

and thus be interpreted as a relict fabric. It is important, meanwhile, to record that there is an extensive development in Scotland of a north-west south-east lineation which apparently *cannot* have been induced by the Caledonian movements, but which must be related to a pre-Torridonian orogeny, since these lineated rocks of the Foreland are overlain by unmetamorphosed Torridonian and Cambrian sediments.

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CRYSTALLIZATION OF PLUTONIC AND HYPABYSSAL ROCKS

SIR,—I should like to reply to Mr. McIntyre's letter appearing in the last number of the *Geological Magazine*. I will deal with the points he raised in order:—

1. If members of a series of related minerals cease to crystallize out one after the other, a eutectic relationship between the individual minerals is excluded. Obviously eutectics and reaction points can occur in the same system, and a "concealed maximum" is associated with an incongruent melting compound which gives rise to a reaction point in the system. I am afraid I assumed my readers would be familiar with the more elementary types of phase diagram.

2. As Mr. McIntyre is so interested, I can indicate the kind of phase

relationships which *may* be involved. So far as hornblende is concerned they could be those which occur when a compound melts incongruently to give two solid phases (in this case augite and hypersthene) and liquid. This case is dealt with, for a ternary system, by Vogel in *Die heterogenen Gleichgewichte*, 1937, p. 446. So far as augite and hypersthene are concerned, they might crystallize out together simultaneously along the boundary curve between their respective fields; or the shape of the boundary curve might be such that one of its tangents passed through the composition of augite, the point of tangency dividing the curve into two parts, one of reaction, and one of simultaneous crystallization.

3 & 4. It is not meant to infer that the final residual liquid in differentiation contains nothing but Ca-Na-K alumina-silicates but such compositions are so closely approached that their position in what Bowen refers to as the imaginary polydimensional space diagram, is substantially determined by the equilibrium relations in the Or-Ab-An-Qz diagram. The position is similar to that in, say, the system leucite-diopside-silica, where the ternary eutectic orthoclase-diopside-silica contains so little diopside that little error would be introduced by considering the binary eutectic orthoclase-silica in its place. Strictly speaking the line representing the crystallization period of biotite in my diagram should be extended to the end on the right hand side, and the words "ceased crystallizing" as applied to biotite should read "ceased crystallizing to all intents and purposes".

5. I do not state that the crystallization curve for the light constituents, determined by study of the rocks, coincides with the theoretical curve followed by a liquid in contact with all its early crystals. The only time I specifically draw attention to the form of the curve is when it has been projected into the system Or-Ab-An, when I state that it corresponds "*with a moderate but not particularly strong degree of fractionation during crystallization*" (p. 215). Naturally the liquid at any stage is in contact with some of the earlier formed crystals. The latter do not all sink like stones to the bottom of the magma chamber the moment they are formed. The whole point is that the liquid is not in contact with *all* its earlier formed crystals, and this is exactly what a moderate degree of fractionation implies. Far from being conclusive evidence that differentiation did not take place by separation of early crystals from the liquid, it shows that a certain amount of such separation must have occurred.

6. The relations between granite-pegmatites and aplites will be dealt with further in a forthcoming paper. The point I tried to make was that the phase boundaries determined in a dry melt system are not applicable without any change in position to a watery solution containing the same components.

7. Yes, I think in some cases only a small amount of magma is required, just sufficient to lubricate the crystal aggregate and allow it to intrude.

8. Only someone already prejudiced against the hypothesis of magmatic differentiation could conclude that the results given in my paper show "that a magmatic interpretation of the Scottish Caledonian complexes cannot be reconciled with the firmly established principles of phase-rule chemistry". The facts brought forward in connection, more especially, with the light constituents indicate a most striking similarity of behaviour between these constituents in natural magmas and in experimentally determined melts. Is it an accident that the last residual liquids of natural magmas should lie on the ternary cotectic curve? This affords a rational explanation of the composition of the aplites and their related lavas and explains why rocks of more extreme acid character are not found here or in other igneous rock series. The hypothesis of emanations does not explain this limitation of composition nor does it explain the fact that lavas and dyke rocks, belonging to the same suite, have the same chemical composition as plutonic rocks lying on the assumed liquid line of descent.

It is interesting to learn that Mr. McIntyre has been investigating the Loch Doon complex, but hardly surprising to find him concluding it has been formed by metasomatic replacement, in view of the address given at the bottom of his letter. I cannot feel this to be an argument against the hypothesis of magmatic differentiation.

In conclusion, it is gratifying to find an emanationist taking some interest in phase diagrams, even if in a rather naïve fashion. If only Mr. McIntyre can persuade his fellow emanationists to do likewise, there may come a day when the emanationist view of the origin of rocks can be discussed as a reasonable scientific hypothesis.

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CHILLED AND "BAKED" EDGES AS CRITERIA OF RELATIVE AGE

SIR,—In their recent letter to the *Geological Magazine*, Dr. Richey, Dr. Stewart, and Professor Wager record the fact that on Glamaig, on Sron a'Bhealain, and in Allt Daraich, in the Red Hills of Skye, they have found marscoite to be chilled against the granophyre with which it is in contact. They interpret this evidence as proof that the marscoite layer is younger than the adjacent granophyre. On equally good evidence, however, i.e. the inclusion of relics of marscoite within the granophyre of these localities, Harker (*Skye Memoir*, 1904,