

Interactive comment on “Central Tethyan platform-top hypoxia during Oceanic Anoxic Event 1a” by Alexander Hueter et al.

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Received and published: 6 February 2019

Dear colleagues,

Thank you very much for your detailed and insightful comment. We believe, this is really the essence of a discussion version of a paper and we welcome your comments. Please find our reply below:

Comparing the $\delta^{13}\text{C}$ curve of Huck et al. (2010) and the one of Husinec et al. (2012; 2018), and taking the $\delta^{13}\text{C}$ of rudists from Huck et al. (2010) into account suggests many similarities (which is good). Due to the geographical distance between the Kanfanar and Mljet sections, there might be potentially different burial and generally, diagenetic, histories that must be taken into consideration. In our view, the bulk $\delta^{13}\text{C}$

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signatures in the Mijet section have seen less diagenetic alteration. In your comment, you refer to the fact that we have shifted the C3 and C4 segments relative to the Huck et al. (2010) paper. Please allow us to explain why we think that this is justified:

In our view, the first occurrence of microencrusting organisms in the platform domain is coeval with the onset of hypoxic conditions in basinal settings expressed as organic-rich deposits comparable to black shales. The C3 segment is characterized by a pronounced negative $\delta^{13}\text{C}$ excursion and marks the onset of OAE 1a equivalent in Kanfanar. When comparing this pattern with the Cismon section (Menegatti et al., 1998), there defined by the first occurrence of black shales, evidence for the assignment of the C3 segment in Kanfanar to the base of the Kanfanar Unit is found. The top of C3 as well as the base of C4 in Kanfanar are picked on top of the last microencruster occurrence based on the lowest seawater oxygen content, as suggested in Ce/Ce^* and by means of redox sensitive trace element concentration, as well as the $\delta^{13}\text{C}$ signatures of rudist low-Mg calcite as shown in Huck et al. (2010).

Are these statements in disagreement with the comments made by Husinec and Read? We believe not! We feel that assuming a high degree of temporal correlation between two sections situated perhaps 1000 km apart, is not likely. Actually, our view of a “global” event is that of a global clustering of regional (and even local) events that are all more or less “coeval”.

Finally, let us comment on the issue of the C4-C7 segments in figure 8. We fully agree that the assignment of the C4-C6 intervals (for example in Fig. 4) are less well constrained. Hence, no disagreement here. Please note the question mark (for example figure 4 etc.). You suggest that you record ongoing hypoxia into segment C7. We are happy with this and again refer to the fact that we should not assume a fully coeval expression of seawater oxygen level across a large, topographically complex platform. Please also note that we documented significant regional differences in the timing of the first and last occurrence of microencrusters providing solid evidence of the complexity of a system that sees a combination of global, provincial and regional

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environmental patterns (perhaps similar to sea-level reconstructions).

What have we learnt from this discussion and how do we respond to this criticism? (i) Error bars for $^{87}\text{Sr}/^{86}\text{Sr}$ data will be added to figures 3, 4, 5. (ii) The reference to the C7 segment will be removed from figure 8D, given that it is not discussed in the text. (iii) The paper certainly has benefitted from this discussion and we will add some text to the discussion chapter that deals with these complex issues.

However, our data point to an increase in seawater oxygenation at the end of C3 or at the base of C4. We agree that the upper part of the Kanfanar section is difficult to interpret due to a lack of automicrite that provides a reliable archive for the proxies applied here. Nevertheless, U-isotope ratios and Ce-anomalies indicate more oxygenated conditions of seawater upsection of the C3 segment. We feel that the U isotope data are the most reliable ones and given the long residence time of U in the global seawater, we would rather expect a global and not a regional signature. Moreover, this interpretation is supported by the $\delta^{13}\text{C}$ signatures of well-preserved rudist calcite (Huck et al., 2010) recording values of 3-4 ‰.

Concluding: In this study, we do not limit platform-top hypoxic events to the C3 segment and we will make this very clear (in case it was not clear in the previous version) in the revised version. Assuming the most severe seawater oxygen depletion at the end of C3, the platform-top is possibly also hypoxic in the C4 segment, but then shows slow recovery to more oxygenated conditions in the Kanfanar section. We would not assume that this pattern must be directly translated to that found in the Mljet section and the Husinec and Read data. These are “simply” very complicated patterns and our means to date and correlate are less than ideal.

Thank you for your very constructive comments,

Sincerely yours,

A. Hueter on behalf of the authors.

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