

he admits that small pockets of such clay were also seen above it. Why does Sir Henry Howorth only quote the latter statement and not the former?

If Sir Henry will study the facts in the field, and especially if he will have a few excavations made at any of the localities where the relative age of the beds is doubtful, he will earn the gratitude of geologists, but his present methods of controversy do not entitle him to their respect.

There is an excellent field for research at Brandon; it is easy to prove that some of the brick-earths pass under the Boulder-clay, but there still remain two points to be decided, (1) do such brick-earths contain flint implements? (2) are there not other deposits containing flint implements and mammalian remains which rest on this Boulder-clay?

Let Sir Henry Howorth do for Geology what General Pitt-Rivers has done for Archæology, and we will welcome the results. Meantime any further endeavour to support a preconceived theory by a partial examination of written statements will hardly be welcome to readers of this Magazine.

September 5th, 1892.

A. J. JUKES-BROWNE.

SHAPES OF SAND GRAINS.

SIR.—It is pleasant to hear from so experienced an observer as Mr. Cecil Carus-Wilson that the views expressed in my paper on Glacial Geology on the generally superior roundness of Marine Sands as compared with river sands are borne out by his own independent observations.

My remarks on the rounding of sand grains were strictly limited to its bearing on glacial geology. The sand-dunes referred to were those of our own coast. Here from Crosby to Southport we have 23 square miles of Blown sand which I have been living on and working in as an engineer for the last 25 years. I can find no detectable difference in form between the sand grains of the shore and those of the dunes.

Desert sands are of course out of the question in glacial geology, and I quite agree with Mr. Carus-Wilson's observations relative to them. His other interesting observations shall have my attention in future work.

I have found my sand investigations of the greatest use in glacial geology, though not originally undertaken for that purpose. The polish in some of the glacio-marine sand grains is quite remarkable. No glacial *shelly* sands that I have examined fail to show much rounding of the grains—not only those quartz but the undoubted glacially derived materials also. There are also other glacial *shellless* sands of which there are the most convincing evidences of marine origin that exhibit equal evidences of extreme attrition.

The non-marine but purely glacial sands are invariably angular. I have just received from Professor J. J. Stevenson, of New York, a sample of sand from Glacier Bay in front of the Muir Glacier, Alaska, which is remarkably angular in grain.

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