



Interactive  
Comment

***Interactive comment on “Upper Pliensbachian – Toarcian (Jurassic) palaeoenvironmental perturbations in a temporal and regional context: an extended  $\delta^{87}\text{Sr}/\delta^{86}\text{Sr}$ ,  $\delta^{13}\text{C}$  and  $\delta^{18}\text{O}$  belemnite isotope study from Bulgaria” by L. S. Metodiev et al.***

**S. Hesselbo (Referee)**

stephen.hesselbo@earth.ox.ac.uk

Received and published: 16 March 2012

This study presents new chemostratigraphic data from the Late Pliensbachian to Late Toracian strata of an eastern European location. The new data comprise strontium, carbon, and oxygen isotopes from belemnites and complement existing similar datasets from other successions of the same Early Jurassic age. The time interval at

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



the focus of the study is important because it includes the well-known Toarcian Oceanic Anoxic Event (T-OAE). The stratigraphic context of the fossils from which the data are derived is well documented with the evident reworking and stratigraphic condensation being highlighted. The overall conclusions are what one might have predicted from the outset – the chemostratigraphic patterns are very similar to those found elsewhere in Europe and, indeed, in other parts of the world. The conclusions (at least as set out in the conclusions section) are sound and are well supported by the data.

The principal challenge for this study, well recognised by the authors themselves, is that the succession is highly condensed. Therefore the sections selected for analysis are not those that would ideally have been chosen if the aim were purely to define the marine chemical history through this time interval. Instead, the value of the study lies in what it tells us about the chemostratigraphy of a particular region for which few data presently exist.

That said, I do think that there are a couple of quite significant issues and apparent contradictions that need to be addressed if a revised version of this manuscript is published:

1. Negative carbon-isotope excursion in the lower *falciferum* subzone (equivalent to the T-OAE).

The abstract and text state that the negative excursion is not present (page 316, lines 11-14, page 335, line 14), whereas the conclusions (page 337, line 20) imply the opposite. The new data on the other hand (figure 8, right column) are ambiguous because of a lack of sufficient sample resolution within the relevant interval. It is also a bit surprising that the negative excursion is still regarded as in some way ‘controversial’ (e.g. page 335, line 2) given its universal and unambiguous presence in all high-resolution records (marine, terrestrial, carbonate, organic, both hemispheres) except in belemnites, as comprehensively documented in many references cited in the ms.

2. Reversals in strontium isotope trends.

It is well established (references cited in ms) that there is a trend towards more radiogenic Sr isotope values in seawater through the Toarcian. In several places the manuscripts identifies anomalous ‘peaks’ that are explained by ‘condensation’ (e.g. page 328, line 11) and/or diagenesis. Diagenesis may certainly explain the anomalous values, but how would condensation work? Condensation could only steepen the isotope curve as plotted against sediment thickness. Apart from diagenesis, reworking of much older belemnites is a possible, if rather unlikely, explanation.

3. Ages and durations of subzones based on the strontium isotope record (Section 5.2.5).

I find this section of the discussion to be confusing. On page 331, lines 22-25 it is stated that “They [McArthur et al. 2000] demonstrated that if the rate-of-change of marine  $87\text{Sr}/86\text{Sr}$  and the sedimentation rate remain constant for a given stratigraphic interval, then the change of  $87\text{Sr}/86\text{Sr}$  with time is very close to being linear.” If one assumes that the rate-of-change of marine  $87\text{Sr}/86\text{Sr}$  remains constant then, as a matter of definition, the change of  $87\text{Sr}/86\text{Sr}$  with time is linear (and sedimentation rates are irrelevant).

Furthermore, numerous conclusions are drawn concerning durations of ammonites zones and subzones that seem to be based on questionable assumptions. In the caption to figure 7 it is stated that “The duration of the various ammonite biozones . . . . have been calculated following the methods outlined in detail in McArthur et al. (2000). Overall it appears that the duration of the ammonite biozones in Bulgaria confirms the results from the UK sections”. This confirmation is not surprising given the fact that the same assumptions were made in both studies, with the only possible differences in result deriving from uncertainties in biostratigraphic correlation, diagenetic alteration, or different numerical ages assigned to tie points. A much more significant determinant of duration is the assumption that the rate of change of Sr isotopes through time has been linear – an assumption not accepted by numerous authors, such as Jones et al. (1994) or Cohen et al. 2004. *Geology* v. 32 no. 2 p. 157-160 doi: 10.1130/G20158.1, and

others cited and not cited. Indeed, several previous papers, including the examples just given, have explicitly discussed the possibility that the rate of change of Sr isotopes through time in these intervals is strongly controlled by extraordinary changes in continental weathering rates (see also page 335, line 1 where a rapid rise of strontium isotope values is referred to). As a result of this significant problem all of the conclusions of section 5.2.5 are challengeable, and the paper would benefit greatly from more clarity in the discussion of this topic.

#### 4. Figures.

In many ways the figures are magnificent – very carefully constructed and detailed. I think that there is an issue with readability though – almost all of the figures have to be printed at poster size to be legible. There would be great merit in complementing the detailed figures with much simplified versions that can be read at the same scale as the print-friendly version of the ms.

Other more minor issues are as follows:

Page 316, line 13. The excursion is present also in most carbonate records.

Page 316, line 23. I think that you mean ‘i.e.’ rather than ‘e.g.’

Page 318, line 14-15. The last clause of sentence does not follow from first.

Page 319, line 15. In what way is the transgression ‘expanding’? Aren’t all transgressions expanding?

Section 2.2. Does this really need to be a separate section?

Page 326, line 23. Ambiguous ‘they’: reworked ammonites, or ammonites more generally?

Page 335, line 13. Jones et al. (1994) is really the primary reference here.

Page 337, line 25. Spelling of ‘warming’.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

