

Like all instruments of research, this one of shape must be used with common sense and the surrounding circumstances taken into consideration, but when as on Moel Tryfaen extremely rounded and polished grains of quartz are found amongst a great mass of very angular material they may be treated as erratics. No rock in the neighbourhood could yield them, and to the educated eye they at once proclaim their sea-origin, whatever mode of transit may be theoretically provided for them according to the proclivities of the geologist.

I am glad of the opportunity of reiterating these views first brought forward in a paper recently read before the Geological Society.

T. MELLARD READE.

PARK CORNER, BLUNDELSANDS, *Sept. 7th, 1892.*

#### THE ROCKS OF SOUTH DEVON.

SIR,—Now that Mr. A. R. Hunt's three-months-long dissertation on the Devonian Rocks of South Devon has come to an end, I may ask space for a very few words, as I do not intend to discuss the subject in detail.

He attaches importance to mineral coincidences between the schists and the admitted Devonian rocks. Some of these, such as the iron-ores, seem to me very much of a Monmouth-Macedon type; others to be more naturally explained by supposing that the latter have derived some of their materials from the former or a kindred crystalline group, an alternative which seems to me inadequately discussed in his paper.

As I have always held that the dark mica-schists were once sediments, as the Devonian phyllites have been, and I have never denied the possibility that some of the green chlorite-schists originally might have been basic igneous rocks, parts of Mr. Hunt's arguments do not affect my position.

From Mr. Hunt's paper I infer that he is not aware that a schist, after crushing (particularly if dark in colour), is sometimes very difficult to distinguish from a much-squeezed dark slate; also that some other crushed crystalline rocks simulate squeezed grits. The difficulties are local, and generally can be overcome when you know what to look for, but they are so real that I always hesitate to express an opinion on microscopic slides when I have not seen the rock in the field, and even then, once or twice, when the outcrops were scanty, have been unable to come to a conclusion.

I have never denied that what it is now the fashion to call dynamometamorphism has greatly modified both the schists and the Devonian rocks, but, in calling attention to it, I pointed out that the one set "went into the mill" as schists, the other as clays. I do not find that Mr. Hunt has adequately discussed this very important matter.

During the nine years which have elapsed since my paper was written, I have many times examined both my own and other specimens from South Devon, and have had unusual opportunities of studying, in other regions, similar rocks and some sections which

were very helpful in illustrating those of the Start district. So, with a greatly enlarged experience, both in the field and with the microscope, I could now improve my former paper (*e.g.* I could amend the accounts of the "chloritic" rocks; should be more ready to recognise altered basic igneous rocks among them; should say that the mineral, very doubtfully identified with kyanite, and some of the smaller grains of water-clear mineral—thought then to be quartz—were more probably secondary feldspars), but I should express myself, if possible, yet more confidently as to the distinction in lithological characters and geological age of the two groups of rocks, the schists and the slaty Devonian system.

Mr. Hunt, so far as I can judge from internal evidence, has had little experience in dealing with problems such as that which he attempts—perhaps the most difficult presented to petrologists. Possibly his experience may be commensurate with my own, but till I have reason to believe that he has studied such problems in other fields than South Devon, and has ample materials at his command for the necessary research, I must decline to do more than say that my original opinion is not in any way altered by his dissertation.

T. G. BONNEY.

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"CONE-IN-CONE STRUCTURE."

SIR,—In the September Number of the GEOLOGICAL MAGAZINE there is a note by W. S. Gresley, on "Cone-in-cone Structure," in which he refers to "Mr. John Young's theory of how the rock was formed." With your kind permission, I beg to state, that I have *no* "theory" on the above subject, and in connection with the explanations that I have given of the cone structure in my paper,<sup>1</sup> the word "theory" is never used in any of my own explanations, but it will be found on p. 25, where I give the opinion of Professor Newberry, who there uses the word "theory" in connection with cone formation, "and the upward escape of gases through a pasty medium." Regarding its formation, all the explanations that I have ventured to give are founded upon what is revealed in the best preserved, and most illustrative specimens of the cone structure that I have found in the carboniferous strata of the West of Scotland, and, I do not think, that in these explanations of the various points of structure, that I have stated anything beyond what the specimens themselves most clearly reveal. I have, in various parts of my paper, pointed out that there are structures which have been referred to "cone-in-cone," but which present appearances so dissimilar to those noticed in my paper, that to them my explanations do not apply, stating, that they will each "have to be described with reference to their external characters and internal structures." JOHN YOUNG, F.G.S.

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<sup>1</sup> Trans. Geol. Soc. Glasgow, vol. viii.