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Solid Earth
Discussions

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

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We wish to thank Gerald (Jerry) Dickins for providing comments on our manuscript on a Toarcian record from the Pacific open ocean. They are greatly appreciated and we address his main comments below using his number scheme:

(1) The similarity between carbon isotope records of the T-OAE and the PETM is indeed striking and behoves one to compare them and establish what these events share and what separates them. While we think such a contribution is necessary, in fact this topic has been previously explored by Cohen *et al*. (2004), it is beyond the scope of the present manuscript. For now it appears premature to dwell on this, as we first need to improve our understanding of the Japanese section and other sections containing a record of the Toarcian event before interpreting and comparing global carbon per-

turbations which are over 130 Myrs apart. Nonetheless, we agree that reference to this similarity may be brought forward earlier in the present manuscript. The similarity in the discussions, as you mention, are perhaps a reflection of the influence that the PETM has had on the discussions concerning the T-OAE. It is therefore not surprising that many of the same arguments are used to explain both events.

(2) We agree that the discussion on oxygen deficiency is somewhat confusing and needs rewriting. In particular, the importance of upwelling needs to be emphasised as a driver of organic-rich sedimentation in open ocean settings. As is, this part of the discussion is too focused on seawater oxygen. In the present-day Pacific, oxygen concentrations continue to drop towards the northeast Pacific and a similar scenario may have been true for the Jurassic Panthalassa. However, if a seamount were located within a water mass containing very low oxygen, but under low productivity surface waters (such as is the case under a gyre), no black shales (cherts or otherwise) would be deposited. This can be illustrated using the example of the northeast Pacific again. At present, in this part of the ocean, oxygen concentrations immediately above the sediment/water interface are about half (3.2 ml l^{-1}) of those in the Norwegian Sea (6 ml l^{-1}) and yet in the former red clays are being deposited, whilst in the latter the sediments have dark grey colours. Many other variables, such as water depth, terrigenous input, organic matter type, etc. are important in determining whether a black shale is deposited or not. Thus, indeed, with a similar distribution of water chemistry, a site at intermediate water depths in the central Pacific would not traverse through higher dissolved oxygen conditions as it moved north from the Equator. However, the importance of a seamount passage under the equatorial divergence zone due to plate tectonics is that it places a mid-water depositional surface within a low oxygen environment under a region of high productivity. The export paths for organic matter would then be greatly reduced, destruction of organic matter in the water column minimised and more reactive organic matter reaching the seafloor on a pelagic setting implies that preservation is maximised for that setting. We further mention (line 23 onwards) that the presence of such 'anoxic' sediments does not necessarily signify that anoxia was widespread

during this interval. Localities with known black shale deposits are commonly plotted on a palaeogeographical map as evidence for widespread anoxia. Their widespread geographical distribution is commonly invoked as evidence for expanded global oxygen minimum zones due to a common cause. We caution against that generalisation here because the processes that we interpret to have led to the deposition of these shales are different from those acting on the contemporaneous European epicontinental shelf. Therefore, although sites may be correlated using chemostratigraphy — the carbon isotope excursion is a global signal — they cannot be correlated in terms of processes and should not be used as evidence for widespread anoxia in the ocean due to a common cause.

(3) It is undesirable to establish a direct link between carbon isotopes and seawater oxygen content variations. Whilst storage of marine organic matter in sediments will drive perturbations in the global carbon cycle (recorded as excursions in the carbon isotope record), low seawater oxygen is not required in all depositional environments. Furthermore, other processes drive carbon isotope excursions. Thank you for the reference to the Nicolo *et al*. (2010) paper as it is very interesting.

(4) A compacted sedimentation rate of 1 m Myr^{-1} for a distal, pelagic setting of Early Jurassic age is not low. Comparison of Plio/Pleistocene sedimentation rates with Jurassic is likely not to be meaningful. Moreover, siliceous ooze is largely dissolved in the water column and in the sediment yielding lower sedimentation rates than expected for a high productivity setting.

(5) Using carbon-isotope stratigraphy as a tool for correlation is well rehearsed for the Mesozoic. Although in Figure 4 we have drawn dashed lines suggesting possible correlations using the carbon-isotope curve, which seem to focus on single points, they are in fact indicative of trends. It must be made aware that the biostratigraphy of the Mochras Farm Borehole is not precise and has errors associated with it due to a lack of fossil ammonite abundance through the core. Thus again, these boundaries are marked by dashed lines indicating our acknowledgement that they are not fixed time

lines but could move with more data and information. Dating radiolarian biostratigraphy with ammonites is also poorly understood for the Jurassic. For example there are few radiolarians found from the critical T-OAE interval around the globe. In fact the reference by Carter *et al*. (2010) on the Oregon section has radiolarians but is very poorly dated using ammonite biostratigraphy. Further work on radiolarian sequences for the Lower Jurassic using a combination of biostratigraphy and carbon-isotope stratigraphy is required to address potential issues regarding radiolarian and ammonite biostratigraphic frameworks.

(6) Whilst intriguing and potentially very important, the significance of a single sample with an anomalous value (which we believe it is) to the main message of the article is not the focus of this dataset. Below the CIE (ranging from 180 to 240 cm depth in the Katsuyama section) the δ^{13}C values average –29 ‰, and above –26 ‰, whereas the average of the four values (excluding the anomalous samples) determined for the CIE is –32 ‰. The isotopic shift in the Katsuyama section is thus comparable to those found in European sections and clearly demonstrates the presence of a major perturbation that does not depend on the anomalously low single δ^{13}C value of –54.7 ‰. The low carbon-isotope value of this sample has been reproduced and at present is still an enigma. We shall refrain from discussing this single value in detail, but we thought it worthy to include the data point rather than dismissing it due to a lack of explanation and understanding.

Interactive comment on Solid Earth Discuss., 3, 385, 2011.