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Discussions

Interactive comment on "An open marine record of the Toarcian oceanic anoxic event" *by* D. R. Gröcke et al.

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The Toarcian Oceanic Anoxic Event (T-OAE) has been remarkably popular among Mesozoic stratigraphers, paleontologists, isotope geochemists, and paleoceanographers in the past 25 years. Recently, interest in this event is only picking up rather than abating. A possible explanation is that the T-OAE shares many similarities with the Paleocene-Eocene Thermal Maximum, which in turn is seen by many as the best deep-time analogue of where we are heading to with the modern global change. Look at it from the other perspective and the T-OAE may provide insights into the working of Mesozoic Earth system. Perhaps surprisingly, all the research effort devoted to the T-OAE still left a few questions unanswered and a few issues still vigorously debated.

One basic aim of the contribution by Gröcke et al. is to demonstrate the truly global

extent of the T-OAE. Initially documented from the epicontinental basins of NW Europe, then from the western Tethys, sites ranging from North and South America to Tibet already pointed to a global signal. Yet the chert record studied by these authors from the Katsuyama section in Japan is indeed the first credible evidence from deep sea facies deposited far offshore in the vast Panthalassa ocean. This make it a significant and timely piece of research.

The expected signature what an OAE would leave in the sedimentary and fossil record include organic-rich strata, carbon isotope excursions (CIE), and biotic turnover or even mass extinction. Organic content is commonly controlled by local factors of surface water productivity and the degree of oxygen depletion in bottom waters. Fossil preservation needs suitable facies and is subject to the vagaries of fossil record. Thus the authors emphasize the third aspect, the presence of CIEs, of which the T-OAE is characterized by a large scale negative excursion immediately followed by a positive shift in the early Toarcian. More recently recognized is a preceding smaller negative peak at the Pliensbachian-Toarcian boundary. The authors claim to find these CIEs in their bulk organic d13C curve, which is constrained by radiolarian biostratigraphy. Morever, the presence of dark chert facies and increases in total organic content (TOC) are also documented. Putting all these together, they offer a correlation to well-established sections in Europe and combine the new data with previously available information to refine the mechanistic model for the T-OAE.

Without questioning the overall value of the new data and the inferences made by the authors, I use this opportunity to point out a few inaccuracies of the paper, to raise a few points that may require clarification, and to take issue with some of the claims they make, in order to help the authors produce an improved version.

1) Geological setting and stratigraphy: The studied strata are said to have deposited in the open ocean, on "a volcanic seamount at mid-water depths" (Page 393, Line 9). A seamount top may not be a good site for continuous sedimentation as radiolarian ooze may be swept away by currents. Mid-water depth is rather vague. For the presence of

volcanics in the basement, please cite evidence or provide a reference.

The 60 cm thick white chert in the section (Fig. 4) is puzzling. I'm perplexed by your interpretation that it is likely a low-grade metamorphic alteration product of volcaniclastics (Page 391, Line 1-4). Such a thick air-fall ash deposit in the middle of Panthalassa would require unrealistically enormous eruptions somewhere, presumably at the convergent margins.

The low-grade metamorphism implied here is in contrast to what is said earlier of extremely low grade metamorphism or chert diagenesis only (Page 389, Line 23). Precluding a metamorphic overprint is important in the context of interpreting the d13C signal as primary.

It is disheartening that a recently published study which used the same section (Wignall et al. 2010) cannot be directly compared with the sampling and data reported here (Page 391, Line 7).

2) Biostratigraphic framework: The radiolarian biostratigraphy for the Toarcian is unfortunately of fairly low resolution. It is not clear if Fig. 2 shows the ranges of all taxa or just a selected few species. The key radiolarian biostratigraphic paper by Carter et al. (2010, Palaeo3) is not listed in the References. The Napora relica – Eucyrtidiellum disparile Zone spans the entire Early Toarcian, thus it does not allow finer correlation of the isotope excursions. The top of the zone is correlated with the "Toarcian radiolarian event (TRE)" here, whereas in the original Hori (1997) paper it was said to occur near the Pliensbachian-Toarcian boundary. Judging from Figures 2 and 3, TRE is an origination event during the rebound from the large negative CIE but before the positive shift. The origination would imply a post-extinction recovery phase, yet that would not occur before the positive CIE elsewhere, especially in Europe. Minor remarks: the radiolarian faunal composition panel in Fig. 2 is left totally unexplained. The genus Parahsuum is misspelled in the text several times (e.g. Page 390, Line 390)

Correlation made to the ammonite zonation is confusing as the standard zonations

and regional zonations are not used consistently. Converting all ammonite data to the NW European standard zonation (as in Page 392, Line 4) would give a false sense of accuracy. Alternatively, use of regional zonations (e.g. Tethyan, see Page 398, Line 24, and North American) may be difficult to follow for readers not familiar with the details.

3) The carbon isotope record: The density of sampling leaves something to be desired. The coarse resolution would certainly preclude detection of a stepped decrease during the negative CIE as documented from Yorkshire. The very negative (-57‰ value measured in a single sample is suspect. I would tone down its interpretation.

4) Correlation and calibration: The time calibration of the studied section, on the basis of a long-term average sedimentation rate determined by Hori et al. (1993), may be overly simplistic. It overlooks possibly significant changes, e.g. the one that may have been caused by the alleged volcaniclastic input into the marine basin (but see above).

The two negative CIE is said to broadly coincide with lithological change to darker cherts and TOC maxima (Page 394, Line 6). I note that looking at it in detail, the smaller and older CIE (thought to correlate with the Pliensbachian-Toarcian boundary) postdates the TOC peak. For the bigger event (thought to correlate with the T-OAE), the TOC peak is coeval with the negative isotope shift, whereas accepted models suggest that increased burial of organic matter would lead to the positive shift.

Fig. 4 suggests that the lower, smaller negative CIE near the +80 cm level marks the Pliensbachian-Toarcian boundary in the Katsuyama section. Radiolarian biostratigraphy presented in Fig. 2, however, indicates that boundary at a significantly lower position, at -30 cm.

Overall, it appears that although broad patterns of the d13C curve, TOC maxima, and radiolarian zonation indeed allow global correlations, the fine details of this correlation web would not stand scrutiny.

5) Genetic model for organic-rich chert sedimentation: Clearly, widespread organic-

rich sediments are one of the hallmarks of OAEs. Thus it is only natural that the occurrence of dark chert layers in the studied section are listed as evidence for the global nature of T-OAE reaching Panthalassa (Page 396, Line 8). Yet the authors also entertain an alternative model, which albeit interesting, actually weakens their main thesis. It is suggested that as the oceanic crustal block travels though the equatorial region by plate motions, high surface water productivity would favour conditions of organic-rich sedimentation within a certain latitudinal window. I cannot comment on the paleoceanographic validity of this model, but I suspect that plate velocities and the width of the equatorial divergence zone would translate into longer duration of high TOC than actually observed, thus discrediting this model.

In summary, this contribution is important for a better understanding of the T-OAE. If some of my criticism is deemed justified, then hopefully an improved final version will be published.

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