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Solid Earth
Discussions

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

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The paper of Metodiev and co-authors presents detailed isotope data (C, O and Sr) measured on belemnite calcite in two sections from Bulgaria covering the Late Pliensbachian-Toarcian interval. This is a key interval in the Earth history because several geological (volcanism, sea-level and climate changes, etc.) and biological (ex-

tinctions and diversifications of several invertebrate groups) events occurred. Thus this paper represents an important contribution to our knowledge of Toarcian events mainly because it reports on a region belonging to the Eastern Tethys that is currently poorly studied, much work being done in NW Europe. The strength of this paper is that the geochemical analyses have been conducted in a very accurate paleontological (mainly ammonites) and sedimentological frame. Taphonomic studies coupled to facies analyses allowed a precise reconstruction of the fossil reworking and condensation occurring at various levels in the two studied sections. I much appreciated this coupled chemostratigraphic and sedimentological approach. The paper is well written and elegant in its form. The paper merits to be published because it is novel and can have international audience. I have, however, some major concerns that are listed below and some issues have to be carefully considered by the authors. (1) Literature cited in the paper.

Although the quoted papers and the reference list are up to date, there are several significant papers that were ignored. For instance, in the introduction the authors state: “These major biogeochemical disturbances deeply affected both marine biota and global carbonate production in the shallow and deep ocean". The associated references do not deal specifically neither with marine biota nor with carbonate production. In the last decade, a number of papers have been published on this topics and I would expect the authors to quote some of them. Just a few examples, Macchioni (2002), Macchioni & Cecca (2003), Cecca & Macchioni (2004), Caswell et al. (2009), Morten and Twitchett (2010) describe in detail how the Toarcian crisis affected ammonites and other marine macro-invertebrates; several papers by Bucefalo Palliani and Riding and van de Schootbrugge et al. (2005) depict changes occurring within organic-walled phytoplankton in response to anoxia; I have been myself intensely working on the response of calcareous nannoplankton to the Toarcian event in terms of assemblages and biocalcification, and other authors have approached this topic too (Tremolada et al. 2005, Erba 2006, etc.); Mailliot et al. (2009) and Reolid et al (2012) described benthic forams extinction during the Toarcian event. Blomeier & Reijmer (1999) describe in a great

SED
4, C90–C94, 2012

detail the sedimentary evolution and the demise of a Moroccoan carbonate platform across the Plienbachian and Toarcian.

(2) Ammonite and belemnite preservation and taphonomy.

These sedimentary patterns have been discussed in detail (section 5.1.2) and this is a valuable issue of the Metodiev et al. paper. These analyses revealed that reworking was occurring at various stratigraphical levels. The consequences of the observed reworking on biostratigraphic interpretations and geochemical analyses, however, were not accurately exposed. When establishing the biostratigraphic scheme proposed in the paper, is reworking taken into account? If yes, the authors should display how. In the legend of figure 2, why the authors do talk about APPROXIMATE correlations with the NW European zones? Is this approximation depending on the fact that reworking was acting on the Bulgarian material? And, more generally, how the ammonite zones and subzones in Bulgaria have been established, are they interval zones or range zones? This is an important issue because, as far as I know, for NW Europe range zones were used, whilst based upon the data shown in figures 3 and 5, it seems like the authors based their zones rather on first occurrences of single taxa, so are they using interval zones? This is an important point to be elucidated in order to be confident that comparable zones are compared. In their figure 7, Metodiev and co-authors show Sr isotope data and it is unclear how they have placed their samples with respect to absolute ages. In fact, there is a wide dispersion on the values they have measured. For instance, why have they placed samples at 145.4, 145.5, and 145.6 Ma if the values measured in those samples closely resemble the values measured by McArthur et al. (2000) as old as 145.8 Ma? Is this dispersion due to potential reworking of the Bulgarian belemnites? Or is it the stratigraphy and age model that are problematic? All these points merit to be more thoroughly discussed.

(3) Ages and durations of subzones (Section 5.2.5).

I agree with the conclusions of previous interactive comments about the slight circular

SED

4, C90–C94, 2012

reasoning when comparing the ammonite zone duration with the results of McArthur et al. (2000). Furthermore, other papers have dealt with durations of Early Toarcian interval, including the negative CIE, and those works should be integrated in the discussion (e.g., Mattioli et al., 2004; Suan et al., 2008b; Kemp et al., 2008, 2011, etc.).

(4) Figures 8 and 9.

I cannot understand the choice of the C and O isotope curves shown in these figures. The authors compare their own data to the data obtained on bulk rock from Amellago, on brachiopod calcite from Peniche but not to the high-resolution bulk rock curve from this section. It seems like more sound to show either all available C and O isotope data regardless the support on which these are measured (biogenic calcite, bulk rock), the authors have thus to integrate published isotope data from Italy (e.g., Sabatino et al., 2010) and other regions; or only show data acquired on biogenic calcite (belemnites and brachiopods, but also the isolated microfractions of nannofossils measured by Hermoso et al., 2009). In their present form, these two figures are incomplete.

(5) Carbonate production crisis.

The authors cite in section 6.3 and other parts of the MS the fact that there is a carbonate production crisis occurring in the Early Toarcian time. It is however unclear which is the expression of this crisis in the Bulgarian sections and in what the Bulgarian record is similar or different with respect to already documented records. I agree that the Toarcian carbonate production and its relationship with the C cycle is an interesting subject, but the way this is treated in the paper is anecdotal and a deeper discussion would be welcome.

(6) Minor points.

- Temperature is mentioned in section 5.2.4 but in figures 3 and 5 this is not displayed.

- Figures are very rich in information but very difficult to read.

- The last two sentences of section 5.2.5 are obscure.

SED

4, C90–C94, 2012

- In section 6.2, line 23, it is stated that the Bulgarian sections represents the most easterly Tethyan record of the Toarcian event. Was this area more easterly than the Pindos zone in Greece (see Kafousia et al., 2011)?

- Figures 8 and 9. Please, check the paleogeographical position of High Atlas and Asturias sections.

Interactive comment on Solid Earth Discuss., 4, 315, 2012.