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Climate of the Past Discussions

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 #2

Received and published: 9 March 2021

Stokke et al. reported new proxy data from Fur, Olst and Store Baelt of Denmark in the North Sea to reconstruct the paleoenvironment of the PETM in this region. This is an overall well-written study with a lot of geochemical data to investigate the environmental changes in response to the warming during the PETM. The paper should be published after the following comments/suggestions are addressed. I list the detailed comments below.

1) Euxinic condition

The authors argue for euxinic condition in the upper half of the PETM body using evidence of Mo (>30 ppm), S (4 wt%), and pyrite (7% of bulk) and low Th/U (<2 ppm).

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There is only one data point that shows Mo higher than 30 ppm, which does not correspond with the highest S and a scarce of pyrite data. The authors show that increase in S, pyrite and low Th/U may not be associated with suboxic and anoxic conditions prior to the PETM onset because of bioturbation, and occurrence of glauconite, rather they could be indicators of ash. This suggests that these proxies are not without controversies, and should provide another independent proxy of euxinia. The authors did mentione that previous studies have found biomarker the sulfur bound isorenieratane reported in (Schoon et al., 2015), although that work studied a different site. Therefore, I think the evidence of euxinia is lacking at the study site, because of the lack of data that show consistent > 30 ppm Mo or other independent biomarker data.

2) organic matter burial and export productivity vs. terrestrial organic carbon sequestration

The authors argue that the carbon cycle recovery is aided by increased silicate weathering and export production in the marine realm, rather than the terrestrial carbon sequestration as suggested by Bowen and Zachos (2010). I wonder if the authors could expand their discussion on why they dismiss the regrowth of terrestrial biosphere as a negative feedback mechanism?

3) Calculation of the Chemical Index of Alteration

Did you account for the CaO from the carbonate fraction? You might need the wt% CaCO3 to do that or follow the work of McLennan et al. (1993) to assume reasonable Ca/Na ratios of silicate. Another index that does not require the knowledge of CaO* is CIX (chemical index of alteration without CaO; Garzanti et al., 2014; Harnois, 1988). Please refer to Fedo et al. (1995) paper for the use of CIA with CaO* (which represents Ca in silicate-bearing minerals only), rather than CaO.

Other comments:

1. show the stratigraphic column in Figure 2 with lighologic log and geologic formations

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and Period. 2. Line 116: “hundreds of NAIP tephra layers . . .”: could you provide an age range for these tephra? 3. Line 129: do you mean “organic matter sequestration or burial” rather than “organic matter drawdown”? 4. Section 2. Field area and stratigraphy: Is it possible to provide a paleogeography map of the area? 5. Line 175: How is the CIE magnitude calculated? Please describe the pattern of the CIE, the plateau and recovery in terms of time. 6. Line 179: how is the sedimentation rate calculated? If it has previously published, please briefly describe how it was calculated. 7. Line 502-503: clarify which boundary is placed at Ash SK1, is it the Paleocene-Eocene boundary? 8. Line 539: change Cu, Ni and V to Cu/Al, Ni/Al, and V/Al to reflect what Fig. 10 shows. Also change Al2O3 to Al in Fig. 10 as suggested above. 9. Line 603-604: As previously indicated (Lines 572), the increase in S, U and V could be attributed to an increased ash component with the glauconitic silt, rather than indicating suboxic and anoxic conditions. Can you preclude the contributions from ash to drive up the S values? Same for the argument based on U enrichment below (lines 606-607). 10. Line 626-627: The sentence “An increase in TOC could reflect declining terrestrial influx, possibly due to increasing sea level. . .” seems lack of support. An increase in TOC could be either due to increase in delivery of terrestrial organic matter, or primary productivity/export productivity of marine organic matter, or increase in preservation due to anoxic conditions. I don’t see how an increase in TOC could reflect declining terrestrial influx. It could be that a decreased terrestrial influx along with increased marine primary productivity/export productivity/preservation may lead to an increase in TOC. Is there any evidence for sea level rise in the studied area? If so, a reference is needed for this statement. 11. Line 636: change “light” to “13C-depleted” 12. Line 637: should be “the long duration” of the CIE (or PETM)

Figures Fig. 3. Why is there a gap between Balder Fm. and Horda Fm.?

Fig. 5. Plot the data point rather than showing a line.

Fig. 10. The d13C and SST panel is way too narrow. I think this figure can be separated into two figures to highlight the details of the CIE and temperature. Similar to

Reviewer1, I also suggest plotting raw data points, rather than the smoothed line. The grey colored horizontal bar overlaps with the plot, please change it to another color.

Fig. 11. It is difficult to compare the productivity proxies and redox proxies to the %TOC because their resolution is very different.

Could you provide an image showing the sample preservation in the box core, in addition to the scanning images?

Data It will be helpful to list the analytical data in a table, including Ba (ppm), Al (ppm), etc. Also, why not showing Ba/Al instead of Ba/Al2O3? The productivity proxy is usually by Ba/Al (see Reviewer1 comments), but distinction between terrigenous vs. biogenic barium needs to be made.

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