These arguments might be expanded indefinitely, but the purpose of the present paper is to call attention to a great opportunity in the hope that many other writers will offer suggestions out of which the real solution will come. Until the time when some capable organization can take charge of the work, correspondence through the scientific journals might accomplish much, or the present writer will welcome private correspondence which he will undertake to arrange and turn over to whatever organization may prove suitable.

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ARE BATHOLITES UP-BULGES OF SIAL?

THE able report to the National Research Council by F. F. Grout and a discussion in Washington recently have revived in my mind a question that has been there a long time. Are the granite batholiths up-bulges of the sial layer some ten kilometers thick of which the earthquake waves have informed us, or are they intrusions of more modest dimensions—phacolites, perhaps, such as are suggested by the work of Balk and Buddington[1] in the Adirondacks? Does the coarseness of their grain throw any light?

In the theory of the coarseness of grain as affected by the rate of diffusion (of heat or mineralizers) which I developed in 1894–1896[2] which is abstracted in Fairbanks' recent book[3] I found that there should theoretically be a central belt of uniform cooling and grain, in which the size of grain did not vary with the distance from the margin. For the grain I obtained a formula:

$$E = \frac{kc}{\pi a \sqrt{u}}$$

That is, the average linear dimension of the grains (E) increases proportionally to the "power of crystallization," which depends on the composition, etc. (k), and to the linear scale of the phenomena, *e.g.*, the thickness of an intrusive sheet and its contact zone (c), but decreases with the square root of the diffusivity (a^2) and the difference between the conditions (temperature) at which crystallization takes place and those of the country rock (u). The initial conditions of the magma are not a factor! They may be much hotter.

In small aplite dikes the grain is fine because (c) is small. But in the pegmatites we may find extremely coarse grain which I take to be because (u) is small. That is, the country rock is hot and the magma is full of mineralizing juice so that the crystallization temperature is low.

But so far as my experience goes, and that of such friends as Larsen and Laforge, in a normal even grained hypidiomorphic batholite of granite there is a rather narrow range of grain, say from 0.2 cm to 2 cm, usually about 0.8 cm. From the character of the quartz we know that (u) is less than 825° C. and probably is about 400° or possibly may get down to 200°. Its square root is then 20 ±. The square root of the diffusivity is about 0.07. From certain cases where the size of the granite dike or boss may be inferred I judge that is something like antilog -5 ± 1, which is quite a range to be sure, but puts limits to (c). For a normal anchieutectic granite (using Vogt's term) k can not vary enormously except with the mineralizing water present, an increase of which will tend to lower (u) and also the viscosity, in other words increase (k). Thus the limit in the coarseness of their grain (hypidiomorphic and not protoclastic) would seem to put a definite limit to the depth and size of granite batholiths. I should like to have colleagues test the matter. The larger the granite batholiths are the greater the (c). The deeper they are the greater the country rock temperature and so the less the (u). In both cases the greater would be the grain. It looks as though the granites we see could hardly be direct up-bulges of a crystallized ten kilometer layer, as Van Hise used to urge and I would rather like to believe.

I will quote from a letter just received from Professor A. Holmes:

> My experience in Mozambique and knowledge of other areas like Finland by their literature suggest to me that batholiths can not be very deep, because no sign of them is to be found in the levels of the crust deeply denuded by long exposure and uplift. The rocks there are all gneisses veined through and through with thin granitic veins.

ALFRED C. LANE

ANOTHER CAPTURE ON THE NEW JERSEY COAST OF THE BASKING SHARK, CETORHINUS MAXIMUS

ABOUT 2 A. M., June 5, 1930, two fishermen (Carl Holgerson and Edwin Gustafson, of Monmouth Beach), in fishing their gill net about 15 miles south by east of Long Branch, found a large shark tangled up in it. The shark had so many fathoms of net rolled around it that there seemed nothing to do but tow it to shore and there after daylight salvage what they could of the net. After two hours' work they succeeded in getting a double half hitch of three-quarter-inch rope around the snout of the shark, and

[1] New York State Museum Bull. 281, 1929.

[2] *Bull. Geol. Soc. Am.*, 8: 403, etc., 1897, also 14: 394–5.

[3] "Laboratory Investigation of Ores," Chapter VI, p. 123.