

Interactive comment on “Black shale deposition during Toarcian super-greenhouse driven by sea level” by M. Hermoso et al.

Anonymous Referee #1

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The manuscript argues for a changing sea level as a primary driver for the local redox conditions using geochemical proxies in concert with sequence stratigraphy during the T-OAE. Constraining sea-level coupled to local redox shifts during major carbon burial events is important for understanding the links and mechanism(s) for initiation, duration and termination. The current state of the manuscript sufficiently explains the proxies and their importance. However, I believe there is portions/section that could be clarified and/or streamlined to make the manuscript flow more smoothly. Such as, re-arranging the discussion-results section with all of the geochemical data and discussion together and the sequence stratigraphy lumped together will be more logical.

Lastly, I would like to see the redox geochemical story tied together a bit better. Where V is high but Mo is low what does this mean? What are the redox sequences, why

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are they not observed in Mo? The figure uses “widespread anoxia” but I think euxinia should be used as these are different depositional settings.

Page 4366 Line 1: Oceanic Anoxic Event does not need to be capitalized and the same for subsequent acronyms (TOC and CIE)

Line 8: Replace studied to study

Line 8: Replace stratigraphy to stratigraphic

Line 20: Atmosphere-Ocean does not need to be capitalized

Page 4367 Line 14: In principle, I agree with the author regarding the Neuendorf et al., black shale definition; however, inherently there are flaws with using lamination alone to define black shales. The word black shale refers to an organic-rich fine grained sediment while laminations imply something about redox conditions and the ecology. Therefore, the TOC content is important for defining these samples are relatively rare in the geologic record. For example, if there was low oxygen bottom waters in the middle of the low productive Pacific gyres it could be imagined these settings would be laminated but due to low TOC they would not be black. Is it possible to combine laminated and a TOC value (i.e. 1%) for the definition.

Page 4368 Line 3: A companion? It is more of a precursor to the OAE.

Line 5: ‘probably reflecting injection of isotopically-light carbon’ – due to the negative shift there has to be an injection of light carbon. Unless I am missing something in this sentence ‘probably’ should be removed.

Line 15: ‘decisive arguments’ I would prefer provided evidence to support or something similar.

Page 4369 Methods – Were there any international standards run? What are the reproducibility, detection limit and error of each metal using the XRF.

How were pyrite concentrations quantified? What is the accuracy?

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If there is new data in this manuscript there needs to be a brief description for TOC, TIC and $\delta^{13}\text{C}$ but if this is all from Hermoso et al. (2012) then this portion of the methods should be cut.

Page 4370 Just because Sc1 is not laminated does not mean it was not originally deposited under reducing conditions then slightly oxygenated to allow for some bioturbations (Boyer et al., 2011).

I am following your argument of oxygenation at the termination of Sc1 due to bioturbation, low pyrite, low TOC. I don't follow the carbonate argument, please explain. How is a change in sedimentation and basinal restriction not considered? I do not believe these changes are completely dictated by sedimentation rates (slower – allowing for deeper oxygen penetration depths – affecting all of these proxies) but this should be addressed. Basin restriction could affect the amount of reactive Fe therefore controlling the pyrite concentrations.

Line 18: in average should be replace to on average

Page 4373 Line 17: “A sharp subsequent decrease in V/Al subsequently although pCO_2 remained continuously high (McElwain et al., 2005; Hermoso et al., 2012)” I am not following this sentence/argument.

Page 4375 Line 10: Be explicit when making this statement as I believe you are stating there is no shift in the redox deposition across Sc1 and Sc2 but you have already stated there was a brief oxygenation between the events.

Generally comments I would argue the paper is in need of a paragraph in the intro discussing the timing and event of this OAE for these ‘steps’ during the OAE.

Generally, the higher Fe/Al during Sc2 and Sc3 values are driven by lower Al concentration rather than an increase in Fe contents which also affect the magnitude of the V ratio but controlling it. This seems rather important and is this due to changing sources of the sediments?

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Caution must be used when using Mo/TOC ratio alone to interpret the global nature of an event as the modern Black Sea shows low Mo/TOC values while the surface water is similar to the open ocean which is partially driven by its restricted nature. Similarly, this could be argued for any given section but there are multiple sites showing similar Mo/TOC values (this need to be very clear).

Figures The symbol for burrows is very difficult to see.

The labels for the OAE and intervals (Sc and Mb) are very difficult to read.

$\delta^{18}\text{O}$ was not discussed until the 2nd to last paragraph and was previously published. I do not see a reason to include this data if there is no discussion prior to the final few sentences.

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9, C2127–C2130, 2013

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