

Letters to the Editor

THE SILURIAN-DEVONIAN BOUNDARY

Sir,

I am in sympathy with many of the views expressed by Westoll *et al.* in their letter on the Silurian-Devonian boundary published in vol. 127 part 3 of the *Journal*, and in particular with their contention that the claims of a boundary at the base of the *M. ultimus* Zone were not given sufficient consideration by the Silurian-Devonian Committee of the International Commission on Stratigraphy. Nevertheless, this committee finally voted almost unanimously in favour of a boundary at the base of the *M. uniformis* Zone and I believe that a world-wide referendum of interested geologists would also show overwhelming support for this boundary. It is not popular in Britain because of the extreme difficulty in recognising the equivalent level in our continental development but it is not to be expected that a chronostratigraphic boundary will be an easily mappable horizon throughout the world. We tolerate in this country a Permian-Triassic boundary which lies somewhere in the middle of our New Red Sandstone and we must now come to accept a Silurian-Devonian boundary which can only be vaguely positioned within hundreds of feet of poorly exposed and sparsely fossiliferous Old Red Sandstone. Stratigraphical boundaries are essentially arbitrary and the most important criterion is international uniformity in usage. This uniformity can only be achieved by the sacrifice of individual and national preferences and I consider that the interests of stratigraphy are best served in this instance by acquiescence in the international decision on the *M. uniformis* boundary.

My support for the final decision of the Silurian-Devonian Committee and my respect for the value of its achievements does not mean, however, that I entirely approve of the way in which the decision was reached. The committee then consisted of about 32 members from 16 countries. Most of its members (if not all) were appointed by virtue of their personal expertise and competence and not as official representatives of national Silurian-Devonian committees. They were evidently under no obligation to make progress reports to other interested geologists in their own countries or to solicit their opinions. One unfortunate outcome of this procedure—or lack of procedure—was the inadequate representation of the views of British geologists on a subject of particular relevance to British stratigraphy. The three British members on the international Silurian-Devonian Committee were only required to express their individual opinions: two voted in favour of the *M. uniformis* Zone and one against. The Stratigraphy Committee of the Geological Society (a committee which might reasonably be expected to express the majority view of British experts) made their remarkable submission to the International Geological Congress in Prague recommending the adoption of the boundary at the base of the *M. uniformis* Zone. A few months previous to these happenings, I circulated most of the British geologists concerned with this problem. Twelve of them expressed their disapproval of the immediate adoption of the *M. uniformis* boundary and generally favoured a more thorough investigation of the possibilities of the base of the *M. ultimus* Zone. Only four

geologists (including the two committee members) were definitely in favour of the *M. uniformis* boundary. Similarly, there were general expressions of dismay and disapproval that the *M. uniformis* boundary appeared to be a *fait accompli*—even before Leningrad. And so it turned out to be. At the Leningrad symposium (which was by invitation only) there was no discussion of the boundary problem at the main meeting and the Silurian-Devonian Committee reached its predictable decision before all the papers had been heard.

The moral of this story is that both national and international committees are liable to separate themselves from the body of geologists they are appointed to serve and represent (not to rule). The deliberations of such committees should be given wider publicity and the opinions of non-members actively sought and published in circulars of general availability. Something of this kind has already been done by Professor Hedberg in his circulars for the International Subcommittee on Stratigraphic Classification and also, to a lesser extent, in the more recent circulars of the Silurian-Devonian Committee itself, although in neither case are the typescript circulars readily available on library shelves.

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THE SILURO-DEVONIAN BOUNDARY REVISITED—
A PERSONAL RECOLLECTION

Sir,

In view of the plea by Westoll *et al.* (1971) for the retention of the Ludlow Bone Bed as the Siluro-Devonian boundary, it might be instructive to outline the story of the Ludlow Bone Bed versus the *Monograptus uniformis* zone.

In 1960 Allen and I began our joint paper on the Downtonian and Dittonian facies of the Welsh Borderland (Allen & Tarlo 1963). I wrote the discussion on the Downtonian-Dittonian boundary strongly supporting Dineley's (1951) view on the subject. On palaeontological grounds I would have preferred to have drawn the Siluro-Devonian boundary there. However, this suggestion was deliberately omitted since it would have detracted from the main sedimentary thesis of the paper, by virtue of the discussion it would certainly have provoked.

But in my monograph (Tarlo 1965a) written in 1963, I explicitly argued that the Siluro-Devonian boundary should be drawn at the revised Downtonian-Dittonian boundary.

Hermann Jaeger, of Berlin, wrote to me on 18 September 1963, as follows, 'I am of course glad to hear that you are placing the Downtonian in the Silurian as geologists almost all over the world have done in the 20th century until 1950 . . . I wonder how other British authors, particularly the members of the Ludlow Research Group, will view your procedure? The upper limit of the Downtonian as defined by Allen & Tarlo (1963) may more or less coincide with the border between $e\beta_2$ and $e\gamma$ ' (i.e. Pridolian and Lochkovian, or the base of the *Monograptus uniformis* Zone). Hence, in 1963, Jaeger and I were in close agreement and

Jaeger (1964, footnote p. 45) suggested for the first time that the revised Downtonian-Dittonian boundary and the base of the *Monograptus uniformis* zone should be taken as the Siluro-Devonian boundary. Indeed in this footnote he stated that Allen and Tarlo had drawn it at the Downtonian-Dittonian boundary. Jaeger explained this slip in another letter (10 Oct. 1964). 'The joy on your statement in the letter of September 1963 made me so enthusiastic that, when running through the paper of Allen and Tarlo, I completely overlooked the fact that you avoided discussion on the Siluro-Devonian boundary, although the authors' characteristic of the refined Downtonian was in itself suggestive for its placement in the Silurian. I regret that misunderstanding. Now I am glad that in your forthcoming paper you give an unequivocal statement concerning the assignment of the Downtonian to the Silurian . . . I hope that your placing of the Siluro-Devonian boundary on the top of the Downtonian sensu Allen and Tarlo will find friends also in England.'

At this time the Ludlow Bone Bed was still generally accepted in Britain as marking the base of the Devonian (Holland, Lawson & Walmsley 1963).

During 1964 the Council of the Geological Society decided that a palaeontological volume should be the sequel to 'The Phanerozoic Time scale,' an Editorial sub-committee was set up and 'The Fossil Record' was finally published in 1967. One of the thorny problems faced by this committee was the establishment of an agreed stratigraphical framework. The base of each division was defined as being at a stated horizon; all definitions were to be in marine successions and wherever possible should be based on planktonic or nectonic organisms. In the original list of 74 divisions only two did not conform to this: the base of the Ludlovian and the base of the Gedinnian. The latter was defined as the Ludlow Bone Bed with appropriate Golden Spike designated.

I proposed (27 October 1964) that the base of the Gedinnian be the '*Monograptus uniformis* zone (approx. Pridoli $\epsilon\beta_2$ /Lochkov $\epsilon\gamma$ boundary).' After initial opposition from Holland this proposal was accepted on 18 December 1964.

The following year Holland (1965a), while professing his personal preference for the Ludlow Bone Bed, concluded 'a feasible suggestion might be to establish a standard at the base of the *M. uniformis* zone . . . In the vertebrate sequence it would approximate to the important Downtonian-Dittonian change in the sense of Allen & Tarlo (1963).'

This evoked an immediate response from me (1965b) congratulating Holland on retracting one of the tenets of the Ludlow Research Group's creed and expressing satisfaction that he had been convinced by Jaeger's arguments on graptolites and mine on the vertebrates. This elicited a sharp retort from Holland (1965b) and a further letter from Jaeger (7 Dec. 1965).

'As to the positioning of the boundary I am in complete agreement with you, as you know. After discussions with many colleagues, also with the Czechs, an international agreement placing the boundary at the base of the *uniformis* zone appears to be possible.'

Later Earp (1967) considered Holland and I had 'already done disservice to the international debate.' Potter (1968) opposed the *uniformis* boundary, while Warren (1969) on behalf of the Institute of Geological Sciences stated that 'we

do not at present feel justified in redefining the Siluro-Devonian boundary in Britain, and we shall, as a matter of policy, in the immediate future at least, continue to use the Ludlovian-Downtonian boundary as the Siluro-Devonian boundary.'

As is now well known, at the International Symposium on the Devonian System held in Calgary, 1967, it was agreed that the *Monograptus uniformis* Zone should mark the base of the system. This was similarly recommended at the Third International Symposium on the Siluro-Devonian boundary in Leningrad in 1968. The British Silurian Sub-Committee in their report (Cocks *et al.* 1971) evidently concur with these decisions. Thus during the last decade we have witnessed a complete change in attitude towards the Ludlow Bone Bed.

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THELODONTS AND THE SILURIAN-DEVONIAN BOUNDARY

Sir,

The discussion by Westoll *et al.* (1971) of the Siluro-Devonian boundary and their opinion that the Ludlow Bone Bed should be retained as the boundary is interesting. The Ludlow Bone Bed appears to coincide with the base of the Pridolian—the *Monograptus ultimus* Zone. However the Siluro-Devonian boundary in the Bohemian succession is not placed at the base of the Pridolian but at that of the *M. uniformis* Zone. Also the base of the Pridolian cannot be pinpointed in many sections. The retention of the Ludlow Bone Bed as the boundary only continues the fifty year old problem of correlating Silurian and Devonian rocks.

Martinsson (1969) provided a valuable discussion of the opinions of British and continental workers. He concluded that the stratotype could be retained in the Welsh Borderland, and that the base of the Devonian should be defined with reference to the Barrandian Series, at the base of the *M. uniformis* Zone. He added that the Downtonian-Dittonian boundary as redefined by Allen and Tarlo (1963) came as close as possible to the System boundary.

As Westoll *et al.* point out the redefined Downtonian-Dittonian boundary is marked by the incoming of the *Traquairaspis* faunas and the concomitant change to entirely freshwater environments in the Welsh Borderland. This has been accepted by many continental workers as a useful definition, and correlation is possible with the East Baltic successions and the Beyrichienkalk (Karatajute-Talimaa 1968). The objection raised, that *Traquairaspis pococki* may occur lower in the Downtonian is reasonable but as yet there is no evidence of such occurrences. Admittedly the evidence of one genus of organism is not enough. Furthermore traquairaspids have been found throughout the Silurian of the Canadian Arctic, (Denison 1963, Turner & Dixon 1971 in press). The faunas may have originated in that area and migrated into the European area during the Upper Downtonian.

There is, however, a new line of evidence from thelodont remains, scales of agnathan fish which occur in many facies throughout the Silurian and Devonian. These have been found in England, Scotland, Norway, Sweden, Lithuania, Latvia, Estonia, Ukraine (Podolia), North Germany, Poland, Siberia, the north east coast of Canada, in the Arctic islands, and Australia. I have studied these in a continuous sequence in the Welsh Borderland and a series of 'zones' has been established which can be correlated with the succession known from incomplete sections and borehole information elsewhere.

The zones are as follows:

IV	<i>Turinia pagei</i> assemblage	Dittonian
III	<i>Goniporus</i> / <i>Katoporus</i> / <i>Logania kummerowi</i> assemblage	Upper Downtonian
II	Acanthodian scales only (thelodonts absent?)	Middle Downtonian
I	<i>Thelodus parvidens</i> assemblage	Lower Silurian to Lower Downtonian

The zone of *Thelodus parvidens* ranges from well below to well above the Ludlow Bone Bed which, as Allen & Tarlo (1963) showed, is only one in a series of many bone beds and is of very limited extent, being diachronous in nature (see also Martinsson 1969, p.155). The major changeover in thelodont faunas in the Welsh Borderland and elsewhere occurs with the incoming of the *Turinia pagei* fauna in nearly all sequences studied, and now this form has even been found in Australia and Arctic Canada, as well as being known from Spitsbergen, England, Scotland, Ukraine (Podolia), Lithuania, and North Germany. This fauna appears to have extended all over the Old Red Sandstone Continent. Previous to this there were at least three faunal 'provinces' into which thelodonts and other

faunal elements fitted (Turner 1970, 1971). I do not advocate the acceptance of the Downtonian-Dittonian boundary on purely arbitrary grounds. There does seem to be a significant change occurring at this point which affects the fish in the Old Red Sandstone areas, and this appears to be very close to the *M. uniformis* zone. There was an event at the Ludlow Bone Bed level but this horizon is almost certainly diachronous. There is a large unconformity in S. Wales, and only a thin bonebed in the Welsh Borderland. The evidence from many groups of animals studied in the detailed continuous succession demonstrates that the Ludlow Bone Bed is not a significant horizon except perhaps as a local facies change. The evidence of spores suggested as useful to the discussion is probably very unreliable because spores would be very much controlled by environmental conditions. This has been tabulated by Richardson & Lister (1969) for the Welsh Borderland. They show nine species which traverse the Ludlow Bone Bed, one only apparently becomes extinct at that point, 15 others are confined to the Downton Series, where there is a facies change. Six become extinct at the base of the Psammosteus Limestone Group, and eight new groups enter at the base of the Psammosteus Limestone Group. This is only enough to delineate the Downtonian, which is useful, as a separate stage, and does not provide sufficient evidence to use the Ludlow Bone Bed as a good system boundary.

The remaining problem is the recognition of a boundary in the field. Allen & Tarlo (1963) when they redefined the Downtonian-Dittonian boundary did not actually designate a level. This however, has been possible from the work on thelodonts, and has been supported by the latest geological survey of the classic area of Brown Cleve Hill (Greig *et al.* 1968). There is a major cornstone conglomerate about 20 m below the local Psammosteus Limestone in this area which marks the advent of the *Turinia pagei* fauna. This had been utilised by the Geological Survey as the Downtonian-Dittonian boundary as a mappable horizon. The location of a boundary in the field would be no less difficult in a continuous succession such as the Bohemian graptolite sequence, where there is no well defined lithological boundary. This is not a sound argument for retaining the Ludlow Bone Bed because, even if it were the only Bone Bed in the entire sequence, it would still be only of local use.

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