

But although the author did not see these schists he discovered the very interesting section in the cliff below the Lazaretto shown in his text-fig. 3. The succession there is read by him as (4) normal White Limestone underlain by (3) a few inches of friable broken-up yellowish dolomitized limestone, which in its turn overlies (2) a grey and streaky or white marble 1-2 feet thick. This marble passes down into (1) a "hornfelsed and darkened limestone, veined and fractured". The author considers that all these beds belong to the White Limestone formation, and that the lower beds have been thermally metamorphosed by "probably some intrusion not far away". On the other hand, my own interpretation of his text-figure is that the base of the White Limestone should be drawn below the few inches of dolomitized limestone, and that the marble and hornfelsed beds below it are members of the Basal Complex on which the limestone rests unconformably, as it does on the hornblende-schists near by.

How otherwise is the presence of the hornblende-schists to be explained? The White Limestone in this area forms a gentle dome with dips of  $25^{\circ}$  to  $30^{\circ}$ , and has never been subjected to severe earth-movement, yet in the core of the dome immediately underlying the normal limestone we find thoroughly foliated schists which give evidence of two periods of dynamo-metamorphism and a much longer geological history.

I consider that the author has failed to prove his case for the Tertiary age of the granodiorite, and, in my opinion, can never hope to do so. He himself notes (p. 263) that the White Limestone can be found within a few inches of this great plutonic mass with no more change than a little dolomitization, although there is a wide zone of hornfelsing not far away. He offers the explanation (without giving any proof) that the hornfelsed rocks have fallen into or become pendent in the granodiorite, while the unaltered limestone is "at the outer contact"! My own view is of course that the plutonic rock was already unroofed when the White Limestone was deposited and that both it and the hornfelsed beds are of much earlier age and are part of the Basal Complex.

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#### NOMENCLATURE OF CONGLOMERATES.

SIR,—In a study entitled "An attempt at the Correlation of the Ancient Schistose Formations of Peninsular India", the first part of which is now in the press (*Memoirs Geological Survey of India*, lxx), I have had occasion to discuss the nomenclature of conglomerates, and as this may prove of interest to the readers of your

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

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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 auto-clastic 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