Clim. Past Discuss.,
https://doi.org/10.5194/cp-2020-150-AC2, 2021

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.

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Received and published: 12 April 2021

We thank the reviewer for the constructive feedback, and have provided answers and comments following the structure of the review.

We would firstly like to clarify that we do not show any data from Ølst and Storebelt, as also described in the field area and sampling sections. We assume this misunderstanding is based on the fact that we included these localities in figure 1. This was done to help the reader understand the local geography as these localities are mentioned in the text due to other important studies having focused on these places. We

have now altered figure 1 and hope that will deter any confusion in the future.

Reply to the reviewers main comments:

1) We would argue that there is not just 1, but 6 datapoints where Mo is > 30, making the entire upper half of the PETM very high in Mo, and both S and pyrite is declining in the uppermost 4 of these. As a result of the review's suggestions, we have chosen to switch to enrichment factors and subsequently not compare with the 30 ppm cutoff due to issues with comparison with modern seawater. This does however, not change the fact that the upper half of the PETM is very high on both Mo and U. These elements are not increasing in the ash-rich section pre-PETM. On the contrary, UEF is rather depleted at these intervals. There are also overall no known great connection between ash content and high sedimentary U, Mo, or indeed S content. There is therefore not reason to believe that the increase in these during the PETM is related to volcanic ash. Especially as there is limited evidence for extensive ash deposition during the PETM body. Regarding the study of Schoon et al. (2015), their data is from the exact same location, as they present data both from Stolleklint and Store Bælt. There is therefore in fact found sulfur bound isorenieratane in the exact same section as we are working. We will change the text to avoid any confusion on this point. Although no proxies are without complications, we believe that the combination of so many proxies suggesting lowered oxygen conditions during the PETM is fairly good evidence of this fact. When that is said, we will based on both reviewers comments refrain from using the term euxinic, and rather stay with anoxic and sulfidic.

2) We do not intentionally dismiss this theory, but we have not focused particularly on it. Our data show an increase in marine OM up section, which does not lend support to the importance of growth of terrestrial biosphere. However, it does not exclude it neither, as there are many factors controlling the relative distribution of terrestrial and marine OM such as changes in sea level, and we know for a fact that the sea-level rose. There are many theories regarding the termination of the PETM, and we cannot in the scope of our paper go into a detailed discussion of all. As our data does not really support

nor exclude the regrowth of terrestrial biosphere as an important negative feedback mechanism, we believe greater focus on this hypothesis is beyond the scope of this paper. Yet, we do cite the paper, and have now mentioned the theory more specifically. Our data is from a marine section, and we suggest using other sections to test the strength of Bowen and Zachos (2010).

3) We had not taken carbonate fraction into account, as this is minimal in the analyzed sediments. However, we agree that it is better practice to do so, and following the reviewers recommendations we have now included this. Still, it does not significantly alter the results and has no impact on the interpretation.

Reply to the reviewers other comments:

1. Based on comments of reviewer 1, we have combined figures 2 and 5 to limit the amount of figures. We have also altered what is now figure 4 to include the period and epoch.

2. Line 116: In the introduction, we refer to the overall ash production, not just the ashes included in the part of the section we are working. The age range for the full Danish ash series is noted in figure 3 (now fig. 2) and for the studies section it is noted in figure 5 (now fig. 4). We have now also stated the age range more clearly in the text and added a reference to figure 3.

3. Line 129: We have changed this sentence.

4. Section 2: We have palaeogeographic maps of the area both in figures 1 and 12 (now fig. 11).

5. Line 175: It has not been possible to perform any astronomical calibration of the PETM CIE at Fur, and we therefore cannot describe the pattern in terms of time beside what is already assumed as the general timing of the PETM. We have now stated this in the text, and we refer to figure 5 (now fig. 4) where we have noted what timings we know.

6. Line 179: We have included a brief explanation of this in the text.

7. Line 502-503: We are referring to the lithological boundary between the Stolleklint Clay and the underlying Holmehus/Østerrende Fm. The exact position of this is a bit uncertain, but based on the PCA of the XRF scanning data we assume it is placed here. We have changed the text to clarify.

8. Line 539: We have now changed to enrichment factors, so this is no longer an issue.

9. Line 603-604: The reason we suggest that the increase in particularly V and S below ash SK1 could be due to a high ash component, is because there are ample chemical evidence of a high degree of ash within these few cm of sediment. This we have from the XRF core scans, the ICP-MS analyses, and not least from the fact that there is deposition of two thick ash layers just above. That means that we may be analyzing just as much ash as sediment in our bulk-rock analyses. This is not the case within the upper PETM body. While there may be some indication of potential thin cryptotephras, this evidence is limited. There are also no major ash layers deposited, and no evidence of bioturbation that could redistribute ash into the sediments. There is also not really very well known that volcanic derived material is particularly rich in U. On the contrary, U is known to be a redox sensitive element with little detrital influence (e.g. Tribovillard et al., 2006). Combined with the high TOC content and Mo enrichment, as well as the sedimentological evidence that the clay is almost black, it is ample evidence that the sediments are likely to be highly anoxic in this part of the section. If ash plays a role in elemental enrichment here, it is comparatively negligible.

10. Line 626-627: We refer here to the fact that sometimes a decrease in detrital input can give the illusion that there is an enhanced preservation of OM, as suggested in Carmichael et al. (2017) that we also refer to in the text. However, we do also reject this theory as a possible cause in the paper. In the revised version, we have omitted this sentence, as it really is of minor importance.

11. Line 636: We have changed this sentence.

12. Line 637: We have changed this sentence.

Figures Fig. 3: The gaps in the stratigraphy refer periods of no deposition or erosion, so-called hiatuses. The stratigraphy in the figure is based on King (2016) and Schiøler et al. (2007), which can provide a more thorough explanation of the central North Sea stratigraphy.

Fig. 5: We have now plotted data points.

Fig. 10: The d13C and SST data is shown in greater detail in figure 2 (now figure 4), and were mainly included in figures 10 and 11 for comparative purposes. We have now changed the figures to have a slightly wider panel and plotted the data points. We do not agree that it would be appropriate to separate the figure into 2 figures, as it is easier to compare the data when plotted together. They grey horizontal lines represent ash layers and are supposed to cut the vertical plot lines. We have made them darker to avoid confusion.

Fig. 11: Yes, TOC and trace metals are normally given with different scales. We are not sure what the reviewer wishes us to do about that.

Provide image of sample preservation of box cores: We do not believe it would be constructive to add yet another image of the box-cores as there are already two of each. We refer the reviewer to figures 6 and 7 (now 5 and 6) for pictures of the box cores. A note on sample preservation has been added to the figure text. The sample preservation of the box cores is good, as the sediments were overall soft and easy to sample. There were some cracks in the cores covering the onset that are evident in the images already provided. These were avoided during XRF scanning, which was conducted on a smoothed plane surface along the middle.

Listing analytical data, and use of Ba/Al: Based on the reviewer's comments, we have decided to remove the productivity proxies Ba and P2O5. We do not have sufficient data to distinguish between terrigenous vs biogenic barium, and as there seems to be

extensive uncertainty in using whole-rock sedimentary Ba as a productivity proxy we decide that the safest option is to omit this from the study.

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