CORRESPONDENCE.

TOPAZ AS A ROCK CONSTITUENT.

SIR,—When a paper on the Gunong Bakau topaz and cassiterite[1] appeared in this Magazine for 1916, it was sufficient for the time being to ask you to publish a report[2] to which the author referred in order to show that before I worked out the structure of Gunong Bakau[3] he held the view that the quartz-topaz rock was "topazised granite", and attempted to explain the horizontality of one of the ore-bodies by faults, thrusts, and a landslip, although in his paper he writes of the "important veins intrusive in the porphyritic granite", and argues that because certain topaz-bearing rocks in Germany and elsewhere are considered to be altered granitic rocks, the same origin should be accepted for the Gunong Bakau quartz-topaz rock. I do not propose to repeat the evidence on which my opinion that the topaz is a primary mineral was based, but there are two points of general interest that might be mentioned in connexion with topaz as a rock constituent.

On pp. 300 and 301 of *The Natural History of Igneous Rocks* Dr. A. Harker writes: "Closely bound up with the greisens are the tinstone veins, the cassiterite probably resulting from reaction between the volatile tin fluoride (SnF_4) and water. The destructive action of fluorides is exceedingly energetic. At Geyer, in Saxony, granite is locally converted to a rock containing more than 90 per cent of topaz," and quotes as his authority regarding the Geyer rock Salomon & His' paper in the Zeit. deutsch. geol. Gesellschaft, vol. xl, pp. 570–4, 1888. Dr. Jones follows Dr. Harker in making a similar reference to these authors; but the fact remains that whatever may be the truth about the origin of the topaz they described, Salomon & His did not write anything in that paper that justifies their being quoted as authorities for its formation by the destructive action of fluorides. On the contrary, Salomon and His made it clear that they considered the topaz in the greisen to be the primary topaz that occurs in the granite. They mentioned topaz as being widely distributed as a constituent of the granite stocks, although it seldom becomes a prominent constituent. They said that one must expect the topaz, so characteristic of the granite, in the greisen as well, and on pp. 573 and 574 they described aggregates of topaz with a little felspar and mica which become converted by decomposition into aggregates of 90 per cent topaz with a little kaolin and ferrite. According to these authors the topaz was not formed by pneumatolysis. Never having seen the Geyer or indeed any German greisens in the field, I am not in a position to say whether Salomon & His were correct in their view or the reverse.

[1] Dr. W. R. Jones, "The Origin of Topaz and Cassiterite in Malaya": GEOL. MAG., 1916, pp. 255–60.
[2] Loc. cit., pp. 453–6.
[3] Quart. Journ. Geol. Soc., vol. lxx, pp. 363–81, 1914.

On p. 379 of my paper on the Gunong Bakau rocks I pointed out that without segregation one could not expect to have a rock very rich in topaz. In a pure orthoclase magma the 18·4 per cent of alumina could only produce 32·6 per cent of topaz if attacked by fluorine unaccompanied by more alumina. Dr. Jones produces evidence to show that alumina was introduced into some greisens.[1] I do not know how the rock-sampling was carried out in the cases quoted, nor on how many analyses the results are based; but I do not wish to question the increase of alumina in any of the altered rocks in the table on p. 260 as compared with the unaltered granite. The greatest increase is 2·09 per cent, which, added to the alumina of a pure orthoclase rock, gives a possible 36·3 per cent of topaz, which is still very far short of 90 per cent, and we are not dealing with pure orthoclase rocks. There can be no question that topaz does occur as an original rock constituent. The Meldon aplite, for instance, has been described anew recently,[2] in which topaz is associated with lepidolite, tourmaline, and fluorspar, among other minerals. There is no doubt in my mind that it occurs also as a pneumatolytic alteration product. Each case must be decided on the local evidence.

J. B. Scrivenor.

YUNNAN CYSTIDEA.

Sir,—A few comments are necessary on Dr. Bather's letter in the March number of this Magazine (p. 143) in reply to my remarks on his articles on Yunnan Cystidea. Especially is this the case with regard to the diplopores in *Sinocystis*. Firstly, it must be borne in mind that the figured specimens which were lent to him for a short time for the purpose of making casts for the British Museum constitute the only material on which he can base his conclusions, while I had three times the number of specimens for study for two years. Secondly, it has not been mentioned that these figured specimens before being drawn or sent to him had been cleaned under my eyes with a weak acid solution, which I then observed attacked and partially dissolved a few of the tubercles, so as to remove the thin covering layer of epistereom in some cases and thus expose the pores. Thirdly, the other specimens of *Sinocystis*, numbering over twenty, which Dr. Bather never saw, were examined by me as they came fresh from their limestone matrix, unaffected by weathering, untouched by any solvent, and often only partly exposed. These did not show any pores on the hundreds of tubercles which I scrutinized, except where the tubercles were obviously injured. Fourthly, his statement that on removing a piece of the matrix from one of my figured specimens there was disclosed a tubercle exhibiting the minute pores completely confirms my experience that there is extreme difficulty in getting rid of the closely adherent matrix without damaging the surface, and thus his discovery is of

[1] Op. cit., pp. 259–60.
[2] Geol. Mag. 1919, pp. 41–2.