CORRESPONDENCE

VOLCANIC ROCKS OF THE ORAMUTIA SECTION, CENTRAL KENYA

SIR,—I visited the locality described by Bristow (*Geol. Mag.*, 1962, Vol. **99,** No. 2, pp. 153–63), both during the course of a regional geological survey (McCall, report awaiting publication by the Geological Survey of Kenya, on the Nakuru-Thomson's Falls-Lake Hannington area) and subsequently with Mr. Bristow. I agree with him that the dyke-like body marked " Dyke 2 " on his figure (*op. cit.*, No. 2) is difficult to explain otherwise than as a feeder continuous with a restricted surface flow, though this is not perhaps incontrovertibly established. If, however, the Katmaian mode of eruption suggested by the field relationships is accepted, it has further implications beyond those stated by Bristow. The following important deductions can, in the writer's view, be made :—

(1) The eutaxitic structure, produced by inset shreds or *fiamme* and characteristic of described ignimbrites, must in this case have developed before extrusion.

(2) Marshall (1935, p. 1) evoked a cloud or shower of dispersed particles as the progenitor of ignimbrites : this image seems at variance with the picture gathered at Oramutia of formation within a body of magma welling up a fissure.

(3) *Tephra* can have played no part in the genesis of this eutaxitic and apparently fragmental rock.

(4) If, as I recollect, the foliation twists steeply downwards within the dyke-like body, lithostatic load effects can be discounted as a factor in producing the flattened *fiamme* structure.

(5) The attribution of such a rock to *nuée ardente* activity is, to say the least, debatable. Admittedly, somewhat nebulously—defined forms of this phenomenon, differing to a marked degree from the rising cloud and avalanche phenomenon so graphically illustrated by Ross and Smith (1961, Fig. 2), are accepted by many geologists. However, it is difficult to accept that a process, operative before extrusion, can be referred to *nuées ardentes*.

If McTaggart (1960) is correct, entrapped air is involved in the mechanism, and if, on the contrary, only endogenous gas is involved, one can surely only say that a body of the type seen at Oramutia might later give rise to a *nuée ardente* : for, while it is still within the feeder channel, it is not yet a *nuée ardente*, and yet the *fiamme* structure is already formed ; and so one cannot say that *nuée ardente* activity is responsible for this structure, which is clearly developed in vesiculating magma well before extrusion.

I have stated, briefly, my case for the froth-flow process in the genesis of these rocks elsewhere (*Nature*, 1962, Vol. **194,** pp. 343–4), and will shortly be elaborating these arguments, tracing the development of the type of structure seen in the dyke-like body and corresponding flow at Oramutia, from its embryonic stage—a segregation of two slightly differing glassy areas in an otherwise normal lava. To my mind the singing kettle effect is the primary cause of all the varied heterogeneous rock types seen in the Kenya field. A coherent rock showing *fiamme* and resembling a lava, has not, in my view, ever passed through a stage in which it had the form of dispersed particles enveloped in gas, and thus the reconstitution of a coherent mass by welding or agglutination which is a fundamental part of the ignimbrite hypothesis need never have occurred. In depth, I suggest, this rock would pass gradationally into a normal, homogeneous, igneous rock. The fundamental difference between the froth-flow hypothesis and the ignimbrite hypothesis is that in the first case incoherent, pumiceous layers are considered as products of rupturing by vesiculation, but incapable of reconstituting themselves by means of their endogenous energy into flows such as we see at Oramutia

(coherent, lava-like masses showing *fiamme*) : in the second case such incoherent masses are considered as not having had the internal energy necessary to return to a coherent state, showing *fiamme*.

I have studied some of the pumiceous upper flow-rocks from Oramutia and it is clear that Bristow is correct in saying that they are frothy, pumiceous lavas and not true tuffs, notwithstanding their superficial appearance of a tuff. It is also clear to me that these have never suffered complete disintegration to an incoherent state.

Finally, I must mention the term devitrification used by Bristow (*op. cit.*, p. 160) : is it really certain that the minute cyrstals referred to are due to the slow conversion of a glass to the crystalline state? These rocks are Pleistocene and not much progress in devitrification is to be expected in the short time that has elapsed since their eruption. I believe that a great deal of primary, incipient crystallization is being dismissed as due to devitrification.

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4th September, 1962.

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THE STRUCTURE OF MID-DEVON AND NORTH CORNWALL

Sir,—The letter from Professor S. Simpson published on the 4th June, 1962, in vol. xcix, part 3, of this magazine under the heading of " Structures of Devon and Cornwall " presents an alternative to my (1962) interpretation of the structure of the tract of country extending from the Cornish coast in the general neighbourhood of Boscastle to the north-western margin of the Dartmoor granite. But at the same time Professor Simpson has misconstrued, at least in part, my remarks on the probable structural implications of Dr. E. B. Selwood's (1961) discoveries. I must accept responsibility for this as I did not take the trouble to embellish my letter with illustrative text-figures. This omission has now been rectified by Professor Simpson. I find that his illustrations can be used, with slight additions, to clarify the essential differences between his interpretation (Text-figs. 1*b*, 2*b*, 3*b*) and the alternative interpretation (Text-figs. 1*a*, 2*a*, 3*a*).

Professor Simpson says that there is a line which runs continuously from the Cornish coast somewhere between Crackington Haven and Widemouth to the northern end of Dartmoor. For convenience it is proposed here that this should be referred to as the Widemouth–Okehampton line. This fundamental structural line, as he called it in 1961, separates recumbent folds on the south from overfolds on the north. I agree with this as a generalization that was already well known from the work of Owen (1937, 1950) and Ashwin (1958) on the coastal sections.

I also agree with Professor Simpson that Text-fig. 1*b* is a valid interpretation of the structural implications of northerly dipping inverted strata. Although he has not yet given a full account of his interpretation of the structure of the area, he has suggested in a brief abstract (1961) that if the recumbent folds have come into position by gravitational sliding it is possible to regard them as the original cover of the older and more deformed rocks of the south (north in the original but an obvious misprint). This statement has been freely interpreted by me in Text-fig. 3*b*.