of Argyll would put in their place.

He states his belief that the Highland mountains have had their contour mainly given to them by "upheavals, subsidences, and lateral pressures, which have folded them and broken them into their present shapes." A belief of any kind must be founded on evidence of some sort, and that evidence must be produced

* "Quart. Journ. Geol. Soc.," xxiv. p. 255, *et seq.*

if the owner of the belief desires that it should be accepted by others besides himself. What evidence, then, does his Grace furnish as the basis on which he expects that his "belief" is to supersede what he is pleased to term "the extravagant theories of the younger glacialists?" Having shown the "antecedent improbabilities involved in the extreme theories of erosion," he states that he "proceeds to test them on the field of fact." We follow him anxiously to the field in question, and find that his so-called facts are stated in such words as these—"Loch Fyne . . . occupies, as I believe, the bed (*sic*) of an immense fault." "The transverse valley of Loch Eck lies across a steep anticlinal, and is due, in my opinion, to the extreme tension to which the crystalline rocks have been subjected." "The Pass of Awe is a rupture and chasm." These, and other similar assertions regarding various parts of the Highlands, are confidently expressed, but they are accompanied by no evidence by which their accuracy may be tested. In truth, the "facts" which His Grace adduces in support of his "belief," are only other "beliefs" and "opinions" of his own. They may be correct or the reverse, but they cannot legitimately be adduced as evidence in a scientific argument.

But they are very far from correct. I utterly deny, for example, the assertion that Loch Fyne lies along the bed of an immense fault, and I ask the Duke of Argyll to try to prove that it does so. Nay more, I challenge him to produce a geological section which would bear a moment's examination on the ground, in which he can show the coincidence of a valley with a line of fault in any part of his own county of Argyll. That cases of this coincidence may be found I do not doubt, but the search for them will be useful in teaching His Grace how exceptional they are.

The Duke of Argyll does indeed offer some explanatory statements regarding some of his assertions of fact. For instance, with regard to Loch Awe, he dwells on the inclinations of the slates and the intrusion of the porphyries among them, as evidence that the present contour has been directly the result of subterranean convulsion, and he triumphantly adduces these and similar appearances "ignored" by myself as a demonstration of the truth of his "belief." But any one who knows the Highland rocks at all may well smile when he is told that a geologist who had ever been over the ground, even in the most cursory way, requires to have these phenomena pointed out to him. In reality I had already granted the existence of these and far more wonderful evidence of underground movements, for I knew the Highland rocks well, and had mapped their structure over leagues of ground from the mountains of Sutherland to the moors of Forfar and the headlands of Islay. I was therefore perfectly familiar with the phenomena to which the Duke of Argyll so confidently refers.

But I had learned more about them than merely their tale of subterranean turmoil. I had found that they did not bear directly on the origin of hill and valley at all. I had traced everywhere evidence that what we now see of intruded granite or curved slate has been laid bare, only after the removal of hundreds and thousands of feet of rock under which it once lay. His Grace, it would seem, has still this lesson to learn, and until he has mastered it, and, apart from any theory, but simply as a matter of demonstrable fact, has realised what it involves, and how vain is the attempt to connect the contorting and hardening of the rocks with the *present* surface features of the country—argument with him on this question seems hardly possible.

Again, I had quoted the mountain Ben Lawers, with its flanking hollow in Loch Tay, as a typical example of the kind of evidence which could be abundantly adduced from all parts of the Highlands to show the relation between geological structure and external form, and to prove from under what an enormous mass of removed rock the present surface of the Highlands has appeared. I gave a section to show at a glance the broad facts of the case—a section from which no conclusion is possible but that which I drew. But here once more the Duke of Argyll's belief in the all-powerful efficacy of granite and igneous rocks, or his thralldom to what he calls "the influence of a preconceived theory," brings out in well-marked prominence that obliquity of vision which prevents him from seeing anything but convulsion and fracture. He scents intrusive rocks of some sort along the south bank of Loch Tay. It would be vain to remonstrate that this alleged influence of the igneous rocks is, to say the least, as pure "invention and imagination" as anything which the "younger school" could readily supply, or that the denudation of that region is a momentous fact to be looked in the face and explained, not to be dismissed or denied, no matter what our "theory" or "belief" may be regarding the origin of granite. Without further ceremony, the proofs of enormous denudation at Ben Lawers and Loch Tay, together with their luckless advocate, are all bundled off with the summary judgment, so happily appropriate to its own author, "I attach no value whatever to a theory which passes over and ignores this class of facts altogether."

The dogmatic assertions which the Duke of Argyll makes regarding the influence of granite and other rocks upon the surface, and as to the existence of fractures and depressions along the line of valley and glen, are really most flagrant examples of the *petitio principii*. In effect, his Grace tells us—"The 'inventions and imaginations' of these younger men are based upon 'assumed facts,' which are, 'in my opinion, to a large extent purely hypothetical.' I am 'suspicious of the influence which a preconceived theory has had on their estimate of evidence.' I therefore

'attach no value whatever' to their statements, and do not consider it necessary to lose time in weighing what they actually mean by this denudation of theirs, and all which as they contend must flow from it. My belief is that valleys are due to fractures and depressions. The Highlands abound in valleys, and therefore it must be evident, to every one capable of forming an opinion on the subject, that they abound also in proofs of fracture and depression."

In the foregoing remarks I have been dealing only with the Duke of Argyll's paper of February 1868, which, in his recent vigorously worded address, he cites as still unanswered, and which, therefore, we may suppose still to express his views. And yet no one can peruse that address without perceiving that it betokens a considerable change of opinion. Especially gratifying must it be to that "younger school" of geologists, against which the Duke has so vehemently lifted up his protest, to observe that the lapse of time, which he would not allow to have had much denuding effect upon the rocks, has yet been able to strip off from himself some of that crust of preconceived "theory" against which no argument or adverse fact could once make any impression. It is true that his Grace formerly thought it necessary to assure us that time could do nothing by itself, "nothing except by the aid of its great ally, Force—"Force working in Time." Well, we shall not quarrel about the use of words, but cheerfully admit that the change which has become perceptible in the opinions of the Duke of Argyll, is wholly the result of "Force working in Time," and not a very long time, for it cannot be stretched out beyond five years. Surely if the lapse of so brief a space, with all the amount of force which we can crowd into it, can have modified geological opinions which certainly seemed as solidly and unalterably fixed as his own Ben More itself, it can hardly be too much to hold that by the end of another *lustrum* still further modification may justify the confident belief that his Grace may still come to join the "younger school" heart and hand. We can assure him a jubilant welcome.

But it may be asked, what is the nature of this present alteration of view? In brief it may be put thus:—The Duke of Argyll finds that after all Denudation is one of those disagreeable facts which will insist on being prominent,—“chiefs that winna ding.” He has discovered that it really has had some share in the shaping of the present outlines of the land. He now admits in words “that the forms of hill and valley which preceded the coming on of glacial conditions [during the *Ice-Age*] had been themselves determined in a large degree by previous denudations.” And even though this general admission is neutralised by statements which follow it, it is most welcome as an indication doubtless of the effect of those “more extended opportunities of observation”

which his Grace tells us he has since enjoyed, and on the continuance of which our hopes of his secession to the ranks of the "younger school" are mainly based.

The Duke of Argyll once more appeals to the details of geological structure. Most gladly do we accede to the appeal. He points to the contorted condition of the older rocks as evidence of the extent to which they have been affected by subterranean movements. But no geologists are more familiar with these facts than his maligned "younger school." He conceives that it was after such movements that the forces of denudation began to work. Most assuredly; this has been explained over and over again. He affirms that "so long as such hills and mountains last at all, and wherever they are exposed to view, they bear upon them the unmistakable impress of their origin, and of the mighty subterranean forces to which their structure is due." This sentence is rather ambiguous. If it means that contorted rocks retain evidence of contortion, such an obvious truism was hardly worth a sentence to itself. If it means that a mountain made of contorted rocks has had its form determined at the time of contortion, the statement is mere assertion, and a begging of the very question to be proved.

In the same address the noble President declares that "denudation has done its work along the lines determined by upheaval, by fracture, and by unequal subsidence." This has never been denied by any one. A main object of my book was to show how, by means of denudation along such lines, much of the present contour of Scotland had been produced. Again we are told, "All sedimentary beds must have had an edge somewhere; and if they are lifted into a vertical position, and the edges come to be exposed, the removal of a small amount of material may result in a horizontal surface, or in surfaces cutting across the lines of structure at every variety of angle." If the Duke intends this explanation to apply not to a mere hand specimen, but to any district of convoluted and vertical rocks, such as the hills of Wales or the southern uplands or Highlands of Scotland, he cannot have noticed the string of physical absurdities which it involves. The rocks are often vertical, or nearly so, for miles at a stretch. Could we put them into something like their original horizontal or gently inclined position, their present edges would end off in a cliff many miles high. Can his Grace expect any one to believe that the beds, which certainly "must have had an edge somewhere," ever ended off in that fashion? But this would be only a part of the feat. In actual fact the rocks have been violently contorted, so that a series several hundreds or even thousands of feet in thickness is folded again and again upon itself. The present surface has been cut across these foldings, and in great part has its inequalities independent of them. If we

could flatten these curved rocks out again from their present condition, they would show a series of deep sharp troughs separated by steep pyramidal ridges of flat strata. And from the Duke of Argyll's teaching we should learn that this wonderful arrangement was the normal plan in old times of laying down sediment, which, instead of always going to the bottom and filling up the hollows as it does now-a-days, contrived then to ascend, layer after layer, like the tiers of the Great Pyramid, as if it were under the impulse, not of mere gravity or of the play of ocean currents, but of the methodical action of organisms like the coral polyps. We should further learn that these neatly-shaped sand and mud ridges and troughs were so accurately laid down, that when subterranean forces came into action and crumpled the whole up, every ridge popped conveniently into a trough below as if a trap-door had been opened for its reception, and with such nice adjustment as to bring its top to the same general level as the bottom of the former troughs!

The truth is, and in common fairness I am bound frankly to state it, that such assertions as those with which I am dealing could never be made if geological structure were really understood and kept in view. This is a matter of science, and is only to be mastered by the same patient toil which is required in other scientific inquiries. Moreover, it is by no means so easily mastered as it seems to be. The first absolute requisite for overcoming our ignorance is to reduce our facts, or what we suppose to be facts, to the test of ocular proof and measurement. Let us construct a section across the tract of which we would master the structure, and to avoid risk of error from exaggeration of proportion, let us begin by making the section as nearly as possible on a true scale, that is, giving the same value to length as to height. With the outline of the ground accurately traced, we may then, section in hand, insert upon it at the proper places, and with the true angle of dip, such rocks as we may be able to see exposed. Having fixed these data in this patient way, we may expect with some confidence to understand and fill in the geological structure of the ground for ourselves, and to make it intelligible and credible to others. Until we have gone through such a training ourselves, or have learned adequately to appreciate what it is from the labours of others, we have no right to utter an opinion on the relations between geological structure and external form, for we are destitute of one of the necessary qualifications for dealing with the problem.

The greater part of the recent address of the President of the Geological Society deals with the traces of ice-action in this country, and the manner in which they are to be accounted for. In his remarks upon this subject the Duke again places himself in opposition to the views of the "younger school," and ex-

presses opinions from which every member of that school would, I am sure, emphatically dissent. It is no part of my present purpose to enter upon these. I cannot, however, pass by one statement in the address. His Grace asserts that these restless "younger geologists" have recently made a most complete change of front. He therefore directs his attack against this new position. He says that they no longer maintain the existing system of hill and valley to have been cut out of the solid by an enormous glacier, but admit the general contour of the country to have been very much the same before the ice-age as after it, all the work of the ice having been to deepen valleys, degrade hills a little, and fill up the plains and hollows with clay and sand. "Such, as I understand it," says the Duke, "is the *new* glacial theory." But surely he can have paid but scant attention to the subject if he imagines that this idea is in any sense *new*. I really cannot recall that the geologists of the "younger school" have for many years past held any other view than that which they are now said to have adopted only recently. If, for example, his Grace will turn to the little volume which he abused so heartily in the spring of 1868, he will find the "new" view stated as plainly there as words can express it.* And yet in this address he thinks it needful to adduce evidence to disprove that valleys have been gouged out by an universal ice-sheet—a notion which, according to his own showing, the younger school does not hold.

These remarks have been extended this evening beyond the length to which I had originally proposed to confine them. My excuse must lie partly in what to myself is the ever-fresh charm of the subject, and partly in the desire to vindicate the fair fame of the modern Huttonian school of geology from attacks which had been in some measure called forth by writings of my own. I have again to express my regret that it was impossible to avoid an appearance of personal conflict, and I am conscious that a man who does his best to give as good as he gets in such conflict is apt to do more than he meant. I can only hope that this consciousness has kept me far within the bounds of legitimate reply.

Of one thing I feel securely confident. When the din of strife has ceased, and men come to weigh opinions in the dispassionate light of history, the profound influence of the Huttonian doctrines of the present time on the future course of geology will be abundantly recognised. By their guidance it will be possible to reconstruct the physical geography of the continents in successive ages back perhaps into some of the earliest periods of geological history. This work, indeed, is already in part accomplished. But much more remains to be done before the history

* *Op. cit.* p. 150.

of the land on which we live has been wholly unravelled. This is the task to which we have set ourselves, in which we have found ample scope for enthusiasm and hard work, and out of which we trust that there will eventually come a story of permanent interest to all whose range of vision extends beyond the present condition of things, and who would fain understand what now is by the light of what once has been.

NOTE.—Since the preceding address was read to the Society, I have obtained my copy of the Quarterly Journal of the Geological Society for November 1873, and have perused the communication which appears there from the Duke of Argyll—"On Six Lake-basins in Argyllshire." This paper is interesting in so far as it shows a further shaking of that confidence with which his Grace used formerly to connect valleys with fractures. He admits that "he is not able to say positively that the bed of Loch Fyne occupies a fault," and that "the strata are so like each other that it is difficult to trace faults." These admissions, though late, are welcome. And yet, instantly forgetting that they have been wrung from him by the evidence of his own eyes, the Duke on the very same page, nay, only nine lines further on, and before the ink could have been dry with which these quoted words were written, goes on to say that the upper basin of Loch Fyne "is sufficiently accounted for when it is seen to occupy a great depression parallel with the general lines of subsidence which the strata have evidently undergone." Here, once more, is the old style of "argument"—the strata dip rapidly to the north-west, therefore they show proofs of "great depression" and "lines of subsidence," and hence the basin of Loch Fyne is "sufficiently accounted for!" Did it never occur to his Grace that there must be, to say the least, just as much difficulty in proving the local depression of any part of a set of strata all dipping one way, as he has now found that there is in obtaining evidence as to where they are traversed by great faults?

The Duke of Argyll refers to "Mr Geikie's Geological map of Scotland" as containing a delineation which he has not been able to verify. If he looks again at the title of the map, he will find that the honoured name of Sir Roderick Murchison appears there as well as mine to share in the responsibility of authorship. The point on which the Duke has not been able to verify our observations involves only a simple piece of geological structure. Loch Fyne runs in a north-easterly direction along the strike of rocks which have a general north-westerly dip, so that the strata on the south-eastern bank belong to a lower, and those on the north-western side to a higher, part of the same great series.

The observations which his Grace here reiterates as to the geological structure of Loch Awe are quite in keeping with his

remarks on Loch Fyne. He states that the strata on either side of the lake dip towards each other, and that "the bed of Loch Awe, therefore, lies in a hollow or basin, which is sufficiently accounted for by structural causes." When his Grace returns to that charming sheet of water, let him take a compass and a sheet of the Ordnance map, and put down accurately the direction of the strike of the rocks. He will then discover that the lake winds across the strike, and certainly does not coincide with any actual structural cavity. At the same time, looking again at that map of Scotland to which he has referred, *and on which the great synclinal trough is marked, stretching north eastward from the Sound of Jura along the tract of Loch Awe*, let him ask himself the following questions:—1. If Loch Awe was formed by the plication of the rocks into a great synclinal fold, why does it not run on along the same line as far as the same structure continues? 2. What am I to make of those vertical rocks in the middle of this lake, for they have clearly no business to be there if the sides of the lake are formed by the inward dip of the schists? 3. The plication of the rocks has been proved to have taken place before the deposition of the Old Red Sandstone. By what possible means can I account for the preservation of the original surface ever since the middle of the Palæozoic ages, when the evidence of enormous surrounding waste, and of the consequent accumulation of thousands of feet of solid rock rising higher than the level of Loch Awe, is so strong that it cannot be ignored any longer? 4. Even if I succeed in keeping the tract tolerably free from the destructive touch of rain, frost, glaciers, and all the other weapons with which the younger school is so well equipped, how am I to prevent Loch Awe from being silted up by the gradual working of the showers and streamlets of all the long ages which have passed since the Old Red Sandstone?

The Duke of Argyll instances several minor lakes in the same part of the country as cases where the grinding action of ice cannot be appealed to as a possible cause. From his description of them, however, they appear to be identical in character with hundreds in other parts of the country which have certainly no origin in mere geological structure, and which, with Professor Ramsay, I believe to have been excavated by ice. But this question of ice-action is not the matter in dispute at present. His Grace adduces no evidence to show that his little tarns took their rise during that ancient time of plication, and have kept their existence ever since. He could not have stumbled on a line of argument more fatal to his cause than that furnished by lakes. This I have shown elsewhere, and I may refer him to the writings ("Trans. Geol. Soc. Glasgow," vol. iii. p. 178, *et seq.*; "Jukes' Student's Manual of Geology," 3d edit. pp. 460–3), in which the question is discussed.