journal, I send you herewith enclosed the copy of a passage which appears as a footnote.

L. L. FERMOR.

GEOLOGICAL SURVEY OF INDIA,
CALCUTTA.

Copy of footnote on page 10 of *Memoirs G.S.I.*, lxx, part i :—

"I propose as far as possible to avoid the use of the terms autoclastic and epiclastic, for obviously they are unfortunate as at present applied. On the analogy of the use of the terms 'syngenetic' and 'epigenetic' as applied to ore deposits, usages which appear happy, the term *synclastic* should be applied only to a sedimentary conglomerate, as the clastic character of the pebbles was produced contemporaneously with the clastic character of the associated sand and clay, and the pebbles were deposited contemporaneously with the associated sand and clay; similarly the term '*epiclastic*' should be applied only to a conglomerate in which the clastic character has been superposed on the rock subsequent to its formation, i.e. to crush-conglomerates. The term *autoclastic* should be applied to rocks in which the clastic character has been produced by the rock itself. It is difficult to imagine the process by which this might happen, but perhaps flow-breccias may be regarded as autoclastic breccias because they are formed by the flow of a portion of the rock itself. A crush-conglomerate is obviously not an autoclastic one."

AMMONITE TERMINOLOGY.

SIR,—In Part II of my work on the Upper Jurassic Invertebrate Faunas of Cape Leslie, Milne Land (*Medd. om Grönland*, vol. 99, No. 3) recently published, I proposed the new genus *Kochina* for a group of ammonites with *K. groenlandica* nov. as genotype. Unfortunately the generic name is preoccupied (C. E. Resser, 1935), as Mr. Alfred Rosenkrantz of Copenhagen kindly informs me, and I therefore propose to substitute for *Kochina* (Spath *non* Resser) the new name *Laugeites*, gen. nov. the genotype remaining *L. groenlandica* (Spath).

L. F. SPATH.

THE NEWRY IGNEOUS COMPLEX.

SIR,—In a *Memorandum* on an excursion to the Newry Igneous Complex, published in your June number, Professor E. B. Bailey records certain conclusions which differ from those reached by Miss Doris Reynolds. The closing sentence reads as follows: "In writing this memorandum, I have not consulted other members of the party, but I am sure from discussion during the excursion that

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most of them have come to similar conclusions." While it is true that scientific progress depends on evidence and not on votes, it seems to me that this attempt to influence opinion by invoking weight of numbers is unfair in that it involves reputations as well as hypotheses. I have therefore ascertained the facts by communicating with the members of the party—other than Professor Bailey himself and one member who is abroad. It turns out that it is not true " that most of them have come to similar conclusions ".

In a recent article on " The Idea of Contrasted Differentiation " (GEOL. MAG., 1936, pp. 228–238), I have already given reasons for differing from Professor Bailey in the field of petrogenesis. Fortunately I need not enter into the technical details of the additional points raised in the *Memorandum,* since Miss Reynolds has clearly presented a host of relevant facts (in press, GEOL. MAG., 1936), which, to my mind, effectively dispose of the criticisms offered. I wish, however, to place on record that since Miss Reynolds completed her field work on the eastern end of the Newry Complex, I have spent nearly three weeks on the ground, and examined all the thin sections that have been cut, as a result of which I am in entire agreement with her statement of the evidence, and with the straightforward and objective interpretation of that evidence which she has given.

Dr. Alfred Brammall, who collected critical specimens from the Newry Complex, and has since examined them—particularly for evidence of the syntectic processes postulated by Miss Reynolds, has invited me to add that he, too, is in entire agreement with her interpretation.

ARTHUR HOLMES,
University of Durham.

SCIENCE LABORATORIES,
SOUTH ROAD,
DURHAM.
18th June, 1936.

ON BABABUDANITE.

SIR,—I have read the letter of Mr. B. Rama Rao in the April number of the GEOLOGICAL MAGAZINE, and though reluctant to take up space on a matter of somewhat local importance, I consider it necessary to correct certain wrong impressions that this letter gives.

Mr. Rama Rao's letter would seem to suggest that I was not aware of Jayaram's statement that bababudanite was probably a secondary metamorphic mineral. I have discussed this matter elsewhere [1] and so shall content myself here by stating that Jayaram's was a mere suggestion unsupported by either field or microscope evidence. I

[1] *Current Science*, iii, 1935, 608.

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