

similar to the Strathspey Complex since it shows great similarities in field appearance and structural environment.

The core of the Strathspey Complex represents a Forceful Intrusion (Read, 1961), rather than an "Older Granite" (migmatite). Miller and Brown show that many Caledonian granites were emplaced about 400 million years ago. It is suggested that the older dates obtained from specimens from the core of the Strathspey Complex, 430–420 million years (Table 2(d), p. 117, Miller and Brown) indicate that granite intrusion commenced earlier at the level now exposed in Strathspey, possibly because it represents a level deeper in the tectogene.

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SILURO-DEVONIAN BOUNDARY

SIR,—In spite of Dr. Tarlo's flattering picture of a "courageous stand", I must explain that my paper had nothing to do with the Ludlow Research Group. That most informal association of workers interested in the Silurian and Devonian has, to my knowledge, no creed which I may retract. My paper was intended to be an objective review of a problem which must be settled eventually by international agreement. Though I have discussed these matters with Dr. Tarlo, I had not seen the introduction to his Polish monograph before I submitted my paper to you. Having read it I am still not "convinced by the case" which he presents. Indeed I have long suspected that he views the Welsh Borderland through Old Red coloured spectacles. I still see there a marked change, both faunal and lithological, at the Ludlow Bone Bed and, not being a student of fossil vertebrates, I find the sharp change in the latter at the top of the Downtonian regrettably difficult to pin down.

As to the views of Dr. Jaeger, I can only say that I suspect his preference to be, like that of other Central and East European colleagues, for a boundary taken at the disappearance of the monograptids. However, after long discussions with him, I came to believe that he favoured a compromise at the base of the *Monograptus uniformis* Zone if a compromise were necessary.

I cannot see that a changed position for the Siluro-Devonian boundary will "facilitate direct correlations" between British and Central European strata. It is perhaps rather more the case that an increasingly accurate correlation, based upon all available kinds of evidence, permits the possibility of a reasoned and lasting decision upon a standard for the boundary.

In his letter, and at greater length in his monograph, Dr. Tarlo refers to the problem of naming Post-Ludlovian, Pre-Gedinnian strata. It should be remembered that no problem exists where the Ludlovian is followed directly by the Downtonian and none in Central Europe, where the Czech workers already have a complete stage terminology. The problem exists only in situations such as that of the Calcaire de Liévin, where there are Post-Ludlovian marine strata which are not of Bohemian (or Downtonian) facies. It is in these situations that Boucot has employed the term Skalian. In his monograph Dr. Tarlo objects to this on the grounds of the prior and different usage of the "étage de Skala" by Kozłowski. Whatever be the answer to this argument, I believe it would be even more confusing to employ Ludlovian for

such marine equivalents of the Downtonian. We have already satisfactorily defined and standardized the Ludlovian succession in the Welsh Borderland and its boundary with the Downtonian.

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GRAVITY SLIDE DEPOSITS IN TIMOR AND ECUADOR

SIR,—The account of the Bobonaro Scaly Clay by Mr. Audley-Charles (1965) was especially interesting because of the similarity which was suggested throughout, with the Clay Pebble-Beds of Ecuador (Barrington Brown and Baldry, 1925; Barrington Brown, 1938; Marchant and Black, 1960).

The uniformity and waxiness of the clay matrix, the characteristic "jigsaw puzzle" type of weathering, the presence but erratic distribution of exotic blocks, the variable thickness of the formation and the evidence for its rapid accumulation—all seem similar. The differences appear to be that the Bobonaro Scaly Clay is varicoloured and contains a greater age-range of exotic material, whereas the Ecuadorian Clay Pebble-Beds is uniformly grey, contains, so far as is known, little exotic material greatly older than itself and, of course, is liberally provided with the characteristic clay pebbles. None of these differences seems to be anything more than one would expect from differences in provenance, especially as the clay pebbles can be explained as lumps which have retained their coherence during sliding.

Mr. Audley-Charles stresses the point that the Bobonaro Scaly Clay is not associated with turbidity deposits in the way that occurs in the Wildflysch of the Swiss Alps. It may be thought that here is another difference from the Clay Pebble-Beds which certainly are associated with turbidity deposits: from the published accounts (*op. cit.*) it may not be realized that these turbidity deposits are not interbedded with the clay pebble material. This has to be recognized clearly: the turbidity deposits everywhere overlie the Clay Pebble-Beds and rest on their uneven upper surface without mechanical break. It merely seems, then, that in Ecuador typical flysch deposition followed the gravity slide, whereas in Timor the Viqueque Formation which rests unconformably on the Bobonaro Scaly Clay, was presumably not of this nature, though it is not described by Mr. Audley-Charles.

Probably the only reason why the Ecuadorian deposit was not long ago recognized as due to gravity sliding and is still not always accepted as such, is because it is poorly exposed and most of its occurrence has to be deduced from subsurface data. If it was so well exposed as the Bobonaro Scaly Clay evidently is, there could not have been so much argument about its nature, nor would its association with enormous masses of contorted sandstones and clays have been so puzzling and misleading, to the extent that it was originally attributed to fault brecciation.

A further difficulty in discussing the Clay Pebble Bed has always been to find any description of other deposits which seemed to be convincingly similar. This has now been removed by Mr. Audley Charles' account.

Finally, now that continental drift is such a respectable idea even if the details may still be argued, it is worth drawing attention to the considerable similarity between the locations of these two deposits on the unstable edges of continental masses. Most other gravity slide deposits in the Alps, Appenines, Canada, Trinidad, and Venezuela are no longer associated so clearly with the present-day advancing edges of the continents, but Ecuador may be said to be classically placed with the expanding Pacific floor disappearing eastwards under the advancing coast of South America, and it is legitimate to suppose that in the Miocene the situation off the Javan-Timor island arc was quite comparable and that the Indian Ocean floor was expanding northward under