Clim. Past Discuss.,
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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.

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We thank the reviewer for the thoughtful comments and suggestions. We have tried to provide a reply to each of the reviewers comments point by point:

i) We did consider using enrichment factors already before the initial submission, but after some deliberation decided on the current presentation in order to be able to compare with certain other studies. Changing to enrichment factors is therefore relatively easy and we have chosen to follow the reviewer's recommendations on this issue. This is also following the recent recommendations from Algeo and Liu (2020), which seems

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appropriate. Uncertainties are now stated more clearly in the supplementary material.

ii) We have tried to make each paragraph more succinct and focused to clarify the main points of the paper.

iii) We agree on the importance of this distinction, and we have changed the text both in the methods and discussion to clarify this. It does not alter our interpretation, as our main point is that the rise in the kaolinite and CIA does not reflect an increase in weathering, but rather a change in sediment distribution due to increased precipitation on an already weathered landscape.

iv) We assume the reviewer are referring to the inclusion of the δ13C and SST curves in figures 2, 5, 10, and 11. Figure 2 is included to get an overall overview of the stratigraphy, but we have now combined this figure with figure 5 (now figure 4) to limit the amount of figures. The inclusion of δ13C and SST also in figures 10, and 11 (now 9 and 10) is done in order to make it easier for the reader to place the data in a comparative framework. We do not think it would make it easier for the reader by combining any more of these figures, as they are already relatively data heavy. We are presenting a lot of analytical data combined with a comprehensive site description in this paper, and we therefore feel that this figure setup is necessary to provide clarity for the reader.

Line 112: We have changed this sentence.

Line 132: We believe that we are using the correct nomenclature by using "Paleo" when writing "Paleocene". Paleocene is an epoch that was included much later than the other epochs of the Tertiary/Palaeogene. It was first named not by Charles Lyell, but by the French geologist Wilhelm Philipp Schimper who wrote the name "Paléocène" in French fashion. It means the old Eocene and is an abbreviation of Palaeo and Eocene, therefore written as Pal+Eocene. Were it Palaeo+cene it would mean old recent, which does not make sense. However, if this is an important point for the editor, we will change the wording to Palaeocene.

Line 218: We have changed this sentence.

Section 3.4: As already stated in the result section, the low Tmax values (<422 °C) indicates that the organic matter is immature. We have now stated this more clearly by also citing our previous study (Stokke et al., 2020a) were we show a dominating odd over even preference in long-chained n-alkanes that indicate thermally immature organic matter. It is therefore reasonable to assume that the OM is immature and that we can to some degree use the HI index to indicate OM sources. When that is said, we agree that we can tone down the cut-off and rather focus on the overall variability. We have also now included the OI in the figure, not just in the supplement as before, and have altered the text to take this factor more into consideration. However, we find that these changes does not significantly alter the interpretation that we have an overall increase in marine derived OM up-section. This is also consistent with previous analyses of n-alkanes (Stokke et al., 2020a) where the TAR ratio show an overall upward decrease in terrigenous OM. We have now included the TAR curve in figures 10 and 11 to support the RockEval data.

Section 3.5: The values are converted to oxide wt equivalents, this is now stated in the method section. We have added accuracy and precision in the supplementary material.

Line 268–270: We agree that these are also important factors, and have added the additional definitions to clarify.

Fig. 5: We plotted without data points for simplification, as the points are shown in what is now figure 4. However, it is no problem including them, and we have therefore edited the figure accordingly.

Line 367: We have edited the result section to make it overall less interpretive and as much as possible purely descriptive. The use of these terms are therefore no longer included in this section. We agree that the distinction between suboxic and anoxic is important, and will strive to include this in the discussion. However, we find it difficult to understand how the reviewer arrive at the conclusion that the environment were partly oxygenated within the lower laminated Stolleklint Clay. While the lamination certainly indicate some regular variation in oxygenation, there are no indication that the environment at any point were actually oxygenated. There is for instance no evidence of bottom-dwelling organisms. On the contrary, there are evidence of green sulfur bacteria, and enrichment of both U and Mo. We therefore do not feel like our discussion should be altered to say it is partly oxygenated in this part of the section, as there just doesn't seem to be any convincing evidence of the fact.

Line 375: We are not entirely sure what the reviewer refer to as the gradient. We are referring to the Biplot of S and Fe counts from the XRF core scans, and try to show that the sediments in the upper dark part of the PETM body have a more homogenous high S concentrations, while lower down in the laminated part the S concentration vary a lot between dark and light laminations. We will try to change the sentence to make this statement clearer.

Line 376: We have changed this sentence.

Section 4.4.2: We agree that these proxies are a bit uncertain, and we will remove them from the paper. '

Section 4.4.3: We thank the reviewer for good suggestions, and have followed the recommendation to switch to enrichment factors. The use of Mo concentrations without normalization was, as guessed, in order to compare with the Scott and Lyon paper. We agree that taking in account the uncertainties in the palaeo-ocean chemistry it may be better to avoid any direct comparison.

Line 437: We have changed this sentence.

Line 494: Again, we agree that this distinction is important, and in line with what we try to communicate. We have altered the text accordingly to clarify the distinction.

Line 515: A potential time-lag is of course relevant, but we still think it is fair to assume hydrological cycling is the main factor, as this area is not that far from the main kaolinite source area and still it post-dates sea level fall, although of course we don't know the exact timing of the CIE curve. In the Kilda basin (Kender et al., 2012) in the central North Sea we see an increase in kaolinite pre-PETM, most likely due to tectonic uplift of the Scotland-Shetland-Faroe area. Kaolinite is a heavy mineral and not likely to be transported very far. It is therefore likely that the kaolinite at Fur is sourced from the Fennoscandian shield (as also suggested by Nielsen et al., 2015). We therefore think there is ample argument to assume it reflects hydrological cycling and not tectonic activity. Still, source-sink lag is absolutely important, but that would only suggest that the hydrological response to the PETM is even more rapid, as we see the sediment reaction a bit delayed.

Line 564: This will no longer be an issue, as we have switched to enrichment factors.

Line 580–583: We agree that the evidence is not conclusive as to exactly when oxic conditions start to deteriorate, as the influence of ash is complicating the interpretation. However, using enrichment factors, we do see that U is in fact depleted until after the PETM onset. Still, we will note the uncertainty more clearly in the text.

Line 613 (and 701): We have changed the paper to state the uncertainty regarding the presence of euxinic conditions, and conclude with sulfidic.

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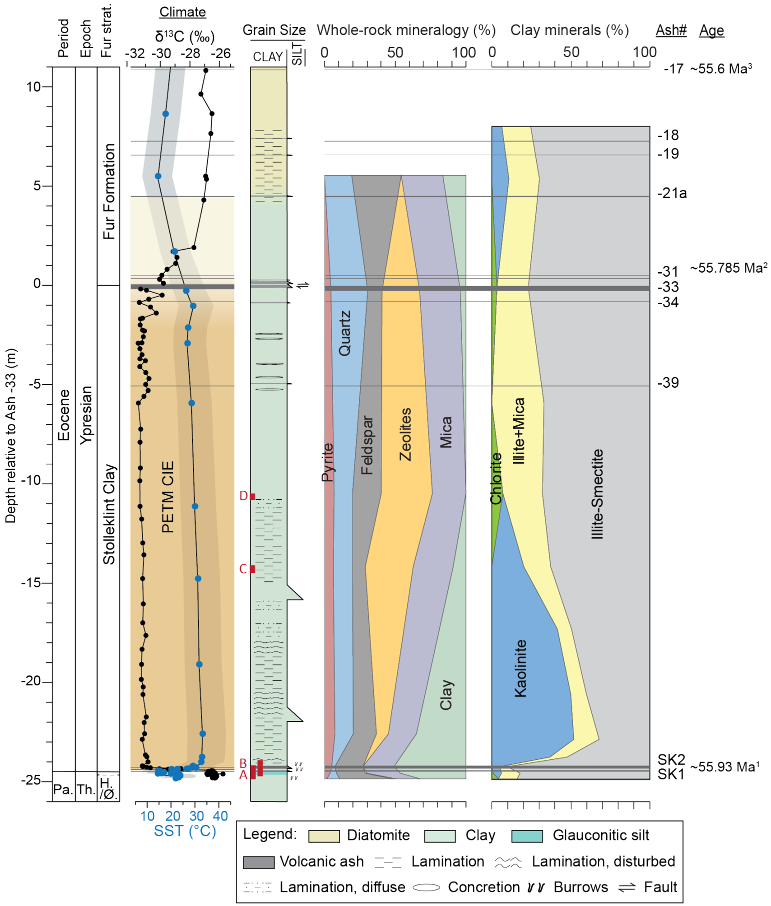

Fig. 1. The new figure 4, combining figure 2 and 5.

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