CORRESPONDENCE.

QUARTZ DYKES NEAR FOXDALE.

Sir,—Though I have not visited Foxdale in the Isle of Man, I venture to express a doubt whether Mr. Lomas (p. 34) has succeeded in proving its quartz dykes to be igneous rocks. 'Dykes' and veins of that mineral are common in many countries, and cut almost all kinds of rock, though, as might be expected, they are rather rare in the more igneous or basic limestones. Sometimes the dykes attain a considerable thickness and may be traced for a long distance; at others veins run off into the finest threads, and their demeanour is unlike that of an igneous rock, from which they are often far away, and their abundant fluid cavities and consequent whiteness (as described by Mr. Lomas) suggest that they have been formed from water. That silica, both crystalline and colloid, is so deposited, especially from hot springs, is well known (see, for instance, a very important paper by the late Mr. J. A. Phillips, published in the Quarterly Journal of the Geological Society, vol. xxxv, p. 390). The fact that at Foxdale quartz veins traverse the granite in itself suggests they are later in origin and formed by thermal waters, with which their occasional relation to dykes of microgranite is quite consistent. Mr. Lomas appears to regard the fact that the veins on entering the granite change locally into pegmatite as strongly in favour of his hypothesis. But the presence of felspar or even mica in a vein does not prove it to have had an igneous origin. I have examined numbers of quartz-felspar-mica veins in gneissoid rocks which seemed to differ in important respects from granite. Sometimes, though by no means always, they have been affected by the pressures which have produced the schistose structure; but the three minerals are usually associated in a clotted and irregular fashion, very different from that characteristic of rocks which have solidified from a molten condition, and their structure frequently is abnormally coarse, even in comparatively thin veins, where a true igneous rock would be almost invariably either compact or not more than microgranular. The most remarkable case of mineral grouping which I have seen was in the neighbourhood of Svolvaer in the Lofoten Islands. Here a coarse gneissoid rock was cut by a quartz vein, varying irregularly in breadth from two or three feet to as many yards. By its side, and in places mixed with it, were a fairly broad band of felspar and a much narrower one of a dark ferromagnesian mineral, which at the time, more than thirty years ago, I took for a pyroxene. The quartz was white and curiously divided by sharp joints into parallelepipeds, rather variable in size. If, then, this was an igneous vein, there must have been three distinct ejections (only locally mixing) of quartz, felspar, and a ferromagnesian mineral (not necessarily in the order of enumeration). The pegmatite of the Foxdale vein, according to Mr. Lomas, contains felspars, some over three inches long, perfectly formed, and showing crystal faces. But the latter habit (except when there is considerable difference between the fusion point of a mineral and the residual

magma) is far more indicative of formation by water, and is not usual in pegmatites, so far as I know them; if, indeed, all these are igneous rocks. Mr. Lomas, however, may reply that he does not assert all quartz veins, even if including felspar and mica, to be igneous, but only that at Foxdale. But if so, we may fairly ask him to tell us how to distinguish igneous from aqueous veins. The former, when they cut through sedimentary rock, especially if it be argillaceous, generally produce rather conspicuous structural and mineral changes, so that here I expected Mr. Lomas to give a careful description of the contact-metamorphism or to offer an explanation of its absence. Instead of this I find only the vague phrase 'altered slate'—a phrase compatible with slight silicification or other changes such as may take place by ordinary infiltration, and thus be no help to his hypothesis. I do not deny that differentiation might possibly be carried so far in an ordinary acid magma as to leave a residuum of pure or nearly pure silica (though I have never met with an instance of it), but I think it more probable that, as Mr. Lomas substitutes at critical points vague phrases and inconsequent statements for precise description, he has yielded to the fascination of a novel hypothesis.

P.S.—The above was written before the publication of Mr. Harker's letter (p. 95). T. G. Bonney.

THE ORIGIN OF QUARTZ-VEINS.

Sir,—In connection with the question of the origin of certain quartz-veins,[1] the fact that quartz reveals plastic qualities at temperatures considerably below the melting-points of many undoubted igneous minerals must be born in mind. J. Joly.

Trinity College, Dublin.
February 9th, 1903.

NEW GEOLOGICAL TERMS AND FALSE ETYMOLOGY.

Sir,—As no one seems inclined to protest against the terms 'calcrete' and 'silcrete' with which Mr. Lamplugh proposes (in your December number[2]) to disfigure geological nomenclature, I must even raise a voice in the desert. Brief expressions for what he intends them to convey would doubtless be useful, and no one would be likely to quarrel with 'calcicrete' and 'silicicrete,' of which one would be two, the other three, letters longer. I admit that public convenience may sometimes prevail over strict etymological rules, as in preferring the inaccurate 'telegram' to 'telegrapheme'; but 'calcrete' and 'silcrete' are even worse than the fashionable mongrel 'peneplain,' and approximate in malformation to the hideous 'phenocryst,' which seems invented to signalize the divorce of geology from culture. T. G. Bonney.

THE DEHYDRATION OF LATERITE.

Sir,—The very interesting paper on "The Constitution of Laterite," by Mr. T. H. Holland, appearing in your issue for February, 1903, raises several questions of chemical physics which

[1] See Mr. J. Lomas's article, Geological Magazine, January Number, p. 34, and Mr. Alfred Harker's letter, February Number, p. 95.

[2] Geol. Mag., December, 1902, p. 575.