

From good data, Professor H. A. Newton, of Yale College, has calculated that they proceed from an elliptical ring, which has a period of revolution of 281 days, and through which the earth passes about this time, occupying three or four days, showing that this belt must be several millions of miles thick, and must contain, at a moderate calculation, more than three hundred millions of small meteoric bodies.

Now the earth at this time is advancing through space at the rate of about two millions of miles a day, and the bodies of this ring, having a retrograde motion, enter our atmosphere with immense velocity. The ordinary height of these luminous meteors is from fifty to seventy miles, and the rare atmosphere at that height opposes sufficient resistance to these rapidly moving bodies to heat them to whiteness, and even convert them into vapour. The latent heat of a given bulk of this rare atmosphere is as great as that of the same bulk of more dense air near the earth, and calculations show that a meteor, moving at a rate of only ten miles a second, or less than one-half the ordinary velocity of these bodies, at a height of thirty-four miles, would in one second's time evolve heat enough to make its mass white hot. The real luminous appearance comes, however, from the atmosphere condensed before the moving body, and from the matter of this converted into gas by the intense heat. If not already dispelled in vapour, these bodies, on reaching the lower region of the air, cease to be luminous from the very density of the atmosphere.—*Montreal Daily News and Gazette.*

CORRESPONDENCE.

ON THE ALLEGED HYDROTHERMAL ORIGIN OF CERTAIN GRANITES AND METAMORPHIC ROCKS.

To the Editor of the GEOLOGICAL MAGAZINE.

DEAR SIR.—The letter of Mr. James Geikie which appeared in your last number obliges me to send a few lines in reply.

I may premise by stating that my communication, "On the alleged hydrothermal origin of certain granites and metamorphic rocks," owed its appearance in print, solely to the fact of the Memoirs therein referred to, having emanated from the pen of a member of the Geological Survey of Great Britain, and it was the official position of the writer which alone caused his productions to be submitted to the severe, but just, criticism which therein appeared.

Mr. James Geikie, in his reply, takes me to task for so confounding him with the Survey; independent of his being actually an officer of the Survey, it will be seen, upon reference to his own memoir,¹ that after stating that the Geological Survey (represented by himself and his colleagues, Dr. Young, and Mr. A. Geikie) was in 1865 extended to the district in question; Mr. James Geikie announces the object of his memoir in the following words:—

"With Sir Roderick Murchison's permission, some of the more

¹ Quart. Journ. Geol. Soc., vol. xxii. p. 514.

interesting results obtained during the progress of the Survey are here described."

In my communication, I based my remarks upon principles which the entire geological world will unanimously concede to me, viz:— That in an investigation of admittedly one of the most intricate and abstruse problems which form the subject of geological research, it is absolutely, nay, vitally, essential that each step forward in the inquiry should be tested with the utmost care and suspicion; that each argument, derived from the collateral sciences, should be thoroughly examined into, as to soundness; and that no misunderstanding should be allowed to arise from the use of a bad or indefinite terminology.

From the tenor of Mr. James Geikie's remarks, may it not fairly be asked:—To whom does he address himself? or, for whom is he writing? whether to beginners in the science, or to the Geological Society of London? If to the former, may it not be inquired, whether the subject is not, in itself, too abstruse for beginners, and should not the most scrupulous care be taken, that nought but admittedly sound arguments, nomenclature, or similes, be made use of; for all know how exceedingly difficult it is to eradicate incorrect notions, when once they get into the head of a beginner in science. If to the latter, to whom his first memoir is especially addressed, are not geologists, when an author ventures to bring novel and sweeping views in the most abstruse departments of the science before a tribunal supposed to represent the highest geological talent of the empire, fairly entitled to demand that, at least, his premises are not indefinite or unsound, and that his phraseology is not, as admitted, "careless and unguarded."

After a careful perusal of Mr. James Geikie's reply, I cannot find anything therein which in any way disproves, or even shakes, the weight of my arguments; but, from that gentleman's defence, I can clearly understand, that the time has come when it will not do to mince matters in this discussion; for, as the reader will perceive, it is not against Mr. James Geikie that I am fighting, but against the system which he now attempts to defend.

Glad should I be if I could (as Mr. James Geikie would charge me with) believe "that the terminology of petrology is as fixed as that of the exact sciences;" what I do, however, believe is, that *it ought so to be*, and further, that it is a disgrace to the present state of geological science that it is not so.

No person is more fully aware that "looseness" in petrological nomenclature is unfortunately the rule, not the exception; and that geologists may continually be found mapping and writing of totally different rocks, under one and the same name; what I, however, would infer therefrom is, simply, that it is high time to reform.

In what, now, does Mr. James Geikie's defence consist? Upon perusal of his reply, it will at once be perceived that it is, in major part, a simple "tu quoque" to other (often eminent) writers upon the subject; an argument which may be very effective against these gentlemen, but one which the rest of the geological world will not

accept, as either exculpating an officer of the Geological Survey, or acquitting him of following an example patently bad.

In such instances one mis-statement becomes a precedent for another, and although such precedents may fairly be brought forward in extenuation, still they do not, as Mr. J. Geikie would have us believe, entirely exonerate himself.

For this purpose, he quotes names of the highest authority in other branches of geology, as Lyell, Phillips, and Dana. I would, however, not do Sir Charles, our great expounder of geological principles, the injustice to suppose that he would attempt to enforce strictly the rock definitions contained in chapters xxviii. and xxxiii. of his *Elements* as a standard for exact petrological comparison; nor, do I imagine, would the cautious Professor Phillips think it fair-play if the chapter v. of his *Manual* was to be dissected for similar purposes; and still less would the celebrated mineralogist Dana commit himself, without reserve, to the rock definitions given in p. 246, vol. ii., of his *Mineralogy*, where he does happen to allude to mica-slate as a gneiss with a distinctly foliated structure.

In questions of petrology, instead of quotations from works on general or elementary geology, I had expected to have been referred to works specially devoted to that subject; but, with the exception of the recent translation of Cotta, on the classification of rocks, a work acknowledged not to fulfil the requirements of the present state of science,¹ Mr. James Geikie does not even allude to them.²

Mr. James Geikie deprecatingly expatiates on the profound knowledge of chemistry, mineralogy, etc., which he declares I would require of the geologist, evidently not wishing to acknowledge that the pith of my argument was but intended as a warning to those geologists who really possessed no knowledge of these sciences not to expose themselves to just criticism by filling their pages with unwarranted or unsound chemical or other data or hypotheses.

The geologist who enters into the details of any one department of his science, will regard "Admirable Crichtons" as fossils from a very early period of science, for nobody knows better the absurdity of any man, however talented, pretending to be an authority on all branches of any one science; for in this century every science presents far too wide a field for any single labourer to cultivate all parts of it properly, or in other words, to be at the same time "well up" in every department.

From time to time, in geology, as in every other science, the appearance of a generalising mind like Lyell is required to take up the accumulating chaos of facts, and mould them into shape: the true steady advance of geological science depends, however, in greater part upon the labours of the working bees who provide these data, by (without attempting to grasp too much) devoting their energies to the minute and careful investigation of some special branch, how-

¹ Vide Reviews in GEOLOGICAL MAGAZINE and *Athenaeum*.

² There is no want of special works on this subject; to witness the publications of Blum, Brongniart, Coquand, D'Halloy, Erdman, Leonhardt, Mayer, Macculloch, Pinkerton, Roth, Senft, Serres, Zirkel, etc.

ever small, of the science, bringing in to their assistance a sound knowledge (acquired by patience and labour) of so much of the collateral sciences, as specially applies to the chosen department of inquiry: without, however, attempting or pretending to the impossibility of being at the same time profound in such science.

Mr. James Geikie does me injustice in making me appear to say, that the development of crystalline rocks from aqueous strata "is a notion supported only by his assertion." How could I give that gentleman credit for an idea far older than either of us, and cases of which I have myself years ago examined and described.¹ Upon reference to my communication, p. 57, it will be seen that I simply record a decided protest against the statement made by Mr. James Geikie, when he writes—

"It is *certain*, however, that rocks, such as diallogite, hypsorthenite, diorite, syenite, or even granite itself, can be developed directly from aqueous rocks," etc.

If instead of "certain," the word possible or even probable had been used, I should not have objected, and now repeat that a very careful inquiry into the literature of the subject, does, I consider, fully warrant me in protesting against any such dogmatic and sweeping assertion being made or accepted in the present state of our knowledge of the subject.

Opinions must not be represented as facts before they have received general acceptance. In a question in which the geological world is undoubtedly divided in opinion, no such sweeping assertions can be admitted as evidence in investigations of such intricate nature as those which are the subject of the present inquiry.

Mr. James Geikie cites, in support of his views, the labours of the late Professor Keilhau in his so-called "Transition-formation of Christiania,"² but surely, in so doing, he must be quite unaware that these had long ago been most thoroughly disproved, and set aside by the results of the subsequent explorations of Sir Roderick Murchison,³ and the still later researches of Professor Kjerulf,⁴ in his work upon that formation, which I would here recommend to all geologists as a model for the investigation of similar metamorphic phenomena as are here referred to.

Notwithstanding my distinct statement to the contrary, Mr. James Geikie seems determined to make the object of my communication appear as a declaration against hydrothermal action, and will not remember that it really was to examine his evidence, not to dispute his conclusions; and I now maintain, whatever truth may or may not

¹ Amongst others, I can refer to the highly crystalline Hornblende rocks so extensively occurring on the whole of the south coast of Norway, examined by me in 1853 and following year, and which it will be seen in my "Geologiske Undersogelse over det metamorphiske Territorium, ved Norges Sydkyst." *Nyt Magazin for Naturvidenskaberne*, Vol. iv. p. 164 et seq., I have declared to be, in my opinion, all formed *in situ* from tuffs of aqueous deposition.

² *Nyt Magazin for Naturvidenskaberne*, Vol. i., 1838, and more fully in the *Gaeta Norwegica*, Vol. i., of same author.

³ *Quart. Journ. Geol. Soc.*, Vol. i. p. 467.

⁴ *Das Christiania Silurbecken chemisch geognostisch untersucht*, 1855.

be in those conclusions, that they were not warranted by the evidence which he has laid before the geological public, who will, I think, agree with me in returning the Scotch verdict of "not proven," and advise him to try again if he wishes to convince the geological world of their correctness.¹

To avoid extending these remarks to too great a length, I will, in conclusion, only refer to one more point in Mr. James Geikie's reply.

That gentleman differs from me as to the meaning of the term "greywacké" in petrology,² and in page 178 informs the readers of your Magazine that "the greywackés familiar to Scottish geologists do not 'consist essentially of seventy-five per cent of quartz,' nor have they any definite composition whatever. The term 'greywacké,' as used by Scottish geologists, is applied exclusively to the hardened felspathic, and sometimes argillaceous sandstones of the Silurian regions, in which, although quartz is frequently present, it is by no means a necessarily preponderating ingredient."

Always regarding science and its nomenclature as cosmopolitan, I am of opinion that such style of argument should be protested against; as no doubt Mr. James Geikie would do, if informed that he must be quite wrong, because "Manx"³ geologists entertained a totally different opinion of the rock species "greywacké."

¹ Since my former communication, the arrival of Dr. Sterry Hunt in this country has procured me the pleasure of his personal acquaintance. The opportunity thus afforded us, of comparing notes on chemical geology, showed how many similar conclusions we had respectively come to, from the study of widely different parts of the globe, and assured us that any difference in opinion could not arise as to the agencies employed in Nature's operations, although we might be somewhat at variance as to the precise extent to which each agent had been engaged.

² A study of the rock in the field in localities specially characteristic, combined with an examination of the descriptions given by the numerous writers on the subject, has resulted in my defining this rock species as follows:—

Greywacké.—A sedimentary rock usually of a greyish colour (whence its name) found extensively developed in the earlier geological formations; but not specially characteristic of anyone of same. Petrologically, "greywacké" is an impure sandstone, more or less argillaceous, formed from the débris of previously existing rocks, rearranged by aqueous action, and subsequently, more or less consolidated. Usually compact, it may vary in texture from fine-grained to coarsely conglomeritic; the stratification of the beds is frequently indistinct, unless viewed upon the large scale; when coarse it may contain fragments of fossils, and of other rocks, as clay-slate, mica schist, granite, porphyry, limestone, etc. Mineralogically, it is essentially quartz, with more or less clay, and frequently contains grains or scales of mica, chlorite, talc, lithomarge felspar, calcite, iron pyrites, etc. Chemically, it is composed of some seventy to eighty-five per cent. silica, along with alumina and a little oxide of iron, with but traces of the alkali and alkaline earths. Chemical and microscopical examination show comparatively little combined silica, the major part being in the free state as quartz.

This definition I maintain is in accordance with the views of all the writers on the subject whom I have consulted, and in corroboration thereof, I would cite the following references: Bischoff, iii. p. 132; Blum, p. 284; Brongniart, pp. 123, 126; Coquand, p. 238; Cotta, p. 301; D'Halloy, p. 15; Erdman, p. 173; Grimm, p. 215; Jameson, p. 226; Kjerulf, p. 72; Leonhardt, p. 171; Mayer, iii. p. 1; Macculloch, p. 358; Page, p. 309; Pinkerton, i. p. 291; Phillips, p. 654; Roth, p. 69; Senft, p. 332; Zirkel, ii. p. 694.

³ And our little Island is probably one of the best localities in Europe for the study of this rock.

I had, however, far too great a respect for the many eminent geologists of Scotland, and too little confidence in either Mr. James Geikie's petrology, or his assertions, to accept the above statement without examining into its correctness, and I think the result of the inquiry will satisfy the public that the name of the Scottish geologists has been taken in vain, and that

1. The term "greywacké," when "familiar to," and "used by" Scottish geologists, corresponds satisfactorily with the definition I have accorded to it—one endorsed by geologists of all nations.
2. That this rock possesses not only a distinct mineral character, but, within certain limits, also a definite chemical composition.
3. That quartz is "a necessarily preponderating ingredient," and is generally present in fully seventy-five per cent.
4. That the term is not, by Scottish geologists, "exclusively applied to felspathic," etc., nor that the word "felspathic" should be at all used when referring to normal "greywacké."

Mr. James Geikie, who in his reply expresses his doubt as to my "careful examination of the literature of the subject," will think it still more strange and presumptuous in my thus attacking him at home, and undertaking the defence of the Scottish geologists; but I would ask him whether he is aware that it was a Scottish geologist, Professor Jameson of Edinburgh, the pupil and friend of Werner, who first introduced the term "greywacké" into the English scientific language; and if he will refer to that author's work upon the mineralogy of the Scottish Isles, published in 1800, he will there find (vol. i. p. 226) my definition perfectly confirmed. If, then, he turns to the (for its period, excellent) work on petrology, "Macculloch's Classification of Rocks," published in 1821, also by a Scottish geologist, it will be seen, at page 358, that the different varieties of greywacké are there respectively defined as rocks composed of—

1. "Quartz sand, intermixed with lamina or massive schist."
2. "Quartz gravel, of various sizes, similarly intermixed."
3. "Argillaceous schist, with very fine grains or powder of quartz. Not fissile; fracture sometimes rough and splintery, and often resembling the fine and grey varieties of primary sandstone."
4. Ditto, "with visible grains of quartz of various sizes, and resembling the coarser varieties of the same rock."

And if, to carry the literature of the subject down to the present day, he refers to a recent work, also Scottish, and which he already has quoted in his reply, viz., Page's *Advanced Text-book of Geology*, he will there find (p. 309) the previous descriptions confirmed—nor in any case will he find the felspathic element of greywacké, which is so convenient for his metamorphic hypothesis, even alluded to by these Scottish geologists.

DAVID FORBES.

LONDON, 3rd April, 1867.