CORRESPONDENCE

PRECAMBRIAN PERTHOSITES IN NYASALAND

SIR,—In his recent article on Pre-Cambrian perthosites of Nyasaland (*Geol. Mag.*, **98,** 235–245), Dr. S. W. Morel evidently deals with a series of rocks showing an evolution of the perthites, which has not received proper recognition. The evolution could perhaps be demonstrated from the rocks formed solely of perthites—through rocks formed of perthites with marginal oligoclase (in optical and crystallographic continuity with the plagioclase component of perthite, p. 241), to rocks formed of perthites " separated from one another by a narrow zone of granulitic oligoclase " (i.e. discrete grains of oligoclase). The apparently " perthite free " types may evolve from complete replacement of perthite. The transitional stages of replacement are marked by relict " wisps of antiperthites " in oligoclase. The particular observation that " the amount of oligoclase present varies with the amount of perthite " signifies a genetic connection between the two. Such trends of textural evolution of perthites have long ago been discussed and demonstrated by Tuttle (1952). The development of sodi-potassic felspar (from corresponding late liquid) in the rocks was very likely mostly post-kinematic, as hinted by the author (p. 241). Igneous action thus partly post-dates metamorphism in the region studied.

Perthosites of Koraput, India, have many features in common with those of Nyasaland and are formed of perthites with crenulated margins, due to development of secondary albite. The exsolving albite shows a tendency to come out of the host and to localize along the periphery of grains. The feature is, however, well marked in the associated nepheline syenites.

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CALCUTTA.
15th August, 1961.

REFERENCES

MOREL, S. W., 1961. Pre-Cambrian Perthosites in Nyasaland. *Geol. Mag.*, **98,** 234–245.

TUTTLE, O. F., 1952. The origin of contrasting mineralogy of extrusive and plutonic salic rocks. *J. Geol.*, **60,** 107–124.

ORIGIN OF ALBITE PORPHYROBLASTS

SIR,—Dr. Trendall, in his letter to the *Geological Magazine* (**98,** 263–4), considers that I have misrepresented his conclusions concerning albite gneisses to an extent worth correcting. My purpose in quoting Dr. Trendall was not to be critical, but to give his work precedence and acknowledgment. It does seem that I have misunderstood him at one point, but I cannot agree with his other claim to have been misrepresented, or with some of his additional arguments.

(1) I apologize for the statement " and Co. Mayo ", which is not correct. In listing evidence for a sedimentary origin of the Dalradian albite gneiss, Dr. Trendall (1953, p. 218) wrote: " 2, The coincidence of the limit of the albite schists, in the south-west Highlands in particular, with stratigraphical boundaries. The albite schists are virtually confined to the Ben Ledi Grits." I wrongly assumed this to apply to their Irish equivalents in general. But my paper was concerned only with the evidence from a very small part of Scotland, and the stratigraphical position of albite schists in western Ireland would not have affected my conclusions about the Grampian Highlands albites.

(2) The Cromar gneisses are certainly migmatites, but are considered to have been formed through the injection of a soda-rich magma (cf. Trendall, 1953, p. 227). Is " trondhjemitic magmatic source " a great misrepresentation?

(2) The Cromar gneisses are certainly migmatites, but are considered to have been formed through the injection of a soda-rich magma (cf. Trendall, 1953, p. 227). Is " trondhjemitic magmatic source " a great misrepresentation?

(3) " Dr. Jones fails to make clear where his views differ from mine and does not in fact suggest any origin for the albite porphyroblasts except to agree with Reynolds and myself that ' sodium was introduced from an extraneous source by a large scale metasomatism '." The answer to the first allegation is simple: there is no fundamental difference between Dr. Trendall's views and mine about the origin of albite porphyroblasts. The only difference, as stated in my paper (Jones, 1961, p. 52, lines 3–7), is in the detailed evidence on which they are based. The second part of the sentence is self-contradictory, since I did not claim to be making a new suggestion, and it is not unusual to agree with previous authors when the evidence demands it.

(4) The angular discordance between the albite and garnet " isograds " is not necessarily that envisaged by Dr. Trendall. His location of the garnet isograd (1953, Fig. 101) is incorrect by over a mile in places according to my own mapping, and consequently I cannot accept his grounds for thinking it inverted. A correct location of the isograd is critically important in this connection. Dr. Trendall's statement that the limit of albite porphyroblasts is a gently curved surface following the predominant regional foliation—the Cowal anticline—is also not necessarily correct. The Cowal anticline does not exist in the present area, but dies out some distance to the west. Moreover, even if the limit did follow the anticline, the curved surface could just as logically be steeply inclined or vertical. There is, however, a sharp angular discordance; but I wonder how many metamorphic petrologists would agree, on the evidence available, that " For two manifestations of the same regional metamorphism to possess bounding surfaces intersecting at a high angle, a considerable time gap seems necessary ". Nothing quantitative is known about nucleation and growth rates of garnet, albite, or any other mineral in metamorphic rocks. There is also no quantitative data about rate of diffusion of sodium through rocks; so, under these circumstances, is it not rather speculative to write about a " considerable time gap "? It is to avoid such speculation that I prefer to refrain from suggesting a source of the sodium until more evidence is forthcoming. There is at present none on which to base a source hypothesis for this area.

I would like, finally, to say that I enjoyed reading Dr. Trendall's thesis, and found it both interesting and valuable. I hope it is now clear where some of our differences lie, and I suggest that they may be more significant than he supposes.

K. A. Jones.

Department of Geology,
The Queen's University of Belfast,
Belfast, Northern Ireland.

23rd October, 1961.

REFERENCES

Jones, K. A., 1961. Origin of Albite Porphyroblasts in Rocks of the Ben More–Am-Binnein Area, Western Perthshire, Scotland. *Geol. Mag.*, **98**, 41–55.

Trendall, A. F., 1953. The Origin of Albite Gneisses. *Ph.D. Thesis, University of Liverpool.*

—— 1961. Origin of Albite Porphyroblasts. *Geol. Mag.*, **98**, 263–4.

ORIGIN OF ALBITE PORPHYROBLASTS

SIR,—Reynolds (1942, p. 59) wrote " albite-schists . . . owe their existence to introduction of sodium silicate, which . . . was driven forward in advance of a syntectonic igneous mass emplaced along the axis of the Carrick Castle recumbent anticline ", and cited with apparent approval in the next paragraph Read's conclusion that certain oligoclase porphyroblast schists of Aberdeenshire were formed " by transference of soda, lime, and silica from . . . trondhjemite magma into the country rock ", at the same time suggesting that trondhjemite is not abundant in Scotland only because it has been removed by erosion. I apologize to Dr. Reynolds if my acceptance of Dr. Jones' phrase " a trondhjemitic magmatic source " as a valid paraphrase of her views on the immediate source of the introduced sodium of the albite schists was at fault, but suggest that the best defence against misrepresentation is lucid and unambiguous exposition.

A. F. TRENDALL.

GEOLOGICAL SURVEY OF UGANDA,
P.O. BOX 9,
ENTEBBE.
5th November, 1961.

REFERENCE

REYNOLDS, D. L., 1942. The albite-schists of Antrim and their petrogenetic relationship to Caledonian orogenesis. *Proc. R. Irish Acad.*, **48**, B, 43–66.

NEW RECORD OF *ANTHRACONAUTA TENUIS*

SIR,—I should like to record further finds of *Anthraconauta tenuis* (Davies and Trueman) in North Staffordshire. The new record is from the Spoutfield Tileries of Caddick and Son, Ltd.

Early this year I received the following report from Mr. M. A. Calver, of the Geological Survey :—

> " . . . the fauna is very similar to that which you recorded previously from Metallic Tile Co.'s pit at Bradwell and Downing's marl pit at Etruria. I am interested to see in your collection a good example of *A. tenuis* except that here again the specimen is somewhat smaller than the holotype, but otherwise agrees with it closely. This shell is associated with typical *A. phillipsii* and there are also intermediates between these two species. The ostracods *Carbonita wardiana*, *C. pungens*, and *C.* cf. *humilis* are also present."

Another visit to the Spoutfield Tileries was made a few months ago and more *A. tenuis* obtained. Again the specimens were found about 3 ft. 6 in. above the basal limestone of the Newcastle Beds.

JOHN MYERS.

148 HEMPSTALLS LANE,
NEWCASTLE,
STAFFS.
19th November, 1961.

REVIEW

CRYSTALLOMETRY. By P. TERPSTRA and L. W. CODD. 420 pp., 274 figures. Longmans, London, 1961. 70*s*.

This accurate, clearly written, and beautifully produced book, can be regarded as a definitive statement of the science of the measurement of interfacial angles in crystals. Full descriptions are given of different methods of deriving the crystal constants from the measured angles, and actual numerical examples abound. The numerous diagrams are all clear and elegantly drawn.