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Solid Earth
Discussions



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

**Anonymous Referee #2**

Received and published: 13 March 2012

This work is an interesting contribution to the knowledge of the late Pliensbachian-Toarcian record in Balkan Mountains (Bulgaria), and another good input to the discussion of Early Toarcian anoxic event (ETOAE). It is a study supported by an exceptional knowledge on stratigraphy, palaeontology and sedimentology of this interval in the studied area. Besides the regional interest, with a singular stratigraphic conden-



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sation of the whole Toarcian series, data presented in this work allows a remarkable discussion about the ETOAE recorded in the Eastern European sectors. The paper is well organized and geochemical analytical methods, developed in international reference laboratories, seem to be appropriate. For these reasons I recommend its final publication, although only after the following issues have been addressed:

Specific Comments

1. Assumption that Toarcian is subdivided into two substages ("Lower" and "Upper"), contrarily to the standard chart proposed by the International Commission on Stratigraphy (ICS) (e.g. Elmi et al., 1997; Ogg, 2004). I know and understand that authors follow the biostratigraphic scheme of Buckman (1910), and there are some (objective) reasons for this subdivision. However, I think that this assumption generates a large confusion when authors correlate their data with other basins. This is the case of the sentence "...the late Toarcian Variabilis ammonite Zone..." (p.317, lines 10-11) as well as other examples in the text (including titles: e.g. 6.3 Late Toarcian). In the standard stratigraphic chart of ICS the Variabilis Zone corresponds to the Middle and not to the Upper/Late Toarcian.

2. My main concerns are related to section 5.1.1, Sedimentary Record.

p. 324: Lines 4-9: I have two main remarks: First of all, I consider somewhat strange to assume the Toarcian studied sections of Varbanchovets and Babintsi, as good examples of hemipelagic deposits. Despite the occurrence of several marly intervals, the carbonate facies described by the authors, including microfacies (such as packstones) presented in the figures 4 and 6, do not seem to correspond to genuine hemipelagic deposits. Secondly, I think that the following sentence "These sections record a carbonate crisis that is...... and elsewhere in the north-western Tethyan domain of Europe (e.g. Tremolada et al., 2005)" is a clear mistake. The unique period emphasized by Tremolada et al (2005) is the Early Toarcian and not the whole Late Pliensbachian-Middle Bajocian interval. Other papers such as Mattioli et al. (2009 – Global Planetary

Change) confirm the Early Toarcian as a period of decrease of pelagic carbonate production.

Lines 8-27: I think that this section needs to be improved. In the general discussion of the depositional model authors pay particular attention to the ooidal ironstones record in the series. I believe that thin Toarcian record in the studied area may correspond to starvation but the invoked transgressive episode should be better justified, integrating the whole Pliensbachian record. p. 324, lines 25-27 – page 325, lines 1-4: "The thin....during a transgressive episode (Fig. 3). This retrogradation...". I understand the idea presented by authors but from Fig. 3 we cannot confirm the evidence because sedimentary record information about the Pliensbachian is missing. Moreover, any sequential interpretation is made by authors in the figure for the whole Toarcian sequence. Also, authors need to better describe and discuss the question of oxygen-restriction facies. In the laminated facies, organic matter is not quantified and/or qualified. What do these laminated shales in the Falciferum Zone of Varbanchovets section mean? And some other "organic facies" recorded later in the Babintsi section? Are there any total organic carbon data to confirm this sentence?

3. 5.2.2 Isotopic trends in belemnite $\delta$13C There is some confusion in the first part of this section! p.329, lines 15-20 - From figure 3 the Early Toarcian $\delta$13C curve shows: a marked Semicelatum positive excursion; an abrupt negative fall at the base of Falciferum Zone followed by an abrupt increase of $\delta$13C observed in the middle-upper part of the Falciferum Zone. A slight positive excursion seems to be observed in the Bifrons Subzone.

4. 6.2. Early Toarcian: I have the following comments:

p. 335: lines 1 to 8: it is important to be rigorous in the presentation of right references. In the sentence "The T-OAE also coincided with the onset of a rapid rise of 87Sr/86Sr ratios and carbon isotope shifts that include a controversial negative $\delta$13C excursion near the Tenuicostatum/Falciferum zonal boundary followed by a return to heavier val-



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ues in the later Falciferum and early Bifrons ammonite Zones" authors present several references that are very important to the ETOAE discussion but that seem out of context in this part of the text. I remark the cases of: Svensson et al. (2007): this paper is not related with isotopic data; Rosales et al. (2004 Palaeo3; and not 2003): it is related only with oxygen isotopic data; In addition, I realize that in this part of the text there is the absence of other important references such as Hesselbo et al. (2007EPSL), one of the main papers concerning the C-isotopic record around the Tenuicostatum-Falciferum zonal boundary. Interestingly, this reference is also absent in the extensive list of references cited in Introduction (p. 316, line 25 – p.317, line 3).

p. 335: lines 12-14; I consider the sentence "In contrast the belemnite $\delta$13C record shows...but the precursor negative excursion is absent (Fig. 8)" very confusing! Where is it in the Toarcian the mentioned ~2‰ positive excursion? And the precursor negative excursion? In the earliest Tenuicostatum zone or in earliest Falciferum zone (e.g. Hesselbo et al., 2007; Littler et al., 2010 Geol Magazine)? Please, clarify. This information is also contradictory with the C-isotopic record observed in the Falciferum of Varbanchovets section. According to the Fig. 3 a negative excursion is clearly observed in the lowermost (?) Falciferum zone.

5. Conclusions: Some of the presented conclusions seem not to have been sufficiently discussed previously. The second conclusion (lines 17 – 18) requires some precaution. First of all, the "short pulse of anoxic deposition" needs some other evidence (e.g. geochemical). Secondly, the statement "...representing the deeper water succession" is presented for the first time. Please, see text between lines 25-27/page 324 and lines 1-4/page 325. Where does the retogradational phase end? In the Falciferum Zone? Third conclusion (lines 19 – 22/page 337): how controversial negative $\delta$13C excursion? Is this a conclusion based on authors data? Do authors recognize or not the early Falciferum $\delta$13C negative excursion? This question is not clear in the text.

6. Figures Illustrations are of great quality showing a large diversity of data. My main concerns are the size of some figures that are not suitable for the reader. Fig. 2 –

Considering the subject (and title) of the document, the absence of any indication to the upper Pliensbachian in the figure 2 seems strange. Figs 3 and 5 show a large spectrum of data obtained by authors. In the present size these figures are impossible to read correctly. It is necessary to enlarge these figures or to subdivide them into two other figures. Fig. 5 – With all the information available I cannot see the reason to assume the proposed Pliensbachian-Toarcian boundary. Please, put a dotted line in this boundary.

Technical Corrections p.324, lines 1 and 2: Varbanchovets and Babintsi sections

References: Citations in the text but not presented in the References List: p.317, line12: Jenkyns et al. (1997); Jenkyns and Clayton (1997)?; p.317, line13: Metodiev and Koleva-Rekalova (2008), not (2009); p.321, line23: Jones et al. (2000); p.322, line13: McArthur et al. (2002) or McArthur et al. (2000)?; p.327, line7: Doyle and Macdonald (1997)? 1993 such as in the References list. p.335, line8: Grocke et al. (2009) ? 2007 in the References list?.

Presented in the References list but not cited in the text: p.344, line 28: Beerling et al. (2002); p.347, line 21: Jones and Jenkyns (2001).

Page 335, line 26: Al-Suwaidi Figure 8: Jenkyns et al. (2000)? Or Jenkyns et al. (2002)?

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