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Climate of the Past Discussions

Interactive comment on "Dynamic climate-driven controls on the deposition of the Kimmeridge Clay Formation in the Cleveland Basin, Yorkshire, UK" *by* Elizabeth Atar et al.

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This manuscript reports on a study of organic carbon (TOC), total sulfur (TS), carbonate, major and trace element contents, and carbon isotopes, together with petrographic observations, in order to investigate the primary controls on Late Jurassic sedimentation and TOC enrichment in the Cleveland Basin, Yorkshire, UK, with a view toward evaluating climatic controls on intervals of inferred high and low primary productivity.

Overall, this study is very well-executed, and the interpretations and conclusions are quite reasonable. It should become acceptable for publication following minor revision. I offer some insights on a few important issues as well as some minor comments that

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Discussion paper

the authors should consider during the revision stage.

The approach adopted in this study is to be commended in one respect in particular: It is useful to identify the main components of a sample set, and to link the geochemistry of the bulk samples to these components. This should be self-evident, but too frequently geochemical proxies are interpreted in chemostratigraphic studies with little consideration given to the underlying host sediment fractions.

Major issues:

1) Flux calculations should be presented for arguments that invoke sediment fluxes. For example: Page 8, line 31: "The fine-grained nature of the sedimentary rock may indicate a siliciclastic starvation process." When making claims regarding high or low siliciclastic or organic fluxes, it is generally a good idea to support them with some actual flux calculations. Making reliable flux calculations may be difficult for poorly dated formations, but the present study units are exceedingly well-dated, spanning the Pectinatites wheatleyensis to Pectinatites pectinatus ammonite biozones. This interval corresponds to ∼1.5 Myr (ca. 151.2-149.7 Ma) per the 2012 Geologic Time Scale (Gradstein et al., 2012, chapter 26). The study interval comprises 45 m, so its average sedimentation rate is ∼30 m/Myr–in other words, a pretty average cratonic rate. There might be condensed intervals within this succession, but it is not sediment-starved as a whole, as implied by the statement above.

Page 9, line 8: "the occurrence of normally graded beds with erosional bases in TOC-rich sections of the HVMIs indicates an energetically dynamic setting." A combination of high energy levels and sediment starvation would produce a lag deposit, i.e., concentrated high-density and/or resistant clasts such as fossil, pyrite, and/or phosphate grains. Are there any features of this type in the study succession?

Page 9, line 31: "Based on the chronostratigraphic time frame for the Yorkshire and Dorset sections (Armstrong et al., 2016; Huang et al., 2010) . . . " If there is a published astrochronology for the study formations (Huang et al., 2010), why not integrate it into the present study and discuss its implications for the duration and accumulation rates of these formations?

2) Interpretation of controls on high TOC or high CaCO3 intervals:

Page 10, line 24: "We therefore propose that nutrient availability was the likely driver of changes in productivity. ... The wet-dry cycles proposed by recent climate modelling (Armstrong et al., 2016) may therefore be the key driver behind oscillations in the production and preservation of TOC, i.e. the switching between the LVMIs and HVMIs." This is certainly possible but might be difficult to prove. An alternative hypothesis is that organic productivity and sinking fluxes were held more-or-less constant, and the large variations in TOC content were driven by variable influx of siliciclastics, leading to variable dilution of the organic carbon flux. Perhaps the authors could provide arguments countering this alternative hypothesis? Of course, it is also possible that both nutrient and siliciclastic fluxes were covarying in tandem.

Here is where the astrochronology of the study formations might help–if a characteristic periodic signal (e.g., 100-kyr eccentricity cycles) is present, then it is potentially possible to calculate short-term variations in sedimentation rates in a study section, rather than being limited to an average sedimentation rate for the entire section (as calculated above). An example of application of a floating time scale to analysis of short-term sedimentation rate variation is given in Algeo et al. 2011 (Algeo, T.J., Kuwahara, K., Sano, H., Bates, S., Lyons, T., Elswick, E., Hinnov, L., Ellwood, B., Moser, J. and Maynard, J.B., 2011. Spatial variation in sediment fluxes, redox conditions, and productivity in the Permian–Triassic Panthalassic Ocean. Palaeogeography, Palaeoclimatology, Palaeoecology, 308(1-2), pp. 65-83.).

Page 10, line 30: "Carbonate productivity, mainly in the form of coccoliths, varies throughout the studied KCF section and is at its maximum within the carbonate-rich sections of the HVMIs." Probably correct, but again this represents an assumption, not a proven fact, and one could argue (as for TOC variations; see above) that variable siliciclastic influence controlled variations of carbonate content in the study section.

Page 11, line 13: “However, enrichment factors of redox sensitive trace elements (Mo and U; Fig. 8) . . . indicate that during the deposition of the LVMIs, the sediment pore water was suboxic to anoxic.” This is open to interpretation, but my opinion is that the modest Mo-EFs (mean ∼4, max ∼20) and U-EFs (mean ∼2, max ∼4) of the LVMIs indicate overwhelmingly suboxic conditions. These values are not strongly supportive of anoxic conditions.

Section 5.3.4: One point about particulate shuttles that is not made clearly here is that they seem to be most effective at authigenic trace metal enrichment when redox conditions fluctuate strongly, as opposed to stably euxinic conditions (see Algeo and Tribovillard, 2009).

3) Minor issues:

Page 9, line 14: “Owing to the shallow gradients and vast extent of epicontinental seaways, sediment dispersal in the LVMIs, which are dominated by terrigenous mud, is likely to have been controlled by wind- and tide-induced bottom currents (Schieber, 2016).” Whether winds and tides could induce significant bottom currents would depend on water depths–at tens of meters, they would be important but at hundreds of meters much less so. The geologic background section (page 3, line 30) indicates considerable uncertainty regarding water depths in the NW European Sea, so the potential influence of bottom currents is uncertain.

Page 10, line 5: “Biological components (coccolithophores, foraminiferans, and organic carbon) occur in differing proportions throughout the section (Figs. 2 and 3). Our petrographic observations (Fig. 2) . . . ” The presented petrographic data appear to be entirely visual/descriptive. Why not undertake point counts of organic maceral types? This would be provide more quantitative information about the nature of the organic fraction that could be compared with other data (e.g., d13C-org).

Page 10, line 22: "Water depth is not likely to have exceeded a few hundreds of meters in the distal Cleveland Basin (Bradshaw et al., 1992), suggesting that the euphotic zone could have reached the seafloor and light did not limit primary productivity." This statement betrays an incomplete understanding of the photic zone. Light intensity is attenuated quickly and drops to ~30% of surface levels by 10 m and to a few percent by 100 m water depth in clear water; in turbid water, the rate of attenuation can be much faster with depth. Most primary productivity is typically in the upper 10 m of the water column, and there will be very little productivity at the depths suggested here.

Page 14, line 32: "we can confirm that the repeated development of anoxic/euxinic conditions in the distal Cleveland Basin was most likely due to high primary productivity, and possibly salinity stratification due to high amounts of freshwater runoff". We encourage the authors to investigate the use of paleosalinity proxies to evaluate changes in freshwater runoff in this depositional system. Check this paper for paleosalinity analysis techniques:

Wei, W., Algeo, T.J., Lu, Y., Lu, Y., Liu, H., Zhang, S., Peng, L., Zhang, J. and Chen, L., 2018. Identifying marine incursions into the Paleogene Bohai Bay Basin lake system in northeastern China. International Journal of Coal Geology, 200, pp. 1-17.

Page 15, line 14: "The HVMIs in the present study bear similarities to the Gulf of California in that they exhibit similarities in Cd enrichment and Mn depletion". The 2016 study by Tim Sweere is highly relevant in this regard and should be cited:

Sweere, T., van den Boorn, S., Dickson, A.J. and Reichart, G.J., 2016. Definition of new trace-metal proxies for the controls on organic matter enrichment in marine sediments based on Mn, Co, Mo and Cd concentrations. Chemical Geology, 441, pp. 235-245.

Page 16, line 2: "While the studied interval shares similarities and differences with both upwelling and anoxic basin type settings, we are still lacking an appropriate modern analogue. Palaeogeography exerts a fundamental control on sedimentation, in particular, TOC enrichment, but there is no modern-day example of a shallow epicontinental seaway." Agreed, but the authors should consider the examples provided in Algeo et al. (2008):

Algeo, Thomas J., Philip H. Heckel, J. Barry Maynard, Ronald C. Blakey, Harry Rowe, B. R. Pratt, and C. Holmden. "Modern and ancient epeiric seas and the super-estuarine circulation model of marine anoxia." Dynamics of Epeiric Seas: Sedimentological, Paleontological and Geochemical Perspectives: Geological Association Canada Special Paper 48 (2008): 7-38.

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Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2018-172/cp-2018-172-RC1-supplement.pdf

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