CRYSTALLIZATION-DIFFERENTIATION.

SIR,—Mr. H. H. Read's acquaintance with modern French literature is evidently thorough, and one is surprised that this acquaintance has not rendered him more apt at simile. He compares my dependence upon crystallization as an explanation of many features of igneous rocks with M. d'Astarac's dependence upon sylphs as an explanation of ordinary happenings. Unfortunately for his comparison, we know that magmas do crystallize, and equally unfortunately for his dependence upon liquid immiscibility we do not know that silicates unmix. Immiscibility is the petrologic sylph.

In fact, immiscibility is a sort of super-sylph. One sees two related and intimately associated rocks, and one remarks that they were evidently formed from a common magma by unmixing. Since nothing is known about unmixing in silicates, one is therefore relieved from the responsibility of any further thought in the matter. However, Mr. Read's last paragraph rouses some hope. He expects that my excessive advocacy of crystallization will lead some one to champion immiscibility. If this some one will analyse immiscibility from a theoretical standpoint, if he will then apply his results to silicate magmas and examine whether associated igneous rocks bear such a relationship to each other in chemical composition, in time and space relations, that they can or cannot be regarded as the result of immiscibility, he will have conferred a boon upon petrologists, whether a succeeding generation shall find his conclusions right or wrong. But no amount of setting up immiscibility as a sylph will advance petrology.

It is possible, of course, that I have overworked crystallization, but no one who has seen the spring break-up in one of our Canadian lakes could doubt that deformation of a crystal mesh must have important consequences in the case of igneous rocks as well. There one sees weakened and honeycombed bodies of ice under impact from other masses, locally compacted into a solid mass with the water squeezed out of the comb, elsewhere stretched, fissured, and traversed by streaks of open water. That more or less related features could develop below ground is not to be questioned.

Though Mr. Read's review is for the most part a series of objections they are nearly always generalities, and are only occasionally specific enough to be answerable. My method of deriving *some*[1] banded rocks by torsion of a crystal mesh would not, as he concludes, give rise to an orientation of the early crystals normal to the bands. It is in the filling of the lenticular spaces that free flow of liquid occurs, and such crystals as would become detached from the walls of the lens during this action would be carried along by the liquid

[1] I have elsewhere suggested that banded rocks may at times be formed as a result of intrusion of heterogeneous liquid (not immiscible liquids). "Later Stages of the Evolution of the Igneous Rocks": *Journ. Geol.*, Suppl. to vol. xxiii, 1915, p. 30.

and oriented parallel to the banding. The slight amount of rotation of crystals possible in the compacted part of the mesh would not be sufficient to orient them in any particular way. The action would, as a whole, give rise to bands showing flow-structure, therefore, and exhibiting the moderate contrast between bands that is normal in banded gabbro. The occasional ultra-basic bands of extreme contrast could also be developed as a further result, for in the larger lenses of liquid crystal sorting would occur under conditions particularly favourable to the production of monomineralic types.

Though apparently not himself a strong advocate of crystallization differentiation, Mr. Read appears to have some apprehension that the credit for originating certain ideas in that connexion might leak out of the Tight Little Island. He therefore points out that Darwin postulated crystal-settling seventy years ago, and in two places that Barrow postulated mechanical straining thirty years ago. Though Darwin needs no eulogy of mine or Mr. Read's, I too have pointed out Darwin's origination of the idea of crystal-settling,[1] and though I have not referred directly to Barrow's work, I have referred to Harker's discussion of it and similar work.[2] If I may be permitted, I would like to point out to Mr. Read that the assumption of immiscibility also dates back to Darwin's time, and that it is still an assumption—a sylph, if he prefers the term.

N. L. Bowen.

Kingston, Canada.
February 27, 1920

OBITUARY.

Robert Etheridge (1847–1920).

Mr. Robert Etheridge, the son of the distinguished geologist and palæontologist of that name, died after a short attack of pneumonia at Colo Vale, near Sydney, on January 4, in his 74th year. Etheridge early took up geological work in Australia, as a member of the first Geological Survey of Victoria, under the direction of A. R. C. Selwyn, in the middle sixties. The survey having been disbanded as the result of a political crisis, young Etheridge returned home and was appointed palæontologist to the Geological Survey of Scotland, his father being then palæontologist to the English Survey. When the natural history collections of the nation were removed from Bloomsbury to the new Natural History Museum in the Cromwell Road, the two Etheridges were brought on to the staff of the Geological Department, where the memory still remains of the vigorous actions and language of "R. E. junior". The chief piece of palæontological work accomplished by Etheridge while in this

[1] N. L. Bowen, "Crystallization Differentiation in Silicate Liquids": *Am. Journ. Sci.*, vol. xxxix, 1915, p. 175.

[2] "Later Stages of the Evolution of the Igneous Rocks": *Journ. Geol.*, Suppl. to vol. xxiii, 1915, p. 14.