When a large mass of molten matter occurred near the surface, and a fissure was produced in the way described, the weight of the ruptured crust would, if the plastic mass beneath were sufficiently liquid, cause the latter to rise in the fissure, producing dykes. Attention was called to the fact observed by Dutton that basaltic vents frequently occur on the brink of cliffs, but never at their bases; also to the existence of dykes having a strike parallel to the Colorado River. In most cases the vertical fissures which received the molten rock would begin to open from below, and the upper strata might altogether escape rupture.

The author discussed the case of the Henry Mountains, and explained the formation of flat-topped and flat-bottomed dykes according to his views. He next called attention to the influence which the motions of the rocks had exercised in determining directions of drainage when fissures left unfilled became occupied by streams. He next alluded to river-valleys, the existence of which had been accounted for by "antecedent" and "superimposed" drainage, and suggested difficulties in the way of accepting the explanations hitherto advanced, and considered them to be instances of fissuring produced by movements of the strata due to the pressure of a mass of molten or highly plastic rock spreading laterally.

After treating of the formation of faults with normal hade, which he referred in some cases to rupture of the solid crust by the spread of a vast mass of viscous matter lying beneath it (the faults being sometimes replaced above by monoclinal folds), he referred in conclusion to the extent of the horizontal compression of the earth's superficial crust, which is seen to be associated with the elevation of mountain-ranges, and called attention to some evidence that the thickening of the strata caused thereby would be more considerable and general than ordinarily supposed.

3. "Notes on a Recent Discovery of *Stigmaria ficoides* at Clayton, Yorkshire." By Samuel A. Adamson, Esq., F.G.S.

The specimen described was obtained in November, 1887, from the beds between the Better-bed Coal and the Elland Flagstone of the Fall-top Quarries of Messrs. Murgatroyd. The author gave measurements of the specimen, and compared them with those of another found in the same quarry in 1886, and now preserved in the Owens College, and with those of a third obtained in an adjoining quarry.

CORRESPONDENCE.

ROUNDING OF PEBBLES BY ALPINE RIVERS.

Sir,—Mr. Irving's remarks in your last number appear to call for a few words in reply. As my paper was entitled "On the Rounding of Pebbles by Alpine Rivers," I fail to see that I was bound to discuss other modes of forming pebbles, unless they seriously interfered with the inductions which I was attempting to draw. Hence, I did not mention "the weathering of débris on the mountain sides," because, so far as that had a bearing on my subject,

it strengthened my argument, which was to show that a respectable pebble was not easily made by running water, and because, in the case of the rocks with which I was dealing, the rounding mentioned by Mr. Irving is, as a rule, a very secondary and subordinate matter.

(1.) Every one knows that certain rocks become tolerably rounded by mere aërial waste, but the débris which reaches Alpine torrents (in the districts of which I spoke) is commonly angular; and this is equally true of the material to which my inferences applied. I may add that I believe few things are more important in attempting to reason inductively from observed facts than to be careful in preserving a due relation between quantities of the first and second order of magnitude (as they are called by mathematicians). Overmuch precision of statement and an elaborate parade of small details interfere with our sense of proportion, and there is great danger, if you look at a sprat for too long a time, and from too near a point of view, that you may at last fancy it a whale.

(2.) In regard to "the scouring action of sand," I cannot pretend to say how much is done by the knocking of the pebbles together, and how much by the friction of passing sand; but I certainly cannot make the distinction which Mr. Irving attempts to do. All the rivers of which I spoke transport quantities of sand as well as pebbles in all parts of their course, though it is only in the lower part that they can deposit much of the former. However, as it takes a very long journey to remove the angles from a grain of sand, I have my doubts as to its conspicuous efficiency as a fashioner of pebbles out of pieces of hard rock, and if Mr. Irving alludes to the action of sand on pebbles which but rarely travel, then I think it would tend to flatten rather than to round them.

(3.) In regard to the general question raised, viz. the origin of the pebbles and other materials of the Bunter group, space will not permit me to enter into details, so I must forbear to criticize minor but not unimportant points in Mr. Irving's letter, such as "the pebble-beds proper being quite *local*," a statement which is only true if a most liberally extended sense be given to the last word. But I express a fundamental dissent by asking whether there is any evidence at all in favour of the Bunter being a marine deposit, and still more a deposit in a sea where, according to the ordinary rules, strong coast currents or a rolling surf would be likely to exist. All I can say is that the Bunter as a whole is remarkably unlike every admitted marine formation which I have ever examined, while it presents a strong resemblance to such deposits as parts of the Old Red Sandstone, some beds in the Lower Carboniferous of Scotland, and the Nagelflue of the Alps: deposits, which most geologists agree in considering more or less fluviatile: nay, allowing for a slight difference in colour and hardness, the Bunter pebble-beds of Central England (I said nothing about Southern England) are indistinguishable from many of the old sub-Alpine river-drifts which I have repeatedly examined.

T. G. Bonney.
