

Interactive comment on “Rapid and sustained environmental responses to global warming: The Paleocene–Eocene Thermal Maximum in the eastern North Sea” by Ella W. Stokke et al.

Anonymous Referee #1

Received and published: 6 January 2021

Stokke and co-authors present a detailed multi-proxy record of sediments deposited on Fur, Denmark, that record an expression of the PETM. The authors discuss their data in terms of silicate weathering, basin deoxygenation, volcanic activity and productivity during this interval, and place their findings into a regional (North Sea) and global (carbon cycle processes) context.

The data are excellent and the study is interesting and highly worthy of being published. My comments are for the most part quite minor and I have outlined these line-by-line below. As a broad overview, I would urge the authors to do a few things: i) Standardise their use of elemental data to normalise to Al or enrichment factors. Uncertainties also

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need to be stated clearly. ii) Try to edit the text to bring out the main points more clearly. The current manuscript is long and as a reader I felt it lost focus in several places due to the level of detail presented. iii) Please make a better distinction between weathering rate and the character of weathered material. The CIA index (and clay mineralogy) is used as a proxy for a change in the source of weathered material but these data are frequently discussed in terms of 'amount' of weathering, which I found quite off putting. iv) There is quite a bit of repeated information in several of the (many) figures. I wonder if you could consider combining display items?

Line 112: Unit, not section.

Line 132: Be consistent with use of Palaeo. . . – it is Paleo. . . in some places.

Line 218: Apostrophe for sediments.

Section 3.4: Note that the use of HI to distinguish OM source relies on low thermal maturity. I would also suggest toning down the ~ 100 HI cut off of terrestrial and marine OM – the reality is that these distinctions are much more gradational and subject to mixing etc. The distinction should also lean on OI and Tmax values too.

Section 3.5: ICP-MS (and AES/OES) do not report major elements as oxides, but rather as elemental mass units. Have the authors converted these values to oxide wt equivalent here? Please also note the accuracy (certified RM) and precision (duplicates/RMSE of standards) of the measurements.

Line 268–270: CIA measured 'in situ' reflects the balance of weathering rate (removal of cations, substrate limited) and intensity (rate limited), not rate alone. Also note that marine sediments reflect source mixing – probably more important than hydrodynamic sorting.

Fig. 5: I'm a bit old fashioned but I like to see data points plotted on figures, rather than lines connecting invisible datapoints. This isn't an issue for the clay mineralogy but I would suggest modifying the SST and d_{13C} data displays.

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Line 367: There is a shift in interpretation from suboxic to anoxic conditions here for the Stolleklint Clay. The two are different – suboxic places you geochemically in the zone of NO₃⁻ / Fe(III) reduction whereas anoxia places you in a zone of sulfate reduction – biogeochemically very distinct. You probably have an oxygenated setting here with subtle – and occasional – shifts to elevated levels of porewater sulfate reduction.

Line 375: I'm not sure about saying 'variability' of the Fe/S data here, rather than the gradient. In fact the gradient changes so much, I wonder if plotting the pyrite Fe/S stoichiometric ratio would be useful to show where you might have 'excess' (non-pyrite) S accumulation.

Line 376: Better to say upsection rather than upwards.

Section 4.4.2: I'm a bit cagey about 'productivity' proxies in the way they are described here. Ba is only useful when present as exported barite, which can become reduced in suboxic porewaters – with the evidence you have for low oxygen conditions, I doubt that Ba/Al is useable in the way employed here and I would suggest removing it. Likewise P/Al is severely biased by the presence of inorganic phosphates that are not uncommon in deposits such as these.

Section 4.4.3: I have a few suggestions here, based around how the various proxies are presented. Firstly, the use of Th/U is a hangover from gamma logging. U reduction is what you are after here, so for consistency, just normalise it to Al and show the UCC ratio for comparison – or better yet, calculate an enrichment factor to compensate for dilution. Second, why isn't Mo normalised to Al like everything else? I suspect this is to enable comparison to the Scott and Lyons (2013) Mo cut-off of 30ppm – but I would suggest removing this. S+L is fine for the modern ocean where we know what the Mo concentration in seawater is, but is sketchy for palaeo-oceans where [Mo] (and other elements) in seawater can vary. Generally a lot of Mo qualitatively = sulfidic conditions and not much Mo = non-sulfidic (or slow sedimentation and adsorption to oxides).

Line 437: Process, not processes.

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Line 494: I like this section, but here (and in the results section dealing with clay minerals and CIA) a better distinction could be made better character of detrital material, and amount of detrital material. CIA and mineralogy trace the former, not the latter. Thus, statements such as 'increases in the flux of terrestrially derived material' need to be subtly altered to reflect this distinction.

Line 515: I don't disagree with the role of hydrological cycling as a driver of kaolinite deposition, but what about the transport time lag of source-sink? Sediments do not move instantaneously, so even after a sea-level fall one might expect a lag of several thousand years before seeing a depositional pulse.

Line 564: If you want to discern U enrichment, plot U/Al, not Th/U that requires the reader to invert the axis!

Line 580–583: As is well described in this section, Mo is a sulfidity indicator. So the conclusion that 'oxic' conditions did not deteriorate until after the CIE onset is not supported by these data (lots can happen before Mo starts to react). Th/U actually starts to decline before the CIE onset (Fig. 10) which might support the argument that 'oxic' conditions did deteriorate earlier.

Line 613 (and 701): This is the first mention of 'euxinic.' Better to stick to 'sulfidic' I think, because you don't have any diagnostic evidence here for free H₂S in the water column.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2020-150>, 2020.

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