

Record of Early Toarcian carbon cycle perturbations in a nearshore environment: The Bascharage section (Easternmost Paris Basin)

[submitted to Solid Earth]

By

M. Hermoso

University of Oxford – Department of Earth Sciences

and co-authors

RESPONSES TO REFEREE #1

Referee's comments are in black

Our responses are in blue and preceded by the sign >

This manuscript by Hermoso et al. presents new sedimentological and geochemical data from Early Jurassic sediments deposited in a nearshore environment within the Paris Basin. The studied interval (late Pliensbachian to early Toarcian) spans an interval of major climate change known as the Toarcian Oceanic Anoxic Event (T-OAE), during which there is evidence for the widespread expansion of reducing marine conditions. The data presented in this manuscript comes from sediments exposed in Bascharage, Luxembourg, and reveals changes consistent with those observed in temporally equivalent deposits, confirming that these sediments faithfully record the global expression of the T-OAE. Additional features observed in the Bascharage succession (most notably a second carbon isotope excursion within the elegantulum sub-zone) are attributed to localised changes in basinal conditions. Overall this is a clear and well-constructed manuscript that improves our understanding of how an interval of major environmental change is recorded in different marine settings. The observation of anoxic sediments and characteristic isotope profiles from a nearshore marine setting demonstrates that the environmental changes observed during T-OAE were not restricted to deep marine basins, and opens the possibility of deriving equivalent records from other global sites. In my opinion these results are therefore suitable for publication in Solid Earth.

One of the key points made throughout the manuscript is the correlation between the geochemical records ($\delta^{13}\text{C}$ and $\delta^{18}\text{O}$) and the sedimentological changes that are indicative of marine anoxia (e.g. increase in TOC content and the occurrence of laminated black shales). The authors quite rightly point out that these changes are used to define the T-OAE in other sedimentary successions, and their agreement with the biostratigraphy lends credence to the fact that the sediments exposed in Bascharage do indeed record this interval. However, there are a few aspects of the new record that warrant further explanation/clarification:

1) What change in $\delta^{13}\text{C}$ at Bascharage is interpreted as the positive excursion that defines the T-OAE in these sediments? The two negative $\delta^{13}\text{C}$ excursions are clearly visible in Fig. 4, but I cannot identify the positive excursion that the authors refer to on p1074 (lines 109-111) and in Section 5.3. To me, the clear $\delta^{13}\text{C}$ increase within the elegantulum subzone reflects the recovery of seawater $\delta^{13}\text{C}$ following the 1st negative $\delta^{13}\text{C}$ excursion (CIE 1), thus isn't necessarily equivalent to the positive $\delta^{13}\text{C}$ excursion found in other Toarcian-aged sediments. It would be helpful if the authors were more specific about what they define as the positive $\delta^{13}\text{C}$ excursion at Bascharage, and how the magnitude and timing of this excursion compares to other Toarcian sections.

> Outside the two CIEs that we have defined, all portions of the $\delta^{13}\text{C}$ curve show a positive trend that we use to define the T-OAE (see our discussion in Hermoso et al., 2014; Clim. Past). As pointed out by the Reviewer, the increase in carbon isotopes is especially visible between the two CIEs, which may be fostered by the recovery of the first one. We have clarified this point in our revised manuscript. As stated in the original discussion paper, we are reluctant to give too much weight to data that have been produced from samples with $< 10\%$ CaCO_3 (open symbols on Fig. 4). This means that the carbon isotope positive trend across the Pliensbachian – Toarcian boundary, and hence inferred T-OAE at this level has to be regarded carefully.

The magnitude of the CIE 1 is compatible with other records from the Paris Basin, Germany or Yorkshire. This point has been added in our revision.

In the absence of cyclostratigraphic on the Bascharage section, we are unable to quantify the duration of the events. We hope that our study will stimulate such a work based on our data, or generation of organic carbon $\delta^{13}\text{C}$ and $\delta^{15}\text{N}$.

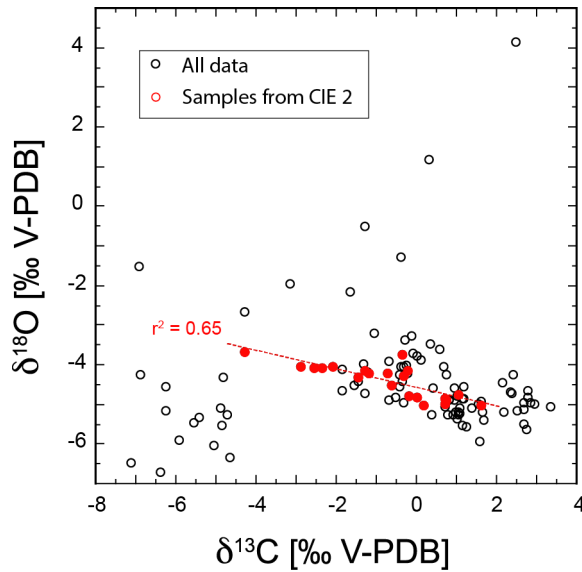
2) The authors state that the principal difference between CIE's 1 and 2 is the relationship between $\delta^{13}\text{C}$ and $\delta^{18}\text{O}$ (namely that $\delta^{18}\text{O}$ decreases during the negative $\delta^{13}\text{C}$ shift associated with CIE 1 but increases with each negative $\delta^{13}\text{C}$ step in CIE 2). However, Fig. 4 appears to indicate a notable ($>2\%$) increase in $\delta^{18}\text{O}$ at the very start of CIE 1 that is not mentioned by the authors. Why is this $\delta^{18}\text{O}$ increase different to those within CIE 2, and what are its palaeoceanographical implications?

> This is true. As stated above for $\delta^{13}\text{C}$ data, these points have very little carbonate. Hence, we refrained from over-interpreting this increase in the oxygen ratios. Nevertheless, such a feature (cooling?) has recently been reported in another section of the Paris Basin (Sancerre). We have added this potential basin-scale palaeoceanographic event in our revised draft.

This $\delta^{18}\text{O}$ increase occurs prior to CIE 1, whereas in CIE 2, increases are concomitant with each of the negative step of the carbon isotope excursion.

Furthermore, how reliable are the variations in $\delta^{18}\text{O}$ observed during CIE 2 (they don't appear to be that large relative to some of the other changes presented in Fig. 4), and is the different relationship between $\delta^{13}\text{C}$ and $\delta^{18}\text{O}$ in CIE's 1 and 2 supported by a change in their relationship when plotted against each other?

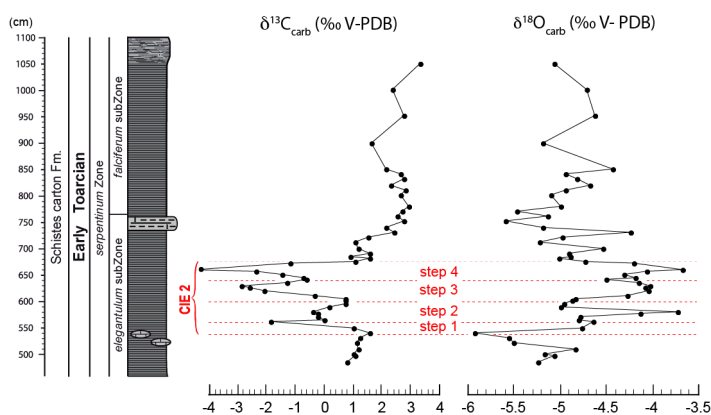
> A supplementary figure showing a scatter plot of $\delta^{13}\text{C}/\delta^{18}\text{O}$ will be incorporated into the new manuscript. In this figure co-variation is more apparent than on the separated curves. This correlation is significant ($r^2 = 0.65$).



[Additional Figure to be incorporated into the revised manuscript]

3) I am confused by the $\delta^{18}\text{O}$ records shown in Figs. 4 and 6. They should theoretically be exactly the same, however in Fig. 6 the $\delta^{18}\text{O}$ values at 500 cm are ~ -3.5 ‰ (one of the highest values observed between 500 cm and 1100 cm), whereas in Fig. 4 the $\delta^{18}\text{O}$ values at 500 cm appear to be ~ -5 ‰ and subsequently increase to values of ~ -3.5 ‰ during CIE 2. Why is there this apparent discrepancy between the two figures?

> The reviewer has spotted a mistake. The data presented in the paper comes from several analytical campaigns (including sampling and different mass specs used), hence the mismatch. We apologise for the confusion. Data have now been carefully checked and the graphs have been amended accordingly. There is no change in the interpretation in the $\delta^{18}\text{O}$ signal with this correction restricted within a 30-cm-interval. The raw dataset will be provided as Supplementary Materials among with the revised manuscript (as we intended to do, and requested by the other Reviewer).



[Revised Fig. 6]

4) Finally, although the authors provide a sufficient amount of information to support their interpretation of the $\delta^{13}\text{C}$ and $\delta^{18}\text{O}$ records (and other sedimentological changes observed at Bascharage), I feel that they could possibly expand their discussion in Sections 5.2/5.3 to provide some consideration of whether the studied succession simply represents an expanded interval over the four $\delta^{13}\text{C}$ steps observed in other locations (e.g. in Yorkshire; Kemp et al., 2005). Given the

inferred high detrital sedimentation rates at Bascharage (p1074, line 17) is it possible that the multiple negative shifts in $\delta^{13}\text{C}$ recorded in this study are in fact the same as those observed elsewhere, albeit with an expanded sedimentary interval between the 1st and 2nd $\delta^{13}\text{C}$ shifts? Related to this, how robust is the biostratigraphic framework for this succession? Although briefly dealt with in section 2.2, it is not clear whether the subzone boundaries used in Figs. 2, 4,5 and 6 are those defined in this study or by previous workers, and what impact any uncertainty in the age of the sediments may have on the interpretation of the new geochemical records. Whilst I don't expect these considerations to change the interpretation of the new data, I think that the manuscript would benefit from the clarification and/or further consideration of these issues.

> This is indeed a good point. However, careful examination of the biostratigraphic framework (which is reliable, as generated at the subspecies level) rules out this hypothesis. Such a record of the subsequent steps would require an extraordinary sedimentation rate at Bascharage. Even in a proximal environment such as Bascharage, such a high siliciclastic supply is unrealistic. The *bifrons Zone*, not seen at Bascharage but in adjacent sections in SW Luxembourg, starts only a few metres above to top of the Bommelscheuer outcrop. In other sections with high sedimentation rate, the CIE is always comprised within a restricted sedimentary interval. Nevertheless, this point is worth discussing, and we have added a paragraph including the above-presented arguments.

Other specific comments:

p1075, Line 11: . . .some intervals corresponded. . .

> Done.

p1080, Line 26: No carbonate was detected in the black shales between 2.6. . .

> Done.

p1080, Line 26: This absence of carbonate is explained by high detrital input (p1084, lines 26-27). However presumably the fact that it corresponds with the most intense period of marine anoxia (i.e. highest TOC contents) is also significant and worth discussing?

> Yes, the reviewer is absolutely right in suggesting that this decrease in CaCO_3 could result from a dilution by the organic material. We will mention and discuss this possibility in our manuscript.

p1082, Line 20: In detail, although. . .

> Corrected.

p1083, Line 18: ratios of 0 ‰ (carbon). . .

> “of” has been added

p1084, Line 19: According to Fig. 4, it is the sediments within the Marnes d’Ottemt Formation that have $\delta^{13}\text{C}$ values of ~ 0 ‰ not the Marnes à Semicelatium Formation as stated.

> This has been corrected.

p1088, Lines 12-13: I would rephrase this sentence as it could be applied to all of the sediments investigated in this study (i.e. implying that none of the $\delta^{13}\text{C}$ and $\delta^{18}\text{O}$ data generated is meaningful). I do not believe that this is the case – only the very lowest exposed sediments appeared to show any variation that could potentially be disregarded as non-representative of marine conditions.

> We have rephrased the sentence for: “Sediments exposed at the base of the studied interval largely revealed to be unsuitable to apply carbonate-based isotopic proxies ».

Fig. 1: Locality 4 (Yorkshire) has not been identified.

> This has been added in the caption.

Figs. 4 and 6: See previous comment regarding the apparent miss-match between the $\delta^{18}\text{O}$ records in these figures.

> This has been sorted.