Solid Earth Discuss., 4, C172–C190, 2012
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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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Response to Reviewer 1 (anonymous)

We appreciate the comments of reviewer 1 regarding our manuscript. Indeed it is important to address the complex Jurassic palaeoenvironmental and stratigraphic issues with multi-isotope and multi-outcrop correlations in order to fully understand the global versus more regional variations in climate and/or ocean water conditions. Below we respond to specific recommendations as outlined by reviewer 1: p. 317 - line 10: We based our sub-stage subdivision of the Toarcian into "Lower" and "Upper" after using the superb, in our view, and well-accepted in the community monograph of Howarth (1992). This is based on the ammonite successions of the classical outcrops in Yorkshire and Somerset in Britain. Since we compare our Bulgarian sections with these particular UK sections, we feel that further subdivision is rather unnecessary and instead of bringing clarity, it creates further confusion. Therefore, we appreciate the comment, but feel that the division into Lower and Upper Toarcian only is more suitable for the aims of our manuscript and that Lower and Upper Toarcian should stay as they are. For clarification, the following text was be added to the caption of Figure 2: "The substage subdivision of the Toarcian in Bulgaria follows that of Howarth (1992) and references therein": The following reference was added to the reference list: Howarth, M. K.: The ammonite family Hildoceratidae in the Lower Jurassic of Britain, Monograph of the Palaeontographical Society London: Part 1: 1-106, pls 1-16 (Publ. No. 586, part of vol. 146 for 1991); Part 2: 107-200, pls 17-38) Publ. No. 590, part of vol. 146 for 1992), 1992. p. 318 - lines 6-15: We agree with this comment and after line 15 of the text the following has been added: Furthermore, our new Sr, C and O isotope data and interpretations are continuation of the works of Metodiev and Koleva-Rekalova (2008), who were the first to apply O and C isotopes to Jurassic palaeoenvironmental issues in Bulgaria based on sections in the Western Balkan Mts. p.324- lines 17-24: This is a valid comment, which aims to bring further clarification into the origin of the iron ooids in the Jurassic sections of Bulgaria. When it comes to origin as volcanic ash, we agree that such textures and deposits may have volcanic origins. However, on the territory of Bulgaria there is a lack of evidence that the shallow marine basins experienced any influence from a volcanic source. Possible reasons for this may be that the volcanic arc source was under water, the prevailing wind direction was opposite the palaeogeographic position of the Toarcian basins of

SED

4, C172–C190, 2012

Interactive Comment

interest and/or simply far away from the studied sections. The following text was added after line 24: In addition, on the territory of Bulgaria there is lack of any evidence that the shallow marine basins experienced input from volcanic ash sourced from nearby arc source(s). p.327- lines 9-20: We appreciate this comment. To identify belemnite guards we used the monograph of Stoyanova-Vergilova (1993). It is now added to our reference list as: Stoyanova-Vergilova, M.: Les Fossiles de Bulgarie, Nikolov, T. (ed.), IIIA, Jurassic, Belemnitida, Bulg. Acad. Sci. Press, Sofia, 212pp., 1993. The following text was added in section 5.1.3, after the first sentence (line 3): We based our belemnite generic identification on the study of Stoyanova-Vergilova (1993). p. 331-332. We agree that our Sr isotope data at the Toarcian–Pliensbachian transition has a nearly identical trend to that reported by McArthur et al. (2000) from the UK sections, even though there are several offsets. Although some of our Sr isotope values are a good match when compared to Italian sections, we feel that the UK record is a more superior and well-dated record from thicker and more complete sequences. Thus, considering our Sr isotope data alone and the thickness of our sections, we feel we are not entirely in a position to address and challenge the well-accepted isotope record as reported by McArthur et al. (2000). When it comes to belemnite rostra preservation, we have listed our arguments for the lack of secondary alterations/diagenetic reworking in section 3 "Materials and Methods" lines 14-18. Moreover, we suggest in our "Conclusions" section 5, that further Sr, C and O studies are required to fully address the issues of isotope cross-correlations between southern Europe and the UK sections. p. 336-line 19. We agree with this comment. The following text is added after line 23: "In addition, the observed 87Sr/86Sr long term increase is also reflected in the fluctuations of the Sr-isotope values derived from sections representing the Mediterranean Realm (Woodfine et al., 2008) and the Panthalassa Ocean (Gröcke et al., 2007)." The following reference was added to the reference list: Woodfine, R. G., Jenkins, H. C., Sarti, M., Baroncini, F., Violante, C.: The response of two Tethyan carbonate platforms to the early Toarcian (Jurassic) oceanic anoxic event: environmental change and differential subsidence, Sedimentology 55, 4, 1011-1028, 2008. Technical Comments: Both of

SED
4, C172–C190, 2012

the issues have been addressed. We have removed the “Whitbyan” and “Yeovilian” from the leftmost column of Figure 2, where we have left only the “Lower Toarcian” and “Upper Toarcian”. We also have corrected all the typos in the text and accepted all of the recommended additions/clarifications.

Response to Reviewer 2 (anonymous):

We are thankful for the comments of reviewer 2 regarding our manuscript. Below we respond to specific recommendations as outlined by reviewer 2: Point 1: We have addressed a similar issue in the response to reviewer 1. For clarification, the following text should be added to the caption of Figure 2: “The substage subdivision of the Toarcian in Bulgaria follows that of Howarth (1992) and references therein”: The following reference should be added to the reference list: Howarth, M. K.: The ammonite family Hildoceratidae in the Lower Jurassic of Britain, Monograph of the Palaeontographical Society London: Part 1: 1-106, pls 1-16 (Publ. No. 586, part of vol. 146 for 1991); Part 2: 107-200, pls 17-38) Publ. No. 590, part of vol. 146 for 1992), 1992. Point 2: p.324 lines 4-9: The first part of the comment refers to whether the studied Bulgarian sections are “good examples of hemipelagic deposits”. We believe that although these may not ideally be “a good example”, based on slow sedimentation rates and the lack of benthic fossils (at least 25% biogenic material present) these sediments are indeed recording hemipelagic environments. Thus, we feel that the term “hemipelagic“should remain. Moreover, the fact that the horizons in question contain packstones is not a good enough argument for removing hemipelagic as a depositional environment, as packstones can form in hemipelagic and even deeper environments too. The word “good” was removed from Section 5.1.1., line 5 The second part of the comment concerns the proper citation of the sources describing the presence of carbonate crisis in the Lower-Middle Jurassic. We agree with reviewer 2 that the work of Mattioli (2009) should be added. “Mattioli et al., 2009” was added instead of the reference Tremolada et al., 2005 on page 324 line 8. The following reference was added to the reference list: Mattioli, E., Pittet, B., Petitpierre, L., Mailliot, S.: Dramatic decrease of the pelagic carbonate production by nannoplankton across the Early Toarcian Anoxic Event (T-OAE), Global and Planetary Changes, 65, 134-145, 2009. The third part of the comment concerns better justification of the idea of transgressive episodes during not only the Toarcian, but potentially the Pliensbachian. However, since the Pliensbachian is missing in the sections of interest, we feel that it will be highly speculative to include information from pre-Toarcian events when deposits recording a potential transgression in this period are not available for study. When it comes for arguments regarding the presence of transgressive episodes in the Toarcian, we have listed these on p. 324-lines 26-27 and p. 325-line1-4. Evidence includes the occurrence of laminated shales and enrichment of organic matter and pyrite aggregates, typical of deep-water strata. Unfortunately, we do not have total organic carbon data or Rock-eval pyrolysis for the bulk sediment samples but do not consider this necessary to determine facies where the facies are so distinctive. Additionally, such analyses are beyond the scope of this paper. Point 3: Trends in δ13C discussed in section 5.2.2. We agree with this comment about the fact that there is a slight positive δ13C excursion in the Bifrons Subzone (Figure 3). We have tentatively (there is only 1 data point) stated so on p. 329 line 27-28. Point 4: Regarding section 6.2. p. 335 lines 1-8. We agree with this comment. Svensson et al., 2007 from p. 335 line 7 has been removed and we have added Hesselbo et al., 2007. This reference is already in our reference list. We also have removed Svensen et al., 2007 from the reference list on p. 349 line 19-21. (Please note that the reference name mismatch Svensson vs. Svensen is originally our typo error). As suggested we have also removed “2003” from the “Rosales et al, 2003, 2004” text used on p. 335-line 6. In the intro section-we have added “; 2007” after “Hesselbo et al., 2000” on line 1 on p. 317. We have rewritten the latter half of section 6.2 to clarify the trends, as seen in our data, and offered two alternatives for the lack of Early Toarcian sharp negative excursion (it is either not there or the sample resolution has missed it). Point 5. We have removed “representing the deep water succession” from our conclusion section 2 on p. 337 line 18. Point 6. We agree with this comment about the size of the manuscript, which makes it difficult for the reader to download the final version of the paper. We believe the issue arises from the size of our figures. We think that the large size on the manuscript is mainly because of two of our figures that are dedicated to the photomicrographic record of the studied sediments, namely Figures 4 and 6. It is in the journal photo size requirements that such figures are submitted in very large resolution and the original submission therefore included figures with sizes >10MB for each figure. We think that the originally large sizes can be shrunk in size by the journal, which hopefully will not alter the original content and the science/evidence provided by these figures. In addition, our manuscript is comparable in size to other manuscripts in the same journal with similar amount of text, data tables and figures. When it comes to the fact that the Upper Pliensbachian is not represented on Figure 2, we have addressed this issue by adding the following text to the Figure 2 caption: "The Pliensbachian is not shown on this figure because of a lack of ammonite evidence." Regarding the format of the figures, we would like to point out that Figure 3 and Figure 5 were shrunk by the journal staff and we had no control on that. We will request that these figures are re-produced as landscape layout and as large as possible. The same is true for Figure 8 and Figure 9. Regarding the P-T boundary on Figure 5 - we have shown it as a dashed line and we believe that the P-T boundary is clearly visible. Technical corrections: On p. 324 section 5.1.1., line 4 – 5, Instead of "sections Varbanchovets and Babintsi" should read "Varbanchovets and Babintsi sections". The reference Beerling et al., 2002 was removed On p. 317 line 1 and line 8 - Jones & Jenkyns, 2001 were added. The rest of the technical corrections concern referencing style/format and have all been corrected exactly as suggested by the reviewer.

Response to Reviewer 3 (G. Suan):

We are pleased for the comments from Guillaume Suan and his lack of anonymity as a reviewer. Below we respond to specific recommendations as outlined: Point 1. In general we agree with this comment. Previously, it became apparent that reworked elements of belemnite accumulations are very common in the Lower Jurassic of Bulgaria (Metodiev and Koleva-Rekalova, 2008). However, there was no clear evidence

SED

4, C172–C190, 2012

Interactive Comment

for reworking in the geochemical data obtained, even from specimens that obviously underwent stronger taphonomic elaboration. As we replied to the similar comment of Reviewer 1, there is a potential the re-elaboration of belemnites to explain the observed chemical trends. However, within the studied sections we did not observe any mixed fossil associations. Although thin, the ammonite associations and co-occurring fossils (belemnites) follow upwards from each other in normal superposition. We believe that the recycling of belemnite rostra is not responsible for the outliers in the isotope trends of the two sections studied. It is probably due to some stronger local environmental controls. Concerning the belemnite genus Passaloteuthis, its highest occurrence in our sections was recorded up to the Semipolitum Subzone of the Bifrons Zone. None among us is familiar with the belemnite taxonomy. Therefore, we are not able to give correctly the names (genera) of each of the rostra analyzed. The determinations proposed are after comparisons of our belemnite specimens with available literature data from Bulgaria and consultation with Stoyanova-Vergilova (1993). Point 2. We agree with the comment that the maximum C isotope values are recorded in the Tenuicostatum Zone and the positive excursion is recorded across the Tenuicostatum-Falciferum zonal boundary: therefore, the positive excursion can hardly be dated as "Late Falciferum". Moreover, belemnite occurrence did not allow us to obtain sufficient resolution from above, in the upper part of the Falciferum Zone. It is better to clarify the interpretation of this part of our dataset that after the positive excursion around the Tenuicostatum-Falciferum Zone boundary the return to heavier δ13C values took place at the base of the Bifrons Zone. Unfortunately we cannot address the problem with the sample density of the Falciferum Zone in Bulgaria based on the sections examined. Until now, this zone in Bulgaria was not described. Usually it is highly condensed (with faunal mixtures with ammonites of the Tenuicostatum and the Bifrons Zone), partly or completely missing from the sedimentary record, or documented in shallower facies (ooidal limestones and ironstones). Perhaps, it would be better to support the isotope data from the section Varbanchovets with stable isotope analyses of bulk sediment: however, such analyses would provide their own questions of reliability (i.e., diagenesis) and interpretative capabilities.

Minor points: Point 1. P317, lines 13-15. The sentence now reads: “It is unknown if these events record further global palaeoenvironmental changes and faunal turnover after the T-OAE or they are discrete events consequence of the post-T-OAE stabilization (Gómez et al., 2008). Here, “and” was replaced with “or”. Point 2. P318, line 18: “Early Jurassic” replaced “early Jurassic”. Point 3. About the usage of capitalization and not italicization for ammonite zones and subzones and their meaning. In this paper we follow the Toarcian zonal/subzonal set proposed by Metodiev (2008) where the names, meaning and correlations of the Bulgarian ammonite zones are discussed is detail. Although incomplete (this is normal), it has been found that Bulgarian ammonite zones correspond to the NW European standard zones (e.g. Elmi et al., 1997). For the purposes of the present study, the zonal/subzonal subdivision has been constructed very carefully, and subzones were defined when possible. The range charts of the ammonite taxa drawn against the lithology and isotope trends clearly shows the meaning of zonal/subzonal units used and the degree of its certainty. So in our view there is no place for confusion. We believe that the empirical basement of the ammonite distribution certainly gives chronostratigraphic background for the isotope trends outlined. If we have to be completely strict, the names of the zones used have to always be written with both generic and species names of the index-species (as is recommended by the International Stratigraphic Code). Unfortunately, this practice is little used and the differences attributed to the names meaning one and the same thing such as “Tenuicostatum”, “tenuicostatum”, and the “tenuicostatum” is somewhat odd. We believe that all of these names mean the beds of full vertical (and lateral) distribution of the ammonites of the Dactylioceras (Orthodactylites), limited from below and from above by the first appearance and the disappearance of the subgenus, and nothing else. Point 4. P324, line 27: the text is now changed and so avoids the sequence stratigraphic determinations for the black shales. Point 5. P330, L10-13: “Unclear. Please rephrase”. We are not sure what is meant by “rephrase” here. The text in our view is clear and the English language used is OK. Point 6. P337: The deposits can hardly be described as “hemipelagic”. As it has been stated already, we believe that the sediments of sections Varbanchovets and Babintsi record hemipelagic environments although not really being “good examples”. Unfortunately, there is a lack of enough data from neighbouring localities and sedimentary sections to confirm what the depositional environment of our successions truly are: for example, were they associated with platform margin or a ramp. So, the word “good” was removed from Section 5.1.1., line 5, but we feel that the “hemipelagic“ should remain as it is. The sentence: “Although thin, the clayey-carbonate successions of sections Varbanchovets and Babintsi represent good examples of Toarcian hemipelagic deposits” (P324, lines 4-5) can be replaced with: “The clayey-carbonate successions of sections Varbanchovets and Babintsi represent examples of deeper parts of the inner shelf deposits.” Point 6/7. P337, Point 2 of Conclusions, line 17. We agree that “oxygen-depletion” is better than “anoxic deposition”, as not being actually able to give other supporting geochemical and petrographic evidence. Changed as requested. Point 8. P337 “warming” replaced “worming” ïĄŁ Point 9. Figures 3 and 5. We used “smoothed” interpolation lines between the isotopic data points because we believe that in this way it is much better to represent the trends that were obtained considering the thin strata and the scattered belemnite occurrence. Straight lines would be much more appropriate in successions with higher sedimentation rates and with higher density of studied samples which is not possible in our case. Point 9. Figures 8 and 9. In general we agree with this comment. The juxtaposition of bulk and fossil isotopic data on these figures was assembled in order to compare events. In our view, if these events reflect global environmental changes, they should be corresponding to each other. Point 10. “Figure 3: the scale of the carbon isotope values was inverted.”- We checked figure 3 and see no further issue with this figure. Point 11. Figure 7: stratigraphic “break” replaced with “brake” ïĄŁ Point 12. Figure 8 and 9. We agree that it would have been nice to add the belemnite data from Yorkshire on Figures 8 and 9. See comments on point 9. The technical corrections concerning referencing style/format have all been corrected exactly as suggested by reviewer 3.

Response to Reviewer 4 (S. Hesselbo):

We appreciate the comments and lack of anonymity by Stephen Hesselbo. First we would like to respond to the note of Hesselbo that "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". We want to state that no thick succession of the Lower Jurassic in Bulgaria, without gaps and condensations was available for this. Potentially, boreholes through the Moesian Platform (the foreland of the Balkan Orogen in Bulgaria) might contain more complete and more continuous sedimentary records, but these obviously would not provide the essential faunal assemblages and diversity that will allow us to conduct the study that we have reported. POINT 1: We agree with the comment about the lack of sufficient isotope data/resolution in the upper Falciferum Subzone. This is true for both of the studied sections. In the Metodiev and Rekalova-Koleva (2009) paper there are some δ13C data but again it is limited. So for clarity we have done the following: - On figures 8 and 9 we have erased the line connecting the data from the Upper Falciferum and the base of Bifrons Zones. -The conclusion section 3 on line 22 p. 337: now reads: "values in the early Bifrons ammonite zone" i.e. as suggested we have not included the upper Falciferum Subzone in the discussion. POINT 2: In general, we agree with this comment that it is possible that the recycling of belemnites from older horizons may cause the relatively sharp Sr isotope fluctuations (in the Bifrons and Thouarsense Zones, for example). However, if recycling were considered as a viable mechanism, then we would expect that recycling/reworking will cover not only the belemnites but also other fossil groups such as ammonites, bivalves, brachiopods, etc. Our detailed work on the division of ammonite zones and subzones (as well as co-occurring fossil groups) did not reveal any evidence for the disturbance of the ammonite Zonal assemblages for the intervals in question. Thus, we feel that although the isotope record, as constructed on belemnite rostra evidence, may be impacted by possible reworking, such process is not responsible for the sharp peaks in the isotope record of the two sections studied. POINT 3: Sharp Sr isotope spikes toward more radiogenic values are very well correlated with negative δ18O excursions, revealing that the isotope record

SED

4, C172–C190, 2012

Interactive Comment

for these intervals are largely controlled by fluctuations of riverine Sr inputs: warmer temperatures, increased hydrologic cycle and weathering. This pattern is evident for both of our sections and includes all samples identified with a star (*) in our data tables. The calculations of the duration of the ammonite zones have excluded these anomalous data points due to the fact that the sedimentation rates would have been different. POINT 4: We agree with this comment about the size of the figures. We think that the originally large sizes should be shrunk in size by the journal production/editorial staff. The minor issues and technical corrections concern referencing style/format and have been corrected exactly as suggested by the reviewer. Regarding Section 2.2. (P320, lines 15-21)- though short, we believe this section is at the correct place and it should remain separate section.

Response to Reviewer 5 (E. Mattioli):

We are grateful for the comments of Emanuela Mattioli on our manuscript and thanks for the lack of anonymity. Below we respond to specific recommendations as outlined: Point (1) Literature cited in the paper. In general, we agree with this comment, although we assure the reviewer that the missing papers were not intentionally ignored. As it has already been stated, the work of "Mattioli et al., 2009" was added on page 324 line 8, and in the reference list as: Mattioli, E., Pittet, B., Petitpierre, L., Mailliot, S.: Dramatic decrease of the pelagic carbonate production by nannoplankton across the Early Toarcian Anoxic Event (T-OAE), Global and Planetary Changes, 65, 134-145, 2009. Other papers devoted to the Early Toarcian biogeochemical disturbances that affected both the marine biota at the carbonate production that were added (as suggested) are: "Cecca and Macchioni (2004)" was added on page 317 line 5, and in the reference list as: Cecca, F., Macchioni, F.: The two Early Toarcian (Early Jurassic) extinction events in ammonoids, Lethaia 37, 1, 35-56, 2004. "Morten and Twitchett (2009)" was added on page 317 line 6, and in the reference list as: Morten, S. D., Twitchett, R. J.: Fluctuations in the body size of marine invertebrates through the Pliensbachian-Toarcian extinction event, Palaeogeogr., Palaeoclimatol., Palaeoecol., 284, 29-38, 2009. "Mail-

SED
4, C172–C190, 2012

Interactive Comment

liot et al. (2009)" was added on page 317 line 22, and in the reference list as: Mailliot S., Mattioli E., Bartolini A., Baudin F., Pittet B., Guex,J.: Late Pliensbachian–Early Toarcian (Early Jurassic) environmental changes in an epicontinental basin of NW Europe (Causses area, central France): A micropaleontological and geochemical approach, Palaeogeogr., Palaeoclimatol., Palaeoecol., 273, 346-364, 2009. Point (2) Ammonite and belemnite preservation & taphonomy. This is a valid comment that aims to bring further clarification on the impact of the reworking of the ammonites and belemnites on the biostratigraphic interpretations and geochemical analyses on the studied sections. Several points demand clarification:

We used the adjective "APPROXIMATE" in the legend of Figure 2, in order to draw attention that even being quite similar to those from the NW Europe, the ammonite assemblages of the Toarcian in Bulgaria display their own (local) characteristic features/peculiarities. Detailed discussion about the Bulgarian Toarcian ammonite successions can be found in Metodiev (2008). The correlations with the NW European zonal/subzonal standard have been made with extreme caution! We believe that this approximation does not depend on the reworking of the material used for the purposes of this study and does not disturb the biostratigraphic scheme proposed. No mixed horizons/levels of different ammonite zones have been found. Like the normal practise and traditions in NW Europe, the ammonite zones and subzones of the Toarcian in Bulgaria are indeed range zones. The studied sections recorded just parts of the fossil record possible in combination with those known from other localities in Bulgaria (see Metodiev, 2008). Because of that, the range charts seem to be somewhat incomplete. That is taken into account. Regarding Sr isotope data plotted in Figure 7: the samples are shown in respect to the absolute ages after a linear regression analysis of the Sr values in respect to the exact stratigraphic position of each sample, and grouping the data into several individual segments (p. 332 of MS). It is true that some of the results obtained displayed wider dispersion on the 87Sr/86Sr, but the majority appears to fall within the cloud derived from the data of McArthur et al. (2000), and there are also absolutely perfect matches of our data of the Sr temporal trend. We believe that our dataset should not be regarded as direct juxtaposition to this given by McArthur et al. (2000). The deviation could be due to local palaeoenvironmental influences such as weathering of different source materials. Our understanding for this new SE European setting (sites) is still in its infancy and more data from other localities will be needed in order to fully compare the two datasets. Perhaps the age-model used is problematic, since the composite Sr-isotope trend for the Lower Jurassic is dominantly constructed on the UK belemnites. It is not certain if the UK curve is representative on a global scale. The scatter between the Lower Jurassic Sr-isotope data may, in part, be controlled by varying fluxes and isotopic compositions of Sr in those regions (assuming that the Sr isotope residence time is shorter than 1Myr). Hence, future research on other Lower Jurassic sites should be investigated in order to test the true nature and variability of the seawater Sr-isotope curve as it is built strictly on UK sections. Point (3) Ages and durations of subzones (Section 5.2.5). We appreciate this comment. Although very difficult to reconstruct the durations of the ammonite biozones in highly condensed sections (Niento et al., 2008), we have attempted to show that at least in the Bulgarian sections, where the ammonite biozones are well defined (Metodiev, 2008 and references therein) and where the first isotopic and sedimentological data has been generated, interpretations regarding sedimentation rates and palaeoenvironmental disturbances can be reconstructed, with at least some level of confidence. We still believe that only after a much more detailed Sr isotope record is established from nearby adjacent Jurassic sections- we will be able to better correlate and discuss the the durations of the ammonite biozones in more detail. For now this is not a major goal of our manuscript and we prefer to wait for more dense Sr isotope data in order to produce better links with the UK and the European record, in general. We plan to collect more Sr isotope data from other outcrops with abundance of belemnites and with the same ages and surely should be able to see through the major issues and assumptions regarding the calculations and approach in general.

Point (4) Figures 8 and 9. This is an important comment and in general we concur with it. However, we believe that despite differences in absolute isotope values and different origins of the data, such comparison of trends can be quite useful. Based on the assumption that the major variations should be fairly similar, therefore they are indeed representing global events. Using the data that is known to us, the proposed figures sufficiently represent what we've done and what is required. The figures support our view in the discussion and concluding remarks. Point (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 what is the expression of this crisis in the Bulgarian sections and how it is similar or different with respect to other 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 required, and hence a separate manuscript in its own right. We note though that it is indeed an interesting, if somewhat enigmatic phenomenon. We have added some more text to discuss the possibilities but we cannot claim to have the full answer to this. The text is: "The failure of carbonate productivity to keep pace with base-level rise at this time could either reflect the rapidity of the rise and/or the occurrence of stressful conditions suppressing carbonate productivity. The stress could include oxygen-restriction although evidence for this condition does not appear until the Falciferum ammonite Zone (laminated shales in the Varbanchovets section)". Point (6) Minor points. Indeed the temperatures calculated from O isotopes are not displayed on Figures 3 and 5, but they are within Tables 1 and 2 (against each sample), where they are calibrated with the stratigraphic position and ammonite zonal/subzonal division. Concerning the figure format, we would like to point out, again, that they were shrunk by the journal staff and we had no control on their choice. However, we will make effort to request SE to publish the figures in more appropriate sizes, as suggested. We agree that the last two sentences of section 5.2.5 are obscure. The differences observed in the durations of the various ammonite zones pose a problem. Taking the principle that the Toarcian was a time of great climatic change and thus caused environmental stress on seawater biota, it might be expected that the relative duration of the ammonite zones is a func-

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

tion of the degree of the environmental pressure on the aquatic ecosystems. In other words, the ammonite zones with long duration will be a consequence of long periods of environmental stability with even slow rates of faunal turnover, whereas the reduction in the duration of the ammonite zones will be an effect of strengthened ecological strain upon the habitats and biota. However, the long duration of the Falciferum Zone argues against the widely adopted assumption that it was the peak-time of profound biogeochemical disturbances and climate perturbations during the Early Jurassic. Available data suggests that some significant effects on the nektonic faunas happened earlier (during the Tenuicostatum Biochron) and later (during the Bifrons Biochron, and especially the Variabilis Biochron), but a study of more Jurassic sections is required. Regarding the question of the palaeogeographic position of Bulgarian sections, we believe that the basin in which the studied sequences were deposited was more easterly located when compared to the Pindos Zone in Greece. See our idea of palaeogeographical position in respect to well known sites in the inset of figure 8 and 9 (upper left corner). The palaeogeographical positions of the High Atlas and the Asturias sections on Figures 8 and 9 have been triple checked and as a result we have moved the relative position of the Asturias section slightly northwards.

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

SED

4, C172–C190, 2012

Interactive Comment

Substage	Bulgaria (after Metodiev, 2008)		North-West Europe (after Elmi et al., 1997)		Substage
	Zone	Subzone	Subzone	Zone	
Upper Toarcian	Aalensis	Aalensis	Lugdunensis	Aalensis	Upper Toarcian
		Mactra	Mactra		
	Pseudoradiosa	Pseudoradiosa	Pseudoradiosa	Pseudoradiosa	
		Levesquei	Levesquei		
	Dispansum		Gruneri	Dispansum	
			Insigne		
	Fallaciosum		Fallaciosum	Thouarsense	
	Thouarsense	Fascigerum	Fascigerum		
		Thouarsense	Thouarsense		
		Bingmanni	Bingmanni		
	Variabilis	Denckmannia	Vitiosa	Variabilis	Middle Toarcian
			Illustris		
		Collina	Variabilis		
Lower Toarcian	Bifrons	Semipolitum	Bifrons	Bifrons	
		Bifrons			
		Lusitanicum	Sublevisoni		
	Falciferum	Falciferum	Falciferum	Serpentinum	Lower Toarcian
		Serpentinum	Elegantulum		
	Tenuicostatum	Semicelatum	Semicelatum	Tenuicostatum	
		Crosbeyi	Paltum		

Fig. 2.

Fig. 1. updated figure 2

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Interactive Discussion

Discussion Paper

SED
4, C172–C190, 2012

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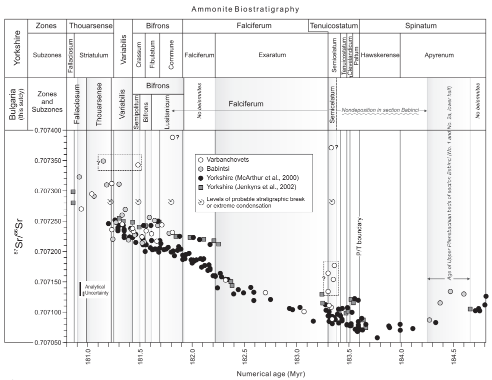

Fig. 7.

Fig. 2. updated figure 7

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4, C172–C190, 2012

Interactive Comment

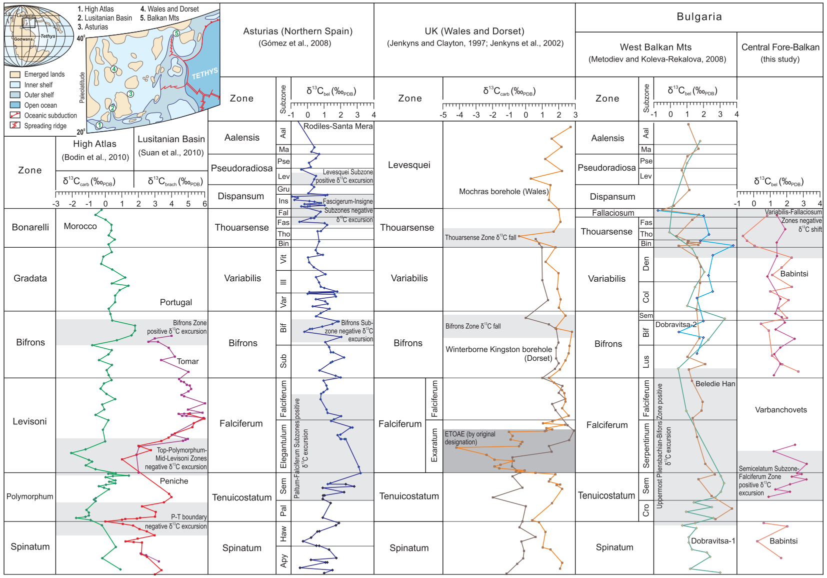

Fig. 8.

Fig. 3. updated figure 8

Interactive Comment

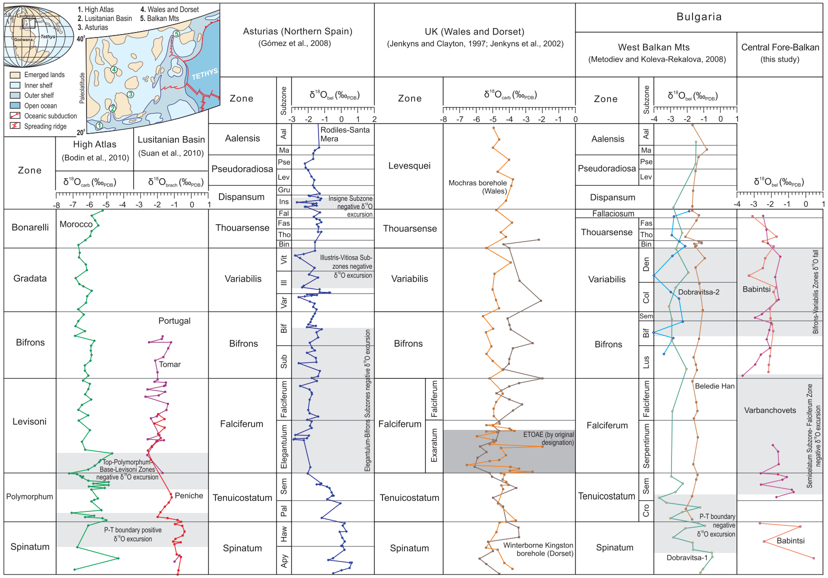

Fig. 9.

Fig. 4. updated figure 9

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