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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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This paper is well written and represents an interesting contribution to our understanding of the spatial and temporal changes in temperature and isotopic composition of marine seawaters during an interval of severe environmental and biotic perturbations. The main interests of the study are that the new geochemical data are presented within

a very detailed sedimentological and taphonomical framework and originate from an area where such data are relatively scarce. In addition, and despite their relatively low temporal resolution, the new data cover most of the Toarcian stage and therefore advantageously complete other less extended records from other areas. The new data therefore give insight into spatial geochemical and thermal gradients developed in the European Epicontinental Seaway during the increasingly studied Toarcian interval.

In its present form, however, the paper presents some ambiguous, incomplete or even incorrect statements that need to be clarified or corrected before publication. Several key assumptions that represent central arguments for their interpretation (e.g., taphonomy of the studied belemnites, age and position of isotopic excursions) need to be strengthened or further discussed. In summary, I think the presented data deserve publication in Solid Earth after relatively moderate revisions.

Major points:

1) Taphonomy of the belemnites and ammonites

The study sites are characterized by very low accumulation rates, a feature that evidently favors fossil collection but can reveal problematic for the interpretation of geochemical data. The authors indeed state that the belemnites from Babinsti show clear evidence for reworking (P327), but this is not further mentioned or discussed in the interpretation of the geochemical data. It would be also useful to know whether similar evidence for reworking has been observed in belemnites from Varbanchovets. If some belemnites are reworked, one should expect that the resulting isotopic profiles likely incorporate geochemical signals of both older and synsedimentary material, with profound implication for their interpretation. For instance, reworking of substantially older material appears feasible in settings characterized by long intervals of non-deposition, and would explain the presence of outliers in the Sr isotope profile. In addition, such reworking would account for the presence of belemnites belonging to the genus Passaloteuthis in the upper Toarcian portion of the study sites (P327), while this genus is

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generally restricted to the lower Pliensbachian-lowermost Toarcian interval in European sites (e.g., Doyle, 1987, 1991). The authors should therefore consider discussing the implications of reworking for the interpretation of their geochemical data. If possible, it would be also ideal to have the name (genus) of the analyzed belemnites in Table 1. Integration of these points would strengthen the interpretation and improve the quality of the paper.

Similarly, a wealth of evidence is presented for ammonite reworking, but there is no discussion about the potential implications for the biostratigraphical assignment of the sections. Can the authors exclude that the biostratigraphic assignment of some zones is biased toward older ages due to reworking? For instance, did the authors observe associations of ammonites from different zones within some horizons?

2) Positive and negative carbon isotope excursions of the Falciferum zone

Given the considerable debate surrounding the belemnite record of the TOAE, one has to be cautious and rigorous about the temporal and stratigraphic context of the data. According to the data shown in Fig 3 and Table 1, maximum carbon isotope values are recorded in the Tenuicostatum zone, and the positive excursion is recorded across the Tenuicostatum-Falciferum zone. The positive excursion can thus hardly be dated as "Late Falciferum", as mentioned at P316, L11. Please correct here and throughout.

In this context it is noteworthy that belemnites and brachiopods from most other sites do record much higher carbon isotope values in the middle to second half of the Falciferum zone, generally reaching values comprised between 4 and 6 permil (e.g., Bailey et al. 2003; Suan et al., 2010; Gill et al 2011). This "long-recognized" positive excursion documented elsewhere is therefore not evident in the isotopic data, and this as much as its "precursor" negative excursion. There are several possible explanations for these "absences", but I am convinced that the relatively low resolution of the sampling combined to the low sedimentation rates should be considered as the most likely causes.

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In this context, I find the statement that the d13C negative excursion is “not evident in belemnite data” (p 316, 335) misleading since 1) the highest values at the Tenuicostatum-Falciferum transition are interrupted by one data point with a value of 1.1 permil that forms a ∼2 permil negative excursion at Varbanchovets (e.g., Table 1), 2) the resolution of the sampling is relatively low (3 samples in the Falciferum zone) and 3) there is no indication of the possible position and thickness of the excursion from other material in the same section. I agree that some previous belemnite d13C records are curious and intriguing, since the negative excursion seems poorly expressed (or even not expressed at all) in belemnites from levels where organic carbon and bulk carbonate do record a marked negative excursion. But I disagree with the statement that the reported belemnite data support the lack of negative excursion as “a consistent feature of this group” (p 335, L24) because there is no coeval, high-resolution organic carbon or bulk carbonate data in the studied Bulgarian sites. The very condensed nature of the study sections greatly enhances the likelihood of “missing” excursions, positive or negative, and the simplest explanation is that the resolution of the belemnites data is too low to fully capture large excursions. I am pretty sure that any bulk organic or carbonate record from the Falciferum zone of the classical Toarcian sites from Europe consisting of five randomly distributed samples would also fail at showing the negative excursion. Please consider revising.

Minor points:

P 317, L13-15: I am not a native English speaker, but I have always been taught that “if” should introduce a condition, not alternatives. Replace by “whether”?

P318, L18: “Early Jurassic” not “early Jurassic”

P323 and throughout: the authors use capitalization and no italicization for ammonite zones and subzones (e.g. Tenuicostatum), which imply a chronostratigraphic value, i.e., the zones are defined by a stratotype. Is there really a stratotype for these zones? If yes, what is the degree of certainty about the temporal (chronostratigraphic) cor-

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relation between the stratotype and the study sites? In addition, some of the employed ammonite zones and subzones have the same name as some other European zones but sometime with a different meaning, which might be another source of confusion. I suggest using italics without capitalization that only imply a formal usage (e.g., tenuicostatum) and that is often preferred for regional subdivisions.

P324, L27: Oxygen depletion, through productivity and preservation, is indirectly linked to sea level but is also influenced by a variety of other factors. It is therefore not necessarily indicative of maximum flooding. Please rephrase.

P330, L10-13: Unclear. Please rephrase.

P337: The deposits can hardly be described as "hemipelagic". Please consider rephrasing.

P337, Point 2: I would use "oxygen-depletion" instead of "anoxic deposition", the latter implying specific oxygen concentrations, a statement that would require corroborating geochemical (e.g. trace-elements) or petrographic (e.g., pyrite framboid) evidence.

P337, point 3: this point of the conclusion is very confusing. The phrasing suggests that there is a negative carbon isotope excursion recorded in the belemnite data, which is in obvious contradiction what is said earlier in the manuscript. Please consider rephrasing.

P337 "warming" instead of "worming"

Figures 3 and 5: Though interpolation lines helps unscrambling trends in scattered data plots, I am not a big fan of "smoothed" interpolation lines (i.e., between isotopic data points), because they are even more misleading than straight lines. I would delete or replace by straight lines, as in Figs 8 and 9.

Figure 3: the scale of the carbon isotope values is inverted. Please revise.

Figure 7: stratigraphic "break" not "brake"

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Figure 8 and 9: merging bulk carbonate and macrofossil isotopic data in the same figure appears inappropriate since they very likely reflect very different signals (e.g., bulk carbonate oxygen isotope ratios are very sensitive to diagenesis). I would only show bulk data for sections where macrofossil data are available for comparison. Anyhow, the obvious absents in the figure are the belemnite data from Yorkshire, which constitute the highest resolution data available so far (see compilation of Gill et al., 2011 EPSL).

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