TERTIARY VOLCANOES OF THE WESTERN ISLES OF SCOTLAND. 187

12. *The TERTIARY VOLCANOES of the WESTERN ISLES of SCOTLAND.*
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I. INTRODUCTION.

IN the year 1874 I communicated to this Society the results of my studies concerning the Tertiary and Older Volcanic rocks of Western Scotland, and their relations to the Secondary strata with which they are so intimately associated *. The very generous manner in which the memoir dealing with those subjects was received by this Society—and especially by those who were at that time the leaders of geological thought in this country—can never be forgotten by me. Such a reception lays upon the author of a memoir certain very obvious responsibilities. In the event of serious and fatal errors being indicated in his observations or reasoning, it is clearly his duty to come forward and frankly withdraw the statements to which this Society has given such wide currency. It is equally incumbent upon him, should his premises and conclusions be openly challenged on what seem to him to be insufficient grounds, to undertake the task of re-examining and, if necessary, of defending his views.

It may not perhaps be out of place to remark that my work in Scotland, to which were devoted five years of heavy labour and careful research, was one that taxed to the full the powers of a private individual. It involved the examination of the physical relations and the discussion of the palæontological characteristics of the whole of the rocks of the Highlands between the Old Red Sand-

* Quart. Journ. Geol. Soc. vol. xxx. (1874), pp. 220-302.

stone and the Glacial deposits. Of these formations, it may be remembered that the strata of Carboniferous and Cretaceous age had remained wholly unrecognized before my studies commenced ; while, with respect to the age and relations of the several members of the Triassic and Jurassic systems, as well as concerning the great masses of igneous rocks, very confused and inaccurate ideas prevailed. I may add that the country in which these rocks are exhibited was at the time quite destitute of any reliable maps upon which the observations of the geologist could be accurately recorded. Under these circumstances, I may claim that my work should be judged, I will not say with indulgence—for such I do not ask—but with that fair consideration which pioneer labour in our science has always received at the hands of those who constitute this Society. If the great outlines of the subject were faithfully traced, and the true bearings of the leading facts were rightly appreciated, I submit that this was all that could be expected under the circumstances, as it was certainly all that I hoped to be able to accomplish.

During the last fifteen years it has been my endeavour to fill in the sketch, of which the outlines had, I believe, been accurately drawn, and to correct or supplement those points of detail upon which my information was defective ; and I trust that, in doing so, I have shown no reluctance to confess omissions or mistakes of which I may have been guilty.

My original intention had been to confine the series of memoirs on “ The Secondary Rocks of Scotland ” to the description of the stratified deposits ; but when I came to study the relations of these to the igneous masses in the Western Isles of Scotland, I found that such very serious errors had been made in the interpretation of the latter, as to necessitate the preparation of a general sketch of what appeared to me to be their true relations. In justification of this course, I am compelled, in consequence of statements that have recently been made, to refer to the literature bearing upon the subject which was in existence when I commenced my task in 1871.

II. VIEWS WHICH WERE MAINTAINED PREVIOUS TO 1874.

The very able geologist, mineralogist, and chemist, Dr. John Macculloch, early in the present century, gave a most admirable account of the igneous rocks of the Western Isles of Scotland. He clearly perceived and distinctly pointed out the fact that the granites and gabbros of the area constitute, with the felsites and basalts, one great contemporaneous series of rocks—a series which, as a whole, overlies and is younger than the Secondary strata of the district. He noticed the *apparent* interbedding of these igneous rocks with Oolitic and Liassic strata ; but he clearly demonstrated that in all such cases the igneous masses are really intrusive in, and therefore younger than, the Jurassic rocks*.

* Macculloch's observations were published in the First Series of Transactions of this Society, and afterwards in fuller detail in his classic ‘Description of the Western Isles of Scotland,’ 3 vols. with plates and maps (1819).

In 1851 the Duke of Argyll brought forward the evidence of the plant-remains of Ardtun, which Professor Edward Forbes correctly interpreted as proving the Tertiary age of the basalts with which they were intercalated *.

Unfortunately, as the result of a hasty yachting-cruise in the Western Isles, Prof. Forbes at the same time suggested, though with much reserve, that the basalts of Loch Staffin might possibly be contemporaneous with the associated Jurassic strata †.

In the year 1858 Dr. Archibald Geikie published his first memoir dealing with the igneous rocks of the Hebrides ‡. In this memoir the most serious charges were brought against Dr. Macculloch, his veracity was impeached, and an attempt was made to throw discredit upon the whole of his geological observations §. The author, as the result of his own studies, maintained that the granitic rocks of central Skye belong to two different periods. He noticed the peculiar rocks of Cnoc nam Fitheach, but failed to recognize that they are masses of volcanic agglomerate.

The same author, in 1861, published a paper devoted to the full consideration of "the Chronology of the Trap-Rocks of Scotland" ||.

This memoir, which is illustrated by a map and section, aims at substituting for the conclusions of Macculloch the following propositions:—

(1) The gabbros ("hypersthene-rock") of Syke are of metamorphic origin and of *Laurentian* age.

(2) The great mass of the igneous rocks of the Western Isles are of *Oolitic* age, and this is also the age of the numerous dykes traversing Southern Scotland and the North of England.

(3) In Mull, however, there occur certain basalts which were erupted during the *Tertiary* period.

In 1865 the same author published a work ¶ in which the *Laurentian* age of Skye gabbros, and the *Oolitic* age of the great mass of the basalts of the Western Isles of Scotland, is still maintained.

Two years later appeared a memoir from the same hand, in which it is admitted that, so far as Mull is concerned, the reference of the great masses of basalts to the *Oolitic* period was a mistake, and that they really belong to the *Tertiary* **.

Still later, in 1871, a very admirable account of the geology of the Island of Eigg, by the same author, was preceded by a statement of his general views concerning the relations and age of the igneous rocks of the Western Isles of Scotland. It must be pointed out that in this general statement, while the *Tertiary* age of all the basaltic

* Quart. Journ. Geol. Soc. vol. vii. (1851), pp. 89 and 103.

† *Loc. cit.* p. 108.

‡ *Ibid.* vol. xiv. (1858), pp. 1-23.

§ *Loc. cit.* pp. 3-4. I have shown on a former occasion that these very grave charges against one of the great pioneers in British Geology are entirely groundless. See Quart. Journ. Geol. Soc. vol. xxxiv. (1878), p. 700 (footnote).

|| Trans. Roy. Soc. Edinb. vol. xxii. (1861), pp. 633-753.

¶ 'The Scenery of Scotland viewed in connection with its Physical Geology,' with a Geological Map (1865).

** Proc. Roy. Soc. Edinb. vol. vi. (1867), p. 71.

rocks is maintained, no reference is made, either in the text or in the table given, to the gabbros of Skye, which, as we have seen, had always been held by the author to be metamorphic rocks of Laurentian age. The gabbros of Mull are doubtfully referred to as being basaltic lavas which have undergone metamorphism*.

In the same year, 1871, Prof. Zirkel published his admirable "Geologische Skizzen von der Westküste Schottlands"†, containing a very excellent account of the petrology of the district. In treating of the age and relations of the various rock-masses of igneous origin, he unfortunately took Dr. Geikie as his guide rather than Dr. Macculloch; and as Zirkel's work has become classical, owing to the excellence of his petrographic descriptions, the erroneous representations of the relations and ages of the igneous rocks given in it have been extensively copied and very widely circulated. Even at the present day most foreign manuals of geology continue to reproduce these admittedly erroneous statements concerning the age and relations of the igneous rocks of Western Scotland.

Such was the state of information upon the subject when I took up the study of the district, and after several years of continuous work upon it, published my paper of 1874. While acknowledging the very great assistance I had derived from the writings of Dr. Macculloch and Professor Zirkel, I was compelled to state that my observations led to conclusions at utter variance with those arrived at by Dr. Geikie, as indicated above‡.

After an interval of nearly fifteen years, the last-named author has issued a memoir§ in which, while abandoning his old views and adopting in almost every particular the conclusions I had announced in 1874, he lays great stress on certain minor points of difference between us, and offers a different theoretical explanation of the facts from that which I had proposed.

My object, in the present memoir, is to direct attention to the following subjects:—

First. The conclusions of my paper of 1874 which find support in the observations since made by Dr. Geikie and other members of the Geological Survey—the results of which are given in the memoir referred to;

Secondly. The observations which seem to contradict or qualify the conclusions at which I arrived in 1874; and

Thirdly. The alternative theory which has been proposed to explain the general relations of the rocks of the district.

III. CONCLUSIONS ANNOUNCED IN 1874, WHICH HAVE BEEN CONFIRMED BY SUBSEQUENT OBSERVATIONS.

I cannot do better than to state, as succinctly as possible, the chief propositions the truth of which I endeavoured to establish in my

* Quart. Journ. Geol. Soc. vol. xxvii. (1871), pp. 282-283.

† Zeitschr. d. d. geol. Gesellsch. Bd. xxiii. (1871), pp. 1-124.

‡ Quart. Journ. Geol. Soc. vol. xxx. (1874), p. 223 (footnote).

§ "The History of Volcanic Action during the Tertiary Period in the British Isles," Trans. Roy. Soc. Edinb. vol. xxxv. pt. 2, pp. 21-184.

previous memoir, and point out how far these have been substantiated, modified, or refuted by the work of the Director-General of the Geological Survey and of different members of his staff.

By far the most important of the results of my study of the igneous rocks in the Western Isles of Scotland was the establishment of the true relation to one another of the so-called *Plutonic* and *Volcanic* rocks of that district. Darwin had long before shown similar relations to exist between the granites and lavas of the Andes; while Jukes and other writers had insisted upon the gradation of highly crystalline into glassy rocks as being on *à priori* grounds probable. But the demonstration that in a particular area there exist the data for illustrating the complete transition of granitic into glassy rocks, both in the acid and basic series, has been regarded not only in this country, but by many foreign geologists, such as Suess, Reyer, and Dana, as marking a distinct and important addition to geological knowledge.

A. The Relations between the Plutonic and the Volcanic Rocks of the Western Isles of Scotland.

My conclusions on this subject may be briefly summarized as follows :—

There exist in the Western Isles of Scotland true Plutonic rocks (*granites* and *gabbros*) exhibiting a perfectly holocrystalline structure, and it is possible to trace every gradation from these through different microcrystalline and cryptocrystalline rocks into truly vitreous ones (*pitchstones* and *tachylytes*). The distinction between Plutonic and Volcanic rocks—however convenient and necessary it may be in practice—is a purely arbitrary one, some lavas being more highly crystalline than certain portions of intrusive masses; for the degree of crystallization in each case is determined by the conditions under which the originally molten masses have been placed. (Quart. Journ. Geol. Soc. 1874, pp. 233–248).

I need not remind the members of this Society that these views—though they were very warmly accepted by many fellow-workers in this country—were received in many quarters abroad with much opposition and even with ridicule. Equally unnecessary is it to refer to subsequent papers in which I endeavoured to support and defend my conclusions by a reference to other areas, or by giving more detailed descriptions of some of the Scottish rocks*.

Within the last few years, however, a number of foreign geologists have described relations very similar to those which I pointed out in Scotland as existing between the Plutonic and Volcanic rocks of other areas; and the conclusions which they have arrived at are almost identical with my own. This is especially the case with

* See especially the Memoir “On the Ancient Volcano of Schemnitz, Hungary,” Quart. Journ. Geol. Soc. vol. xxxii. (1876), p. 292, and ‘Volcanoes, what they are and what they teach’ (1881), pp. 53–56; also Quart. Journ. Geol. Soc. vol. xli. (1885), pp. 354–418, and vol. xlii. (1886), pp. 49–97.

Messrs. Arnold Hague and J. P. Iddings in regard to the rocks of the Washoe district*; with Signor B. Lotti in respect to the Elba rocks†; and with Prof. A. Stelzner in respect to the rocks of the Andes‡, previously studied by Darwin.

It is very gratifying to me to find that in the memoir recently published there is the most unreserved and complete acceptance of the conclusions announced by me in 1874; and this acceptance is of the greater significance when we bear in mind the great difference in the views previously maintained by the author of the memoir. The studies of the Director-General of the Geological Survey and of different members of his staff have afforded evidence of the most unmistakable character that gabbros graduate insensibly into dolerites, and dolerites into basalts, while the last-mentioned rocks pass into tachylytes (Trans. Roy. Soc. Edinb. 1888, pp. 122–124); they are equally satisfied with the proofs that true granites merge insensibly through various intermediate forms, called “granophyric” by Rosenbusch, into the acid lavas and pitchstones (*loc. cit.* pp. 145–150); not less unhesitating is the testimony of the recently published memoir to the relation between the textures of the several types of igneous rocks in the Hebrides and their geological positions. It is maintained that the finer-grained varieties occur in the smaller intrusive masses and in the peripheral portions of larger ones (T. R. S. E. 1888, pp. 120–147, &c.), and that in many massive lava-streams we find rocks of more highly crystalline character than in some of the smaller intrusive dykes and sheets (*loc. cit.* pp. 77, 117, &c.). Finally the conclusion is stated that the differences between the gabbros and the basic lavas, and between the granites and the more acid ones, must be ascribed to the circumstance that the Plutonic rocks have consolidated at great depths beneath the surface, and therefore slowly and under great pressure, while the lavas cooling at the surface have not been subjected to these conditions (*loc. cit.* p. 140 &c.). These passages may be compared with the general account of the relations of the igneous rocks of the district (Q. J. G. S. 1874, pp. 233–248).

B. The Existence of five Great Centres of Eruption, with many minor and scattered Vents in the Western Isles of Scotland.

My conclusions on this subject, as given in 1874, may be stated briefly as follows:—

There exist at five well-marked centres—namely, Mull, Ardnarmurchan, Rum, Skye, and St. Kilda—clear evidence that eruptive action on a grand scale took place. This evidence is as follows:—

(1) There occur enormous masses of lava, of both acid and basic

* “On the Development of Crystallization in Igneous Rocks of Washoe, Nevada,” U. S. Geol. Surv. Bull. No. 17 (1885).

† ‘Memorie descrittive della Carta Geologica d’Italia,’ vol. ii.; “Descrizione Geologica dell’ Isola d’ Elba” di B. Lotti, R. Com. Geol. d’ Italia (1886).

‡ Beiträge zur Geologie und Paläontologie der Argentinischen Republic (1885), pp. 198–213.

composition—successive currents being piled upon one another to a great thickness.

(2) Vast deposits of volcanic agglomerate (scoria and dust) are found, sometimes containing many blocks of non-volcanic materials, and thus passing into breccias, which are composed of materials derived from rocks underlying the centres of eruption.

(3) Numerous intrusive masses, frequently nearly horizontal in their disposition (sheets) and sometimes more or less vertical (dykes), are crowded together at these five centres. The largest of these intrusive masses, especially the great nearly horizontal sheets, consist of gabbro and granite, while the smaller ones, the dykes and the peripheral portion of the great bosses and sheets, pass into dolerites, basalts, and “felstones,” exactly similar to the materials of the lava-currents.

(4) The basic intrusions tend to form wide-spread sheets, while the acid ones assume those more bulky and lenticular forms for which the name of “laccolites” has since been proposed by Gilbert.

(5) Both the sedimentary and the igneous rocks, among which these masses are intruded, afford very striking illustrations of contact-metamorphism around the intrusions.

(6) Many minor centres of eruption exist in the area, of which the examples of S’Airde Beinn* (Sarsta Beinn) and Beinn Hiant (Beinn Shiant) are described as being among the most striking and typical.

It is interesting to find every one of these conclusions adopted and confirmed in the memoir lately published.

(1) With respect to the centres of eruptive action, it is only necessary to turn to the table of contents of the memoir recently published to perceive that the display of volcanic activity and the development of intrusive masses are alike recognized as occurring at just the five points indicated in 1874. (See the description of these phenomena, T. R. S. E. 1888, pp. 84–100, 122–175, and compare it with Q. J. G. S. 1874, pp. 242–270.) Near one of these great centres, that of Mull, the piled-up lava-streams are asserted, in spite of the great denudation they have suffered, to attain a thickness of over 3000 feet. (See T. R. S. E. 1888, p. 91.)

(2) With respect to the vast masses of volcanic agglomerate at the several centres of eruption, an attempt is made in the recent memoir to minimize their importance. It will be sufficient for my purpose to point out the descriptions given of the tuffs of Antrim (T. R. S. E. 1888, p. 87 *et seq.*), of Mull (*ibid.* p. 83), of Rum

* In 1874 the only maps available for geological purposes were the Admiralty Charts (in which only the parts adjacent to the coast-lines are shown in detail) and some very defective maps issued by private individuals. In all of these a more or less phonetic spelling of the Gaelic names was adopted. In the admirable maps of the Ordnance Survey, which have since been issued, the correct spelling of the Gaelic is given; but many of the names, as now written, have a very unfamiliar appearance to English readers. To avoid confusion, I have placed the old spelling in brackets after the new ones employed on the maps.

(*ibid.* p. 134), and of Skye (*ibid.* pp. 97, 98, 109, &c.). Especially significant is the admission that in Strath we have evidence of whole mountain masses of volcanic agglomerate, and the belief of the author of the memoir that these indicate the existence of a great funnel or vent, originally filled with these materials, *which was no less than two miles in diameter* (*ibid.* p. 108). These accounts may be compared with those which I gave in 1874 (see Q. J. G. S. 1874, pp. 239–242 *et seq.*). Especially close is the agreement between the account given of the volcanic breccias of Rum (compare T. R. S. E. 1888, p. 134, and Q. J. G. S. 1874, p. 253). It is perfectly true, as was pointed out in 1874, and insisted upon in the recent memoir, that at a distance from the great centres of eruption the lava-sheets are usually separated only by thin and unimportant layers of tuff.

(3) The great intrusive masses so often presenting a horizontal disposition and pseudo-stratification, exhibiting evidence of successive extrusion, and the phenomena of segregation or contemporaneous veins, were described in 1874 (see Q. J. G. S. 1874, pp. 238–252, and pl. xxiii. figs. 1, 3, 4, 5). All the details are fully confirmed in the recently published memoir (T. R. S. E. 1888, pp. 111–143 and 151–164).

(4) In 1874 Mr. Gilbert had not described the interesting phenomena to which he proposed to give the name of “laccolites;” but the lenticular form of the great felsitic intrusions was clearly pointed out in my paper (Q. J. G. S. 1874, pp. 268, 269), and the concentric structure of some of the more acid rock masses, so closely resembling the internal character of the domitic Puy^s in Auvergne, was also indicated (compare Q. J. G. S. 1874, p. 245 (footnote), and T. R. S. E. 1888, p. 162).

(5) The evidences of metamorphism produced in both sedimentary and volcanic rocks by the great intrusive masses, which was pointed out in 1874, is very clearly and fully described in 1888 (compare Q. J. G. S. 1874, pp. 251, 252, &c., and T. R. S. E. 1888, pp. 101–103, 138, 165–167, &c.).

(6) The very full accounts of later eruptions, like that of S’Airde Beinn (Sarsta Beinn) and other smaller outbursts composed of different kinds of acid and basic rock, and seen bursting through the basalt of the great plateaux, as given in the recent memoir, are in full accord with the earlier descriptions (compare Q. J. G. S. 1874, pp. 260–267, and T. R. S. E. 1888, pp. 101–103, 177, 178, &c.).

C. *The Subaerial Origin of the Lavas and Tuffs, and the Tertiary Age of both the Plutonic and the Volcanic Rocks.*

My conclusions on these subjects may be summarized as follows :—

(1) There is a total absence of any contemporaneous marine deposits intercalated among the lavas of the district; but, on the contrary, we find many unmistakable proofs that the lavas and tuffs were ejected under *subaerial* conditions. These proofs consist in the

presence between the successive lava-flows of burnt soils ("laterites" of some authors); of beds of lignite, with the remains of terrestrial vegetation; of sheets of volcanic mud; of river-gravels; and of characteristic lacustrine deposits—including beds of iron-ore analogous to the well-known "lake-ores" of Sweden.

(2) While some of the igneous rock-masses are of strikingly fresh appearance and unaltered character, others exhibit the most unmistakable evidence of having undergone remarkable changes, so that they have come to resemble the igneous products of far older geological periods. There is the clearest evidence, however, that the whole of the igneous rocks, gabbros, and granites, equally with basalts and "felstones," are of *Tertiary* age.

(1) The conclusion as to the subaerial origin of the volcanic rocks of the Western Isles is fully borne out by the detailed memoir just published (see T. R. S. E. 1888, pp. 87-89). This confirmation is particularly important at the present time.

My friend Dr. E. Reyer, in his recently published very valuable 'Theoretische Geologie,' has argued that the highly crystalline character of the gabbro- and granite-masses of the Western Isles of Scotland must be the result of "hydrostatic pressure," and that the weight of overlying and permeable rocks could not possibly account for the production of the granitic texture. He therefore insists that the more highly crystalline portions of the Hebridean eruptions must have been of submarine origin, and contemporaneous with the marine Mesozoic strata among which they lie*.

On the other hand, M. Loewinson-Lessing, adopting a view very generally held in Germany that the ophitic structure—which I have shown to be so characteristically exhibited by the basaltic lavas of Western Scotland—is distinctive of the diabases, has argued that this structure is the result of the outflow of lavas of basic composition under a considerable pressure of sea-water†.

That the ophitic, or "diabasic" structure, as some authors prefer to call it, is found both in Iceland and in Scotland in rocks which were extruded in Tertiary times and under subaerial conditions has been maintained both by M. Bréon and by myself, and I am happy now to be able to fortify my own position by the testimony of the officers of the Geological Survey.

(2) With respect to the Tertiary age of the gabbros and granites, as well as of the basalts and "felstones," the evidence brought forward in the recently published memoir is equally conclusive (T. R. S. E. 1888, pp. 84, 182).

It is impossible, in view of discussions which have taken place upon the subject, to overestimate the importance of the admissions now made.

Dr. Sterry Hunt and others have frequently quoted the case of the so-called "Norites" of the Western Isles as supplying evidence

* Theoretische Geologie von Dr. E. Reyer (Stuttgart, 1888), p. 371.

† Bull. de la Soc. Belge de Géol. &c., tome ii. (1888), pp. 84-87.

that this particular class of rock is characteristic of the Laurentian period.

On the other hand, the accuracy of the conclusions announced by me in 1874 has been again and again disputed, on the ground that the basalt and gabbros had been severally referred to the Jurassic and Laurentian systems.

In the year 1874, Mr. Allport, in his classical paper on the British Carboniferous Dolerites*, showed conclusively that many of the basaltic rocks of Palæozoic age were as fresh and unaltered as many Tertiary lavas. In the paper which I published in the same year, it was pointed out that many of the rocks occurring among the Tertiaries present all the characters which would, if found among older rocks, cause them to be classed as "porphyrites," "melaphyres," "diabases," &c. It is therefore satisfactory to find that the author of the recently published memoir is able to testify that he has "been unable to recognize any essential difference of structure or composition" between the Tertiary igneous rocks and those of far earlier geological age (T. R. S. E. 1888, pp. 74, 145).

IV. CONCLUSIONS ANNOUNCED IN 1874, WHICH ARE DISPUTED IN THE RECENT MEMOIR.

There are several points of detail in which it is asserted that the results obtained by recent studies are at variance with those stated in 1874. In some of these cases I am perfectly willing to confess that I fell into error; but in others I shall certainly be able to show that the mistake has not been on my side. As none of these minor details in any way affects the main questions at issue, I may, for the present, pass them by.

There are two contentions in the recent memoir which do, however, more or less seriously affect the conclusions which my memoir of 1874 aimed at establishing. One of these is a matter of fact and of observation—namely, the relations between the different kinds of igneous rock-masses in the district; the other is a subject of inference and theory—namely, the explanation that is to be given of what I have shown in the preceding pages to be now mutually admitted facts. I will take up these two questions in the order named.

A. *The General Order in which the several Varieties of Igneous Rocks were erupted.*

The memoir of 1874 stated that, as a whole, the more acid rocks of the district were erupted before the more basic ones; but that after the extrusion of the great mass of the basaltic lavas there were numerous sporadic outbursts, and, as the result of these, various acid and intermediate varieties of rock made their way to the surface (see Q. J. G. S. 1874, pp. 272 &c.).

It may be pointed out that this conclusion was not one for which

* Quart. Journ. Geol. Soc. vol. xxx. (1874), pp. 529-566.

I was primarily responsible. As I stated at the time, Professor J. D. Forbes, in 1846, brought forward evidence which he believed showed that the "hypersthene-rocks" (gabbros) are intrusive in, and therefore younger than, the granites*; and Professor Zirkel, in 1871, arrived at the same conclusion†. I certainly found evidence in the central part of Mull and elsewhere which I think is conclusive on the point; and the responsible officers of the Geological Survey of Ireland, two years later, published an account of the relations between the acid and basic rocks in County Antrim which are in striking agreement with these results‡.

The recently published memoir, however, aims at showing that, both in the Western Isles and in Antrim, the granitic and other acid rocks are younger than the gabbros (T. R. S. E. 1888, pp. 151-171 &c.).

I must here, however, point out an unfortunate misunderstanding into which the author of the recent memoir has fallen with respect to my views concerning the relations of the different volcanic rock-masses.

Though I have insisted that the acid lavas (felstones) were, *as a whole*, ejected before the basic ones (basalts), yet I most clearly and emphatically pointed out that the great masses of granite are intrusive in, and therefore younger than, the great mass of the felstone-lavas. In proof of this I gave a section, seen at the summit of Beinn na Duatharach (Beinn Uaig), in the Island of Mull (Q. J. G. S. 1874, p. 246, fig. 1), which, with its legend, as given in 1874, is exactly reproduced on the next page (p. 198).

I cannot help thinking that much of the difference of view expressed in the recent memoir, as compared with the paper of 1874, is due to the employment of the same terms with a different signification.

At the very outset of my paper I stated that I should employ petrographical names (with certain specified exceptions) exactly as they were defined in Prof. Zirkel's admirable 'Lehrbuch der Petrographie' (Q. J. G. S. 1874, p. 233). I described my "basic, basaltic, or pyroxenic" rocks as in all cases containing olivine (Q. J. G. S. 1874, pp. 233-236 &c.), and I used the old English name of "felstones" as a convenient one for all lavas in which olivine was not an essential constituent. In employing the term "felstone" I desired to make it serve as a field-geologist's name for very varied types of rock, more acid than the olivine-basalts, which I saw would require much time and labour to be expended upon them before exact diagnosis was possible. In the present chaotic condition of petrographic nomenclature, opinions will no doubt differ as to the desirability of limiting the definition of basalt in the manner indicated. Two years later I pointed out the desirability of

* Edinb. New Phil. Journ. new ser. vol. xl. (1845-6) p. 86.

† Zeitschr. d. d. geol. Gesellsch. Jahrg. 1871, p. 90.

‡ Geol. Surv. Ireland. Memoir on Sheets 21, 28, & 29 (1876), p. 17. Doubt has now been thrown on the accuracy of these maps and memoirs by the author of the recent memoir (T. R. S. E. p. 171, footnote).

removing from the acid rocks the less highly silicated types which had all been included under that head by Bunsen, Durocher, Cotta, and others, and erecting them into a group by themselves *—the “intermediate” rocks. But I think that I cannot fairly be charged with leaving in doubt the sense in which I employed the term basalt in the year 1874; and I have since described these “ophitic

Rocks forming the Summit of Beinn Uaig, Isle of Mull.

a. Felstone-lavas, with agglomerates†.
b. Syenite-granite, graduating into felsite.

olivine-basalts” of the district in considerable detail‡. I was very careful to point out that many of my “felstones” are as dark-coloured as the basalts, but that they might be distinguished by their macroscopic as well as by their microscopic characters (Q. J. G. S. 1874, p. 236).

Many of the cases cited in the recently published memoir as intrusions of granite and felsite into basalt—and on which so much stress is laid—will, I think, be found to be, as at Beinn Uaig, intrusions into the dark-coloured “felstones” of my first period of eruption §.

I undoubtedly stated that, as a whole, the great ejections of acid material, both as intrusive masses and as lavas or tuffs, took place before that of the gabbros and basalts. But considering how vast a

* Quart. Journ. Geol. Soc. vol. xxiii. (1876), p. 295.

† These “felstone-lavas” are of very dark grey, almost black colour, and might easily be mistaken for basalts. Microscopically studied, however, they are found to present the very closest analogies with the rock described by M. Bréon from near Stikkisholmur, Iceland (see his *Géologie de l’Islande* &c. p. 23, pl. iii. fig. 1). This rock, which, like that of Beinn Uaig, contains no olivine, but has a microstructure very similar to that of many andesites, is classed by Bréon as an “augite-andesite.” In the second edition of his ‘*Massigen Gesteine*’ (1887), p. 687, Professor Rosenbusch adopts this reference of the Stikkisholmur-rock to the augite-andesites, and compares it with rocks of a similar type described by Förstner in Pantellaria. The “syenite-granite” is the usual drusy (miarolitic) granite of the district, which graduates through various granophyric modifications into a distinctly rhyolitic rock.

‡ *Ibid.* vol. xliii. (1886) p. 49.

§ A reference to the discussion which followed the reading of this paper will show that the author of the memoir of 1888 groups together the andesites and basalts as distinct from the acid rocks. See p. 219.]

period of time must have been covered by these Tertiary volcanic outbursts, and how enormous is the thickness of volcanic materials, it would not be at all surprising to find that some intrusive granites may have been ejected at a later date than some of the earlier-formed basalts and gabbros. I have, moreover, always contended that some ejections of acid materials belonged to a *third* period, later than that of the ejection of either the felstones or the basalts.

There are many observations made by the author of the recently published memoir which, while they are strikingly confirmatory of the view that the acid rocks are, as a general rule, older than the basic ones, appear to me to be quite irreconcilable with the opposite opinion.

Thus it is stated that "there are two horizons on which protrusions of acid materials have been specially abundant. One of these is the base of the bedded basalts of the plateau, the other is at the bottom of the thick sheets of gabbro" (T. R. S. E. 1888, p. 176). Abundant evidence is given in many parts of the memoir of this significant circumstance (*loc. cit.* pp. 116, 117, 172, 173, &c.).

It will, of course, be seen that this fact of the infraposition of the acid to the basic rocks points to the earlier age of the first-named rocks; and the author of the memoir in one case mentions an acid rock which is apparently split up by "a sill of dolerite" (*loc. cit.* p. 173). But in this and all similar cases he rejects the facts which seem to point to the priority of acid to basic rocks, and falls back on the explanation that the acid rocks have a "tendency" to be intruded just in the positions they would occupy if they were older than the basic ones (*loc. cit.* p. 172). I cannot myself accept the view that the acid rocks are in all these cases of an intrusive character.

Again, the author of the recent memoir is compelled to admit that abundant fragments of felsitic materials occur in the agglomerates alternating with the streams of basaltic lava (*loc. cit.* pp. 81, 82, 83, 108, 156, 157, &c.).

Now this fact, which I had myself frequently observed—especially taken in conjunction with the circumstance that many of these "felsitic" fragments present a markedly scoriaceous character—has always seemed to me to be only reconcilable with the preexistence of acid lavas from which these fragments were derived.

This difficulty appears to have been recognized; but it is suggested that the presence of these derived fragments of acid rock among the basaltic ejections is to be accounted for by supposing that the fragments in question were derived from *unejected* masses of igneous material, that were not actually brought to the surface bodily till a much later date! (*loc. cit.* p. 144). The explanation will be regarded by most geologists, I fear, as a "desperate" one.

Lastly, the author of the recent memoir is compelled to admit

that basaltic dykes are found, sometimes in great numbers, traversing both the granites and the felstones. In the central district of Mull it is stated that the vast masses of granite and other acid rocks are penetrated by numerous basalt-veins and dykes; and that these are, indeed, so abundant that the geologist "ceases to take note of their presence" (see T. R. S. E. 1888, pp. 158, 159, and fig. 50).

While, however, I cannot admit that the evidence points to the conclusion that the acid rocks of the Western Isles are of younger age than the more basic ones, there is one point on which I gladly take the opportunity of confessing and correcting an error which my subsequent study of the country convinces me that I fell into in 1874.

It was at that time inferred by me that a considerable interval of time must probably have separated the period of the eruption of the more acid lavas ("felstones") from that of the basalts which flowed from the great central volcanoes. Now, although the evidence is often very clear that the acid rocks had suffered a considerable amount of erosion before they were buried under the outflows of basalt, yet there are so many other indications of great denudation having taken place at different intervals during the whole of the vast periods covered by these Tertiary eruptions, that I am no longer prepared to maintain the special importance of this particular period of quiescence.

On the contrary, the longer I have studied the district the more convincing appears to me to be the evidence of a gradual change from the acid to the basic eruptions. Some of the felstones are augite-andesites and labradorite-andesites, but little more acid in character than the olivine-basalts of the great plateaux; and there are also abundant examples of lavas of more or less acid type having been erupted from the central vents from time to time, while the basalts were being ejected. But, as I have formerly shown, while the extremely liquid basalts flowed to distances of forty or fifty miles from their point of origin, the less fluid, acid lavas seldom flowed to distances of more than ten miles, and are, consequently, found confined to the flanks of the volcanoes from which they issued.

Just as there are examples of "felstone"-lavas among the basalts, so are there cases in which basaltic currents may be found among the older ones of more acid composition,—though it seems to me to be clear that the acid series was, *as a whole*, of earlier date than the basic one. The "pale-coloured felstones of Beinn More," which are claimed by the author of the recent memoir as being posterior to the basalts, are, if my interpretation be correct, a part of the older volcanic ejections.

The great mistake which I made in 1874 was, not in insisting upon the existence of a great series of "felstones" underlying the basalts, but in not recognizing the fact that these felstones include representatives both of the rocks which are more properly desig-

nated as acid, and of those for which I, two years later, proposed the name of "intermediate" rocks*.

I hope shortly to be able to describe some of the chief types of these rocks of intermediate composition—the lavas, including a great variety of andesites, their altered forms (the "propylites"), and their Plutonic representatives (diorites and quartz-diorites). When this is done, it will be seen how numerous and varied are the different types of these rocks which occur in the Western Isles of Scotland, and what striking resemblances they present with masses of the same age in the Faroe Isles and in Iceland. Still more remarkable are the curious modifications which these rocks in Scotland can be shown to have undergone from the action upon them of acid vapours when they were situated in close proximity to the great central vents.

B. The Existence in the District of great Volcanoes, which have been dissected by Denudation.

While there is such a close and remarkable agreement between the conclusions enunciated by the author of the recent memoir and those published in 1874, with regard to the phenomena that may be observed in the Western Isles of Scotland; yet, with respect to the *interpretation* of those phenomena and the general theoretical explanation of the mutually admitted facts, there is the widest possible divergence between that author and myself.

The generalization which I arrived at in 1874, and to which especial exception is now taken, may be stated as follows:—

The relations of the different rock-masses around the five centres of eruption in the Western Isles show that these were the sites of five great volcanoes. From their central craters, from parasitical cones on their flanks, and from fissures opened radially around these volcanoes, numerous eruptions, both of the explosive and the effusive type, must have taken place. The data now remaining to us in these old Tertiary volcanoes,—which have been admirably dissected by denudation,—enable us to make an approximate estimate of their dimensions, and to determine the general order of appearance of the materials which were ejected at the surface, and were simultaneously injected into the solid framework of the volcano itself or into the stratified rock-masses subjacent to them.

It is gratifying to find that most of the *observations* upon which I based these generalizations are accepted as correct, and, indeed, are adopted almost without reserve by the author of the memoir.

* The statement that though more acid rocks "occur abundantly in fragments in the volcanic rocks and agglomerate of the plateaux, not a single instance has been observed of their intercalation as contemporaneous sheets among the basalts" (T. R. S. E. 1888. p. 106), is certainly founded on too partial observation. The statement is qualified later on in the account of an andesite lava among the basalts of Eigg, and I shall have to describe many cases of the same kind in Mull, Skye, &c.

It is admitted that very numerous intrusions of both acid and basic rocks took place at just those five centres which were indicated by me in 1874—namely Mull, Ardnamurchan, Rum, Skye, and St. Kilda (T. R. S. E. 1888, pp. 84, 122-170). It is agreed that it is quite impossible to draw any sharp lines of distinction between the materials—whether acid or basic in composition—which constitute these intrusions and the lavas which during the same period were poured out at the surface (*loc. cit.* pp. 76-81, 122, 125, 178, &c.). The existence of enormous masses of volcanic agglomerates and breccias, the result of explosive action near these centres, is granted, although an attempt is made to minimize their importance (*loc. cit.* pp. 83, 87, 108, 109, 134). It is even allowed that there is proof of a crowding of the vents of eruption in the neighbourhood of these five great centres which have been indicated (*loc. cit.* p. 121).

Still more significant are the admissions that there is no evidence of the outflow of basaltic lavas having ceased before the extravasation of the gabbro-bosses, and that it is difficult to suppose that none of the dykes “communicated with the surface and gave rise to outpouring of basalt and the ejection of dust and stones” (*loc. cit.* p. 121). It is granted that “the points of extravasation of the materials,” “which ultimately solidified as dolerites, gabbros, troctolites, picrites,” &c., “were mainly determined by the positions of the larger or more closely clustered vents of the plateau-period, where lines of weakness consequently existed in the terrestrial crust. Rising as huge bosses through such weak places, the gabbros and associated rocks raised up the overlying bedded basalts, and forced themselves between them, forming thus a fringe of finer-grained intrusive sills and veins around the central amorphous cores of more crystalline material. Whether, in any of these vast domes of upheaval, the summit was disrupted, so as to allow the basic intrusion to flow out as lava at the surface, cannot now be told, owing to the enormous subsequent denudation” (*loc. cit.* p. 183).

It is only necessary to grant the very high probability of this last suggestion being the true one, to bring the views of the author of the recent memoir into complete accord with my own. I have argued that the very complete gradation which is now admitted to exist between the gabbros of the bosses and the ophitic basalts of the plateaux points to the conclusion that—all through the vast periods during which the basalts were being poured out at the surface, from a central crater or from parasitical vents—masses of the same material were finding their way into lower portions of the great cone and among the subjacent rock-masses, and consolidating in a more highly crystalline form. My critic, on the other hand—I fail to see upon what grounds,—asserts that the intrusion of the gabbros took place *after* all, or nearly all, the basalts had been erupted.

Lastly, it is admitted that the central subsidence—to which I particularly called attention as being similar to that which Darwin and other authors had shown to have taken place in the case of many volcanic centres of eruption—really occurred in the case of

the great volcanic centres of the Western Isles of Scotland (*loc. cit.* pp. 94, 142. Compare Q. J. G. S. 1874, pp. 256-7).

So far, indeed, as I have been able to discover, there are three, and only three, reasons adduced by the author of the recent memoir for rejecting the explanation which I offered of the phenomena in 1874; and these we may now proceed to consider.

First. It is said that there is no thickening of the sheets of basaltic lava as we approach the supposed central vents (T. R. S. E. 1888, pp. 99-100).

I may point out, in answer to this, that I have nowhere suggested that the basaltic lava-sheets were thicker nearer their points of origin than further away. On the contrary, I have again and again insisted upon the striking proofs which exist of the extreme liquidity of the basaltic lavas; and such liquidity would militate against the thickening of the lavas as we approach their points of eruption. Very liquid lavas of this type exhibit abnormal thicknesses where they have flowed into hollows (those of Skaptár Jökull are said to have attained a depth of over 600 feet in certain ravines); but I know of no observations which would warrant the conclusion that a progressive diminution in thickness can be detected in such lava-currents as they are traced away from their point of ejection. Further, I have insisted that many of the currents of basaltic lava must have flowed, not from a central crater, but from parasitical cones on the flanks of the great volcanoes, and of such parasitical cones I described a conspicuous example in S'Airde Beinn (Sarsta Beinn). (See Q. J. G. S. 1874, pp. 264-266, and compare T. R. S. E. 1888, pp. 103-104.)

On the other hand, I have always asserted that, as we approach the great centres of eruption, we find the short and bulky andesitic and more acid lava-streams increasing in number till they form a considerable portion of the whole mass (Q. J. G. S. 1874, p. 248). My critic admits the importance of these felsstone-lavas in the case of Beinn More in Mull, but with respect to other localities he appears to have overlooked their great number and significance. This seems to arise from the fact that he regards all the masses of acid rocks which lie below the basalts in Mull, Rum, Skye, and Raasay as being intrusive. He makes the important admission, however, that "as we retire from the mountain-tract [that is from the great centre of eruption in Mull] into the undisturbed basalts of the plateau, these acid intercalations gradually disappear." (T. R. S. E. 1888, p. 172.)

In the account which I propose to give of the remarkably varied series of intermediate and acid lavas at the five great centres of eruption, I shall show how they graduate insensibly into dioritic and granitic types on the one hand; while on the other they exhibit, where they approach the great central vents, those curious and interesting modifications of their constituent minerals, from the action upon them of acid vapours, which are so characteristic of the rocks which have been called "propylites."

Secondly. It is asserted that there is an absence of masses of pyroclastic materials (tuffs and dust) such as we should expect to find around great volcanic centres.

In reply to this I may remark that some volcanoes, like those of Hawaii, exhibit very little evidence indeed of explosive activity, and that their cones are almost wholly built up by successively outwelling lava-currents. But, as a matter of fact, there are very abundant evidences of explosive action at these great centres. My critic has completely adopted the account which I gave in 1874 of the agglomerates of Cnoc nam Fitheach in Skye, and of the volcanic breccias in the tract lying south of Allival and Barkeval in Rum. (See T. R. S. E. 1888, pp. 107 & 134, and compare Q. J. G. S. 1874, pp. 253-255.)

When it is admitted that at one of the great centres of eruption, Skye, there are masses of volcanic agglomerate developed on such a scale that it is possible to interpret them as indicating the existence of a volcanic "neck" two miles in diameter; when, at a second centre, Ardnamurchan, one mass of agglomerate (Faskadale), more than a quarter of a mile in diameter, is described, and another (Maclean's Nose) more than half a mile in diameter (T. R. S. E. 1888, p. 106); when, at a third centre, Rum, it is granted that volcanic breccias attain a thickness of 200 feet, extending as a continuous mass for three miles, and in a more interrupted manner much further (*loc. cit.* p. 134); and when, finally, details of similar occurrences are given in the case of a fourth centre, Mull (*loc. cit.* p. 83), it is hard to understand how the deposits of volcanic ejectamenta can be spoken of as being insignificant!

When the geological surveyors come to study the country in detail, they will find how numerous and extensive the beds of tuff and agglomerate really are around the great centres. Of course, such deposits are not so conspicuous as they would be if not covered by taluses of fallen blocks from overlying lavas; and they have, moreover, often suffered greatly from denudation,—*first*, from their greater softness, and *secondly*, from their having in many cases occupied the highest elevations. Enough of them still remain, however, to indicate their character and illustrate their former extent.

Thirdly. It is said that there is no proof of a communication having existed between the great intrusive sheets and dykes of gabbro and dolerite, and the currents of basalt that were poured out at the surface.

Here, too, I must point out that I have never asserted that any such connexion can be *directly* traced. Following Darwin and Jukes, I argued that if the source of an obsidian lava-current could be traced down sufficiently far into the bowels of the earth, every gradation from glass to granite *might be* observed. But in the case of the Western Isles of Scotland, I stated that the proofs of the connexion between the Plutonic rocks and the lavas, necessarily inferential, were as follows:—

The intrusive Plutonic masses, "in their ultimate chemical com-

position, coincide perfectly with the rocks composing the lavas ; but in the varieties of their texture and mineralogical constitution they exhibit a much wider range. Thus while we find veins of basalt in which the rock-structure is identical with that of many of the lavas, we find also others in which the rock passes into a glass—tachylyte ; while others, again, are composed of the highly crystalline, or granite-gabbro-rocks. Similarly, felstone-veins are related to those of pitchstone and obsidian on the one hand, and to masses of felsite, syenite-granite, and granite on the other.” (Q. J. G. S. 1874, p. 238.)

Not only did I refrain from asserting that an intrusive mass of gabbro and dolerite could be traced passing into the basaltic lava-current, of which it formed the filled-up duct leading to the surface, but in my diagrammatic sections drawn to illustrate what I supposed to be the real interpretation of the phenomena, I represented the supposed connexion between the great, nearly horizontal sheets of intrusive gabbro and dolerite and the superficial lavas by *dotted lines* (see Q. J. G. S. 1874, pl. xxiii. fig. 3).

The fact is, I perceived then, as I do now, that only by the most extraordinary series of accidents could we expect a mass which has consolidated at such a depth as to form a gabbro, to have its connexion with the lava-current that reached the surface preserved in such a way as to permit of the change being followed step by step.

How fully my critic has adopted, and how clearly he has expressed the views which I put forth concerning the actual transitions from gabbro into dolerite, from dolerite into basalt, and from basalt into tachylyte, I have already pointed out. He states quite truly that in the gabbro-bosses “there are evidences of successive discharges or extravasations of crystalline materials, during a probably protracted period of time, and in the intricate network of veins crossing each other, and the general body of the rock in every direction,” . . . . (the geologist) “will recognize the repeated renewals of subterranean energy.” (T. R. S. E., 1888, p. 130.)

It will thus be seen that we both believe that the basalts of the plateaux and the gabbro-masses around the five great centres were alike formed by a number of successive manifestations of the volcanic forces extending over enormous periods of time. We are at one in the opinion that in chemical composition, and in mineralogical constitution, the gabbros and the basalts show the most striking analogies ; while, in texture, the holocrystalline rocks exhibit remarkable transitions into the hypocrySTALLINE ones. But while I have argued that the same series of volcanic throes which brought the basalts to the surface, may have produced *at the same time* the vertical and horizontal fissures, and injected them with the materials of the gabbro, my critic asserts that the gabbro-injections *belong to a distinct and later period* than the outflow of the basalts.

It is scarcely necessary to remark that the explanation I have given is by no means inconsistent with the view that many of the

younger of the gabbro-bosses may be of later date than the oldest of the basalt-flows—those, namely, which have escaped denudation. We are equally agreed as to the enormous periods occupied by the eruption of the basaltic lavas, and as to the vast amount of denudation which they have since suffered.

V. THE ALTERNATIVE THEORY OF “FISSURE-ERUPTIONS.”

We have now, I think, dealt with the arguments which Dr. Geikie brings forward as tending to throw doubt on the theory of the origin of the volcanic rocks of the Western Isles of Scotland, which was proposed by me in 1874. So far as I can understand them, the observations made by the Director-General and the officers of the Geological Survey—especially as to the order of appearance of the several varieties of igneous rock in the area in question, and in the closely related districts of Antrim and Carlingford—are quite capable of being completely reconciled with the views I have propounded. But Dr. Geikie has, in 1880, proposed a rival theory*; and to this theory, in support of which a number of arguments are now adduced, we must proceed to direct attention.

A. *Can great Basaltic Plateaux be formed by ordinary Volcanic Action?*

It may be well at the outset to consider an *à priori* objection which has been frequently raised,—in which doubt is thrown upon the possibility of thick plateaux of basaltic lava, covering many thousands of square miles, having been poured out from ordinary volcanic vents.

Now, in seeking to explain the volcanic phenomena of the Hebrides and the north of Ireland, no illustrations drawn from modern examples of eruptive activity are likely to be of greater value and appropriateness than those derived from the area which comprises the Faroe Isles and Iceland, and which stretches away northward into Greenland. As I have already shown, those districts form parts of a great petrographical province, and the various types of lava poured out at successive periods are wonderfully similar throughout the whole of the vast area. For some reason, not yet explained, however, the eruptive activity, which at the central part of the area (Iceland) is still in full vigour, appears to have died out, probably in the Pliocene period, both in the northern part of the province (Greenland) and at its southern extremity (the British Isles).

I know of no valid grounds whatever for doubting that the great plateaux of basaltic lava which cover so large an area in Iceland have been poured out by a long succession of outflows from ordinary volcanic vents. Within the comparatively short period covered by Icelandic history, we have the example of the eruption of 1783 at Varmárdalur (usually spoken of as the Skaptár Jökull eruption). From the carefully prepared accounts of this eruption, drawn up by

* ‘Nature,’ vol. xxiii. p. 3 (November 4, 1880).

a Danish Commission appointed for the purpose, and which have been confirmed by the observations of many travellers who have since visited the district, we learn that two streams of very liquid basaltic lava having lengths of from 40 to 50 miles, with breadths varying from 7 to 15 miles, and an average depth of 100 feet, were poured out during this eruption. Professor Bischoff has made a very remarkable estimate, which shows that the quantity of material brought from within the earth's crust and spread over the surface in this single outburst exceeds the bulk of Mont Blanc*.

It is scarcely necessary to point out that a succession of flows of such magnitude as that of Skaptár Jökull, continued through the long periods of time covered by the Tertiary epoch, would be amply sufficient to produce the great basaltic plateaux of Iceland, without calling in the aid of any sudden and overwhelming extravasation of lava, to which we can find nothing approaching a parallel among the volcanic phenomena of the present time.

It is only fair to Dr. Geikie to point out that his own observations, and those of other members of the Geological Survey, are clearly shown to support the conclusion that the plateaux of basalt in Antrim and the Western Isles are built up of a great number of separate lava-streams, some of a volume comparable to the lava-sheets of "Skaptár Jökull," others of much smaller bulk, with not a few of quite insignificant dimensions.

After pointing out that the thickness of the individual lava-sheets in Antrim and the Scottish Isles varies from 60 or 70 feet to only 6 or 8 feet, Dr. Geikie goes on to say:—

"Each bed appears, on a cursory inspection, to retain its average thickness, and to be continuous for a long distance. But I believe that this persistence is in a great measure deceptive. It is not often that we can follow the same bed with absolutely unbroken continuity for more than a mile or two. Even in the most favourable conditions, such as are afforded by a bare sea-cliff on which every bed can be seen, there occur small faults, gullies where the rocks are for the time concealed, slopes of débris, and other failures of continuity; while the rocks are generally so like each other that on the further side of any such interruption it is not always possible to make sure that we are still tracing the same bed of basalt which we may have been previously following. On the other hand, a careful examination of one of these great natural sections will usually supply us with proofs that, while the bedded character may continue well marked, the individual beds die out, and are replaced by others of similar character. On the south coast of Mull, for instance, cases may be observed where the basalt of one sheet abruptly wedges out

* If it should be asserted, as has sometimes been done, that the Icelandic eruptions are themselves not ordinary volcanic outbursts, but "fissure-eruptions," it is only necessary to refer to the accounts given of the "Skaptár Jökull" out-break by the Danish Commission and subsequent authors. The explosive action was so violent as to produce phenomena quite comparable to those occurring at Krakatoa in 1883, and lines of cinder-cones were thrown up, just as in the eruptions which take place on the flanks of Etna,

and is replaced by that of another. Where both are of the same variety of rock, it requires close inspection to make out the difference between them; but where one is a green, dull, earthy, amorphous amygdaloid, and the other is a compact, black, prismatic basalt, the contrast between the two beds can be recognized from a distance. Again, along the west coast of Skye, the really lenticular character of the beds can be well seen." (T. R. S. E. 1888, pp. 80, 81.)

The author goes on to show that the detailed study of the lavas of Antrim, where the Geological Survey has been completed, strongly supports the same views.

In reply to those who believe that such basaltic plateaux as those of Antrim, consisting of successive lava-sheets, lying almost horizontally, and piled upon one another to a very great thickness, could not possibly have been poured out from volcanoes, I cannot do better than point to the example of the Sandwich Islands. And very opportunely we have just at the present time new and important light thrown upon that interesting district by the valuable studies of Captain Dutton*, and the still more striking observations recently made by the veteran Professor J. D. Dana†. Geologists all over the world may well hesitate as to which calls for the greater admiration—the vigour and enthusiasm which prompted our esteemed Foreign Member to undertake a journey of ten thousand miles in order to clear up difficulties left in his original studies of the district in 1840—or the new and valuable results obtained by the investigations of one who, after nearly half a century of thought and inquiry, has returned to the field of his early observations.

The island of Hawaii, taking only that portion of the mass which is at present above the sea-level, has an area almost identical with that of the basaltic plateau of Antrim, and the rocks composing the island, *though undivided*, form a great mass, the slopes of which seldom, if ever, exceed a few degrees. The individual lava-streams, like those of Antrim, are of the most varied bulk and, like them, also lie almost horizontally; in many parts only insignificant beds of tuff or ash can be detected between the different lava-sheets. Yet there cannot be the smallest doubt that this plateau of basalt has been formed by ejection from the three great volcanoes—Mauna Loa, Mauna Kea, and Hualalai. Either from the summit-craters of those volcanoes or from the numerous parasitical vents that have been opened upon their flanks have the successive lava-currents,—sometimes small and insignificant, at other times forming vast floods of molten rock—been poured out.

The examination of the Antrim-plateau, which has been carried on during the last twenty-five years by different officers of the Geological Survey, has shown that too much importance must not be attached to the, at present, almost horizontal position of the several lava-sheets of which it is composed, or to the inclinations which they can now be shown to exhibit. The author of the recent memoir states that "the varying dip of the beds must be attributed

* Fourth Rep. U. S. Geol. Surv. pp. 81-219.

† Ann. Journ. Sci. 3rd ser. vols. xxxiii.-xxxvii.

mainly to post-volcanic movements, or at least to movements which, if not later than all the phases of volcanic action, must have succeeded the outpouring of the plateau-basalts" (*loc. cit.* p. 89). I have again and again insisted upon the same fact in the case of the basaltic plateaux of the Western Isles of Scotland, and have especially dwelt upon the dip of the lava-sheets towards the central vents (Q. J. G. S. 1874, pp. 256-258.), and the correctness of these observations the author of the recent memoir appears not only to fully admit, but also to adopt, for he says:—"There appears also to have been a general tendency to sagging subsequent to the gabbro protrusions, and the inward dip thereby produced has probably been instrumental in effacing at least the more gentle outward inclinations caused by the uprise of the eruptive rock." (T. R. S. E. 1888, p. 142.)

B. The Age of the Basic Dykes of North Britain.

The author of the recent memoir, in seeking to give an explanation of the volcanic phenomena of the Western Isles of Scotland, begins by laying much stress on the great importance of the numerous dykes which traverse the central and southern districts of Scotland and the North of England. For the sweeping generalization which he announces, however, that those dykes or, at all events, the great majority of them, were formed during the Tertiary period, I confess I can find no evidence. That the great mass of the basaltic dykes in the Western Isles of Scotland, and in Antrim, many of which can be shown to radiate from the great centres of eruption, are of this age, I fully admit; but when the numerous dykes of the Central Valley of Scotland, of Fifeshire, of Forfarshire, the Southern Highlands, the Borderland, and the northern counties of England are all claimed as fissures from which ejections took place in Tertiary times, it would have been satisfactory to have had the grounds of such a very startling conclusion a little more clearly stated. Such field-evidence as is available only suffices to prove that while some are later than Silurian or Ordovician strata, others are younger than Old-Red-Sandstone or Carboniferous times.

Two sets of facts on the other hand make me think this sweeping generalization a very doubtful one indeed. Mr. Allport's valuable researches concerning the Carboniferous Dolerites showed how remarkably fresh are some of the basic rocks of this age; and his studies also demonstrated the fact that the materials filling many of the dykes are perfectly similar to some of the lavas which were undoubtedly erupted during the Carboniferous period in the South of Scotland. The author of the memoir we are now considering, it is true, at one time maintained that at Arthur's Seat (where some of the doleritic lavas of freshest appearance are found) there was evidence of eruption having occurred in the Tertiary period; but this conclusion he has long since abandoned. I see no ground myself for doubting that many, and perhaps the great majority, of the basaltic dykes of the Southern Highlands, of the Central Valley of Scotland, of the Border-

land, and in the North and West of England are really of Newer Palæozoic age, and are connected with the ejection of the very similar lavas that were erupted during that period.

That a few dykes in the North of England which intersect the Mesozoic strata are probably of Tertiary age, has long been very generally admitted; but, as Mr. Teall has so well shown, the Tertiary dykes consist of very characteristic materials (augite-andesites or "tholeites" of Rosenbusch), while other truly basaltic dykes in the same district appear certainly to be of far older date.

That certain dykes, like that of Eskdalemuir, which cannot be proved to intersect strata younger than Palæozoic, are probably also of Tertiary age, is highly probable, from the similarity of their materials to those of the Cleveland and other late dykes; but in the large and valuable series of facts brought forward by the author of the recent memoir—in which he summarizes the results of the labours of the Geological Survey in the North of England, in Western Scotland, and in Ireland—I fail to find any evidence based either on the petrographical character of the dykes, the directions in which they run, or any other peculiarities, that we can safely discriminate between the Newer Palæozoic dykes and those which are of Tertiary age. Still less can I find any evidence for the confident assertion that the great majority of the basic dykes over such a wide area must all be of Tertiary age.

It is an interesting fact, to which I called attention in my original memoir, and the full evidence for which I hope shortly to lay before this Society, that the materials ejected during the latest eruptions in the West of Scotland are augite-andesites ("tholeites" of Rosenbusch), presenting the most striking similarity to the rocks of the undoubtedly later dykes in the North of England, described by Mr. Teall*. It is on this ground that I continue to maintain that these dykes represent the long radiating fissures which were produced after the great central vent became extinct, and that in many cases they give rise to lines of cinder-cones with issuing lava-streams, similar to the well-known "pays" of the Auvergne (Q. J. G. S. 1874, pp. 260-267).

C. Insufficiency of these Dyke-fissures as Sources of the Plateau-basalts.

Even if we were to grant the contention that a large proportion of the North British basic dykes are really of Tertiary age, I believe that the fissures occupied by those dykes are quite inadequate to have served as the vents from which such enormous masses as the basaltic lavas of the plateaux could have been outpoured.

I am happy to find that the author of the recent memoir is quite at one with myself in regarding the existing patches of these basaltic lavas as mere vestiges, which have escaped denudation, of masses originally having a far greater thickness and covering a much wider

* Quart. Journ. Geol. Soc. vol. xl. (1884), p. 209.

area. Where only a few scattered islets now remain, we both believe that extended and thick plateaux of basalt must have once spread over the whole intervening area. I have maintained that, away from the great central vents of the district, these basaltic lavas formed masses which were, in places, 2000 feet in thickness, but that, around the vents, they attained to a far greater depth. My critic thinks that even where there were no great vents, the plateaux were as much as 3000 feet in thickness! (T. R. S. E. 1888, pp. 91, 99; compare Q. J. G. S. 1874, pp. 244, 255, &c).

Now even if we accept a large proportion of the basic dykes as being of Tertiary age, we must take into consideration several very important facts.

First.—Many of these dykes, for large portions of their course, do not, even at the present day, reach the surface at all. This fact was long ago pointed out by Winch, and has been confirmed by the observations of the Geological Surveyors and of Mr. Teall. The recent admirable studies carried on by Mr. G. Barrow in the North of Yorkshire have demonstrated the frequency of this upward dying-out of the Cleveland-dyke over considerable distances*. But the present surface of the country must be many hundreds or thousands of feet below that which existed in early Tertiary times; and if we could restore the vast masses of rock removed by denudation, the abrupt upward termination of the dykes would doubtless be found to be of much more frequent occurrence; and it is of course only where the dykes actually reached the old surface early in the Tertiary period that they could have served as lava-conduits.

Secondly.—While a few of these dykes are 50 feet or even more in breadth, the great majority of them are of far smaller dimensions, many of them, indeed, being quite insignificant; and it has always appeared to me difficult to believe that such enormous floods of lava could have been erupted through channels so narrow.

Thirdly.—If it be argued that even comparatively narrow fissures might have given rise to enormous outflows of lava, provided they were of the nature of habitual vents, then a fresh difficulty has to be faced. In such cases we might surely expect that the long-continued flow of molten materials through the same channel would have given rise to the most striking and profound contact-metamorphism on the sides of the fissure. But this is what we never find to be the case; as the author of the recent memoir justly remarks, "evidence of any serious amount of alteration is singularly scarce, a slight induration of the rocks on either side of a dyke is generally all that can be detected" (*loc. cit.* pp. 62, 63).

It was on these and similar grounds that I long ago rejected the suggestion so very frequently advanced by some of the older geologists, and which has been revived at various times by my critic, that the plateau-basalts or "trap-rocks" are masses of igneous material which have come up through the dyke-fissures and overflowed the surface. In the case of the Western Isles of Scotland, at all events,

* The Geology of North Cleveland (1888), pp. 60-64.

I have shown that it is not necessary to resort to such a theory of the origin of "trap-rocks."

D. Arguments drawn from the Lava-fields of the Western Territories of the United States.

In the year 1876 the author of the memoir which we are now considering published a Geological Map of Scotland, in which he, for the first time, publicly renounced the view that the gabbros of the Western Isles are of metamorphic origin and of Laurentian age. The relations of the igneous rocks to one another, as indicated upon this map, were precisely those which I had announced two years before.

But in 1879 the same author, as he himself informs us, undertook an excursion to the Yellowstone Park, and, while on his journey to that famous locality, observed facts which profoundly modified his views concerning the mode of origin of the igneous rocks of his own country. Riding across the plains of the Snake River, he was struck by the absence of volcanic cones in a wide area covered by basaltic lavas; and he came to the conclusion that the lavas must have reached the surface by means of fissures opened in the subjacent rocks.

Now I cannot help remarking, at the outset, that this generalization, in the case of the lavas of the Snake-River district, appears to have been a somewhat hasty one. The opportunities for careful observation during a rapid ride of this kind must have been necessarily few; and, as Darwin pointed out long ago, the most careful observer is very prone to ascribe to a single outflow of lava the materials which were actually accumulated by a number of distinct ejections. As Darwin well showed, in his 'Volcanic Islands,' it is often impossible to detect the limits of the different, comparatively fresh flows, except by studying the age of the vegetation which has sprung up on each of them.

The statement that the basalts of the Snake River welled out in floods from fissures in the subjacent rocks, is admitted to be purely conjectural; and several of the observations which the author of the statement has made with regard to the district appear to me to be quite irreconcilable with his theory. He admits that cinder-cones were seen here and there during his rapid ride; and when we remember how liable such cinder-cones are to be swept away, firstly by the outwelling of lava from their craters, and secondly by the ordinary agents of subaerial denudation, this fact is a sufficiently significant one. That a considerable amount of denudation has taken place in the Snake-River district is shown by the circumstance that it presents, according to the author's own description, "low hummocks or ridges of bare black basalt, the surfaces of which exhibited a reticulated pavement of the ends of columns"*. These appearances, which are illustrated by a drawing, are scarcely consistent with quite fresh lava-streams, but are exactly what is met

* See Text-Book of Geology (1882), p. 257.

with when the scoriaceous surfaces and upper layers of streams of lava have been removed by denudation. But if denudation has done so much work on the solid lava-currents, what may we not expect it to have effected in the case of cones composed of loose scorïæ?

But other observers who have visited the district, and had far greater time and opportunity for studying the phenomena, have come to very different conclusions from those which my critic announces.

Captain Reynolds, one of the earliest explorers of the Snake-River district, examined the country in 1868, and was accompanied by Dr. Hayden, who described volcanic agglomerates as by no means wanting in it*.

Dr. Hayden, visiting the district again in 1872, found sections that proved the basalts not to be massive floods of lava, but thin cappings overlying beds of trachytic tuff and other rocks, and he describes cones, one of them 500 feet in height, from which these lavas seemed to have issued. His descriptions forcibly remind us of some portions of the Auvergne†.

At a subsequent date Mr. Clarence King described the basalts of the Snake River as being well exposed in the Snake Cañon, where he found them to consist of thin flows of basalt, superposed one upon the other; and the evidence found in the Cañon is said to distinctly negative the idea that the lava could have flowed from a plexus of dykes, inasmuch as such dykes are few in number, and sometimes none of them are found for very great distances‡.

But in his recent memoir the advocate of "fissure-eruptions" has sought to extend the generalization at which he arrived from an inspection of the Snake-River basalts to other areas of the Western States of America; and first among these fresh examples he places the basalts of the Uinkaret Plateau described by Captain Dutton in his admirable work 'The Tertiary History of the Grand Cañon District.'

Captain Dutton's clear descriptions and Mr. W. H. Holmes's beautiful drawings of the scenery of the district give to every geologist the opportunity of judging for himself how far the hypothesis of fissure-eruptions is supported by the district referred to. We are told that from one point of view alone the observer may see "120 to 730 distinct cinder-cones, and that there are many others that will escape detection." Many of these are said to be quite small, but others are from 700 to 800 feet in height and are a mile in diameter§.

The maps|| of the district and the beautiful drawings of Mr. Holmes fully confirm this description. The panorama seen from Mount Trumbull, where we observe stratiform rocks capped by basalts and cut through by streams with later currents of lava issuing from cinder-

* See Captain Reynolds's Report, published in 1868 and 1869, chapter x. pp. 85, &c.; quoted in U. S. Geol. Surv. of Territories, 1872.

† *Ibid.* for 1877.

‡ U. S. Geol. Explor. of Fortieth Parallel, vol. i.; Systematic Geology pp. 672-3.

§ Tertiary History of the Grand Cañon District (1882), pp. 104-112.

|| Atlas to accompany the above; see plates vii., viii., ix. and x.

cones, so exactly reproduces the features illustrated by Mr. Scrope in his well-known panoramic views of the Auvergne, that it seems difficult to understand how any geologist could maintain totally distinct modes of origin for these features in the two cases.

Captain Dutton's remarks on the High Plateaux of Utah and the lava-fields of New Mexico are also quoted as giving still further support to the theory of "fissure-eruptions."

A reference to these writings of Captain Dutton, however, shows that cones and craters are by no means absent in the districts referred to; they are, indeed, said to be very numerous in some parts of the areas, though usually of small size. Moreover the lava-fields are stated in some cases to have suffered so greatly from denudation that it would be a hopeless task to look for cinder-cones upon them.

But there is one very important circumstance which seems to have been unfortunately overlooked by my critic. Captain Dutton, after studying the lava-fields of the Western Territories, and showing some disposition to refer them to "fissure eruptions," paid a visit to the Sandwich Islands, and there—amid the abundant proofs afforded to him of the fact that from the summit and sides of true volcanoes highly liquid basaltic lavas are often poured out, with but a very slight accompaniment of explosive action—he very candidly confessed that he felt he might have been in error in invoking the aid of the "fissure-eruption" hypothesis to account for the phenomena witnessed by him in his own country. Captain Dutton's remarks are so much to the point that I cannot do better than quote them.

Referring to the lava poured out from Mauna Loa in 1855, Captain Dutton remarks:—

"As I looked over this expanse of lava, I was forcibly reminded of some of the great volcanic fields of the western portion of the United States, where the eruptions are of such colossal proportions that they have received the name of massive eruptions. Richthofen, after studying many of these lava-fields in California and Nevada, was led to the conclusion that they had burst forth from great fissures, inundating large areas of country with fiery seas of basalt. He was led to contrast the immense volume of these rocks with the comparatively insignificant streams which have emanated from Vesuvius, *Ætna* and other modern volcanoes, and concluded that the incomparably grander overflows of Western America must have occurred under circumstances differing widely from those of ordinary volcanic eruptions. Although the volcanic rocks of Western America may be considered as very well exposed, as compared with rocks of equal antiquity in other portions of the world, they would be regarded as relatively obscure by any one who has had an opportunity to inspect carefully the recent lavas of Mauna Loa. I am by no means certain that Richthofen's conclusions are wrong. But here is a lava-flow, the dimensions of which fully rival some of the grand pliocene outbreaks of the west, which demonstrably differs in no material respect, excepting in grandeur, from the much smaller eruptions of normal volcanoes"*.

* Fourth Rep. U. S. Geol. Surv. p. 156.

In several other passages of the same Report Captain Dutton clearly intimates that his belief in "fissure-eruptions" was most seriously shaken by what he saw in the Sandwich Islands.

VI. CONCLUSION.

But even if it were conclusively demonstrated that the basalts of the Snake-River area, of the Uinkaret plateau, of the Highlands of Utah, and of the plains of New Mexico, could only have originated in "fissure-eruptions," I fail to see how this affects the question of the existence of great volcanoes in the Western Isles of Scotland. It would seem that districts which have only been hastily traversed are scarcely likely to afford evidence that can be placed in comparison with the clear and unmistakable indications found in a district which has been made the scene of careful and searching investigation.

It is very difficult to understand what idea the advocates of "fissure-eruptions" wish to convey by their favourite phrase. If by using it they intend to imply that volcanic action takes place along lines of fissure, I know of no volcanologist who has ever denied it. In that sense *all* volcanic outbursts are "fissure-eruptions." If it be meant to indicate that the outflow of lava from a vent may take place with little or no explosive action, this is equally a part of the creed of all volcanologists.

But if, on the other hand, it be intended to assert that, either during the Tertiary or any earlier geological period, there is evidence that lavas were extruded upon the earth's surface under wholly different conditions from those which prevail in the volcanic areas of the present day, this is a proposition which I utterly deny:—and I challenge the supporters of "fissure-eruptions" to bring forward a particle of evidence in its support*.

The study of the Hawaiian volcanoes has long ago shown how remarkably the effusive action, resulting in the outwelling of successive sheets of lava, may preponderate over explosive action at certain volcanic vents.

Captain Dutton very justly remarks that "the long and gentle slopes of Mauna Loa are merely the surface of a mass of lavas which have been piled over each other in the form of lava-streams, poured out at intervals throughout an epoch of vast but unknown duration. These great lava-floods burst out seemingly in a most capricious manner, here there and everywhere. They break out far more frequently at or near the summit than upon the lower flanks of the mountain, and so vast is the amount of lava outpoured at each eruption, that the streams often reach literally from the summit to

[* My difficulty in understanding what is meant by a "fissure-eruption" has been only increased by the explanations offered after the reading of the present memoir (see page 219). It is said that the lavas have not flowed out "directly from long open fissures, but that vents were established on such fissures, sometimes of considerable size, with the usual accompaniment of volcanic eruptions." I presume, then, that when an outburst occurs at the summit-crater of Etna, it must be called a *volcanic* eruption, but when it takes place on the flanks of the mountain, like that of 1874, so well described by Silvestri, it is a *fissure-eruption*!]

the sea" [that is distances of from 30 to 40 miles!], "spreading out from a quarter of a mile to two or three miles in width. Upon such a broad surface as that of Manua Loa it must necessarily happen that some portions may lie for centuries unscathed by fire, and during this period of immunity the lavas decay, soil is formed upon them and accumulates to the depth of many feet"*..

The facts which I observed in the Western Isles of Scotland led me, in 1874, to state in very similar terms the real nature of the basaltic plateaux of that district. I wrote as follows:—

"The whole of these facts point to the conclusion that, during the period of the emission of the great lava-floods which form the enormous plateaux of the Hebrides and Antrim, the surfaces over which they flowed were above the sea-level, and, further, that intervals of sufficient duration occurred between the outpourings of the lava-streams to admit of the formation of those very interesting intercalated deposits, which are in every case of a terrestrial, fluvial, or lacustrine origin"†.

The study of the Hawaiian volcanoes by Captain Dutton, and especially by Professor Dana, has thrown much new light upon certain phases of volcanic activity; but the formation of great plateaux built up by successive outpourings of lava of basaltic composition and of great liquidity had been long before fully recognized by Mr. Scrope and other vulcanologists.

I have no wish to suggest that the great volcanoes of the Western Isles of Scotland were of the same type as those of the Sandwich Islands. On the contrary, I believe that there is ample evidence to show that, while the great basaltic plateaux which surround those volcanoes were built up by successive outflows of very liquid lava from their summits and flanks, yet a considerable amount of explosive action, indicated by many relics of great masses of tuffs, took place both at the central craters and from the parasitical vents.

The eruption of the basaltic lavas was preceded, and to some extent accompanied by the extrusion of lavas of more acid composition—andesites, dacites, and rhyolites; but these lavas, as I showed, were generally wanting in the great liquidity which characterized the basaltic extrusions, and so are, for the most part, found confined to the immediate vicinity of the great eruptive centres.

During fifteen years I have devoted my attention to the study of the varied products of these great volcanoes of the Western Isles of Scotland, and I have again and again revisited them to clear up difficulties as they arose in the course of those studies. Many errors in points of detail I am prepared to admit, but of the truth of the main propositions enunciated in my memoir I have become more and more convinced, the longer I have studied the subject.

If such large quantities of volcanic material, both of acid and basic composition, were transferred from the interior of the earth's crust to the surface, then surely we ought to find, in a greatly denuded area, the traces of those conduits and channels by which the igneous

* Fourth Report U. S. Geol. Survey, p. 93.

† Quart. Journ. Geol. Soc. vol. xxx. (1874), p. 230.

materials found their way from below. The materials filling these conduits, I argued, would have consolidated slowly and under great pressure, and I therefore inferred that while agreeing in ultimate chemical composition with the lavas, they would exhibit a more highly crystalline structure. I then showed that in the great bosses, sheets, and dykes composed of gabbro and granite we have just such masses of rock—occupying precisely the positions they might be expected to do, if the interpretation which I gave of the structure of the country was the true one. On the other hand, I maintain that the long narrow dykes are altogether inadequate as the sole channels for the extrusion of these enormous volumes of lava.

The hypothesis of “fissure-eruption” appears to me now, as it has always done*, as unnecessary as it is vague. Effusive action, the welling-out of great masses of lava, is as much a characteristic of volcanoes as is the dispersal of scorïæ, lapilli, and dust by explosive action. In the Sandwich Islands we have an admirable example of the formation of plateaux built up of basaltic lava-streams, with only small and rare intercalations of volcanic tuffs. In the great tuff-cones of Java and their remarkably explosive outbursts we have the other extreme type of volcanic activity admirably exemplified. The study of the recent tremendous catastrophe at Krakatoa was particularly interesting, as affording to us a striking instance of the explosive type of volcanic activity, so strongly contrasted with the effusive type, as illustrated in the Sandwich Isles. But I am quite at a loss to understand why the explosive action that has produced the great tuff-cones of Java should be regarded as “ordinary volcanic activity,” while the effusive action which has given rise to the basaltic plateaux of the Sandwich Islands should be denied that title.

Professor Dana, after very clearly and forcibly pointing out the distinction between the two types of volcanic activity, very justly adds:—“The marked contrast between volcanoes of the Mount Loa and Vesuvius types, based on the liquidity of the lava, making Mt. Loa discharges to be almost solely outflows, and those of Vesuvius both upthrows of cinders and outflows of lava, has been sufficiently explained. With this exception, the contrast as to their eruptions as well as to their ordinary action is far less than is generally supposed. There is no reason to regard the forces as different in kind or mode of action”†.

The volcanic outbursts of Iceland in recent times—and there is every reason for believing that those of the British Islands during the Tertiary periods were precisely similar to them—combine in a most striking and instructive manner the explosive and the effusive types of activity. The eruption of Varmadalr (Skaptár Jökull) in 1783 was not more remarkable for the enormous volume and great liquidity of the lavas, than for the violently explosive action which accompanied their extrusion—these explosions producing effects quite comparable to those which resulted from the Krakatoa outburst, just a century later; the Icelandic eruption moreover resulted in the

* Volcanoes, what they are and what they teach (1881), p. 188.

† Amer. Journ. Sci. ser. 3, vol. xxxvi. pp. 172, 173.

formation of lines of scoria-cones similar to those so constantly formed when an eruption takes place on the flanks of Etna.

It is very necessary that the true nature of the phenomena displayed at Krakatoa and at Mauna Loa respectively should be correctly appreciated, and that the manner in which, at all volcanic centres, the two kinds of activity—of which these are the extreme types—may be found in ever varying combinations should be clearly understood. When this result is arrived at, I am convinced that the hypothesis of “fissure-eruptions” will soon sink into that same oblivion which has already overtaken its predecessor and parent “the theory of the trap-rocks.”

DISCUSSION.

Dr. GEIKIE, alluding to the fact that his memoir, which had been reviewed by Professor Judd, had probably not yet been seen by many of those present, who were therefore unacquainted with the mass of detailed observations on which his conclusions were based, proceeded to give a history of his researches in the volcanic rocks of the West of Scotland. He had first broken ground among them more than 30 years before, and after publishing various papers on the subject, had presented to the Society in 1871 what he intended to be the first of a series of papers on the Tertiary volcanic rocks of the British Isles. Before the second paper of the series was ready, Professor Judd entered the field, and by his paper of 1874 covered most of the ground that he himself had been working over. He laid aside the subject for a few years, but an excursion into Western America in 1879 gave him new insight into the volcanic history of Western Scotland, and he then resumed his researches, and continued them until he was able to publish, last May, the large memoir which had been criticized by Professor Judd*. He had not been able to confirm his critic's conclusion as to the order of succession among the volcanic rocks, which he found to have been the reverse of what had been stated. But with a strong dislike of controversy, he had refrained from emphasizing the differences of opinion between Professor Judd and himself, and after stating generally his disagreement, had abstained from pointing out what he believed to be serious errors of observation, and preferred to quote from the paper of 1874 when he agreed with it. He wished emphatically to declare that his Memoir was not official work of the Geological Survey, but that for any demerits it might contain he was himself solely responsible. Exhibiting on the wall an enlarged copy of Professor Judd's map of Mull, he showed also a rough enlargement of his own map of the same island, which, out of deference to Professor Judd, whose work it contradicted, he had refrained from publishing in his paper, but which he now felt compelled to produce. He pointed out what he considered to be the grave errors in the former map.

There was in reality no central core of granite with a ring of

* Trans. Roy. Soc. Edinb. vol. xxxv. pp. 21-184.

felstone and masses of acid breccias and tuffs, as laid down by Prof. Judd. The acid rocks were confined to a few bosses and hundreds of dykes and veins, and instead of forming the oldest masses of the volcanic series, as, trusting to the map of 1874, he had believed to be the case, they were actually the youngest, and could be seen in innumerable localities, sending veins into them. He had been unable to discover any trace of felstones which had reached the surface before the outflow of the basalts. The latter he had everywhere, through Mull, Rum, Eigg, and Skye, found to be the oldest lavas. Next in age came the gabbros which had been injected into the already solidified basalts, andesites, &c. of the plateaux; and lastly the granophyres and other acid rocks were erupted through the whole, except the youngest basic dykes.

With regard to the criticism which had been offered as to "fissure-eruptions," he thought that some misapprehension existed. He did not suppose that the lavas flowed out directly from long open fissures, but that vents were established on such fissures, sometimes of considerable size, with the usual accompaniments of volcanic eruptions. He contended that there was not the slightest evidence of any great central volcano of the type of Vesuvius or Etna in any part of the Western Islands, but that the vents had been numerous, widely scattered, and, on the whole, of small size.

It would be impossible to discuss all the details of Prof. Judd's paper, and if he had not replied to all the points now raised it was not from inability to do so. He entered into such discussions with much reluctance, and though he regretted having to differ so greatly from Prof. Judd, he felt himself entitled to make known the conclusions to which the researches of so long a series of years had finally conducted him.

The AUTHOR agreed with Dr. Geikie on one point, namely, reluctance to enter upon this controversy, which had been forced upon him. It was perfectly true that Dr. Geikie had published many papers on the district in question before his own appeared in 1874; but the views maintained in all these papers were utterly irreconcilable with those which were enunciated by himself, and which were now adopted by Dr. Geikie. The key to his own explanation of the structure of the district was the recognition of the intimate relations between the gabbros and the basalts; while Dr. Geikie had always held that the former were of metamorphic origin and of Laurentian age, and the latter were regarded by him at first as Jurassic, and afterwards as Tertiary lavas.

The sketch-map of Mull, as he had stated when he published it, was necessarily diagrammatic, inasmuch as the district was one of great complexity, of which no reliable topographical maps were at the time in existence. But he maintained its general accuracy; the contrasts between it and the map exhibited by Dr. Geikie were largely due to differences in the system of rock-nomenclature and the scheme of colouring employed in the two cases.