Review 2

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Review of Methner et al. Clim. Past. – first version.

Summary

The manuscript presented provides (1) new stable carbon isotope data across part of the Schoningen lignites (2) detailed palynological assemblage data from the same interval and (3) a comparison with other NW European lignites which are argued to be time-equivalent. With these data, the authors aim to resolve the response of wetland/peat systems to global warming across the Paleocene-Eocene Thermal Maximum.

I agree with the authors that it is of great importance to understand the behavior of wetland/peat systems in warm(ing) climates and this is an appropriate subject for this journal.

The results lead the authors to several general conclusions about the analyzed section, most of which I have no serious concerns about, including how the influences of increased fire activity (seasonal drying) and drowning due to higher relative sea level led to the (local) demise of these peat mire systems.

However, I think it is uncertain whether the comparison to other records is solid and this potentially has major implications for the regional picture and the extrapolations to past and present global warming. The regional comparison is mainly built on the assumption that the sections are time equivalent (major concern #1) and that relatively small variability (~2‰) in carbon isotopes in bulk organic matter within heterogenous lithological columns is indicative of the PETM or a similar hyperthermal event (major concern #2).

- This is a major issue that has been similarly addressed by the comments and reviews#1. We acknowledge that in the first version of the manuscript a clear assignment of our detected CIE to the PETM might have been too bold.
- Identical reply to comment by Gerald Dickens: We acknowledge this by
  - (1) adding a whole new section (now section 2) to the manuscript in order to describe the available age constraints and the pitfalls/discrepancies of them in more detail (section 2; p. 3, ln. 13 – p. 4, ln. 10)
  - (2) being more careful in our wording while describing solely the CIE and not unequivocally relating the CIE to the PETM, ETM2 or any other Early Eocene hyperthermal (section 4.1; p. 7, ln. 20-24).
  - (3) discussing tentatively the possible assignment of the CIE to the PETM vs. any other Early Eocene hyperthermal (section 4.2; p. 8, ln. 16-20)
- We rephrased major parts of section 3.1, now section 4.1 (p. 6/7) and included a statement that we compare the European wetland records despite the possibility that they may reflect different hyperthermal events in section 4.2 (p. 7, ln. 31 – p.8, ln. 6) and 4.4 (p. 12, ln. 25-29).
- In order to place our results in a more regional framework, we still perform the comparison with nearby lignite sites (Cobham, Vasterival) in which the reported CIEs have been assigned to the PETM. We feel, in agreement with this comment, that this comparison might still be valuable to detect similar behaviors of these Paleogene wetlands during carbon cycle perturbations. However, we now clearly state that there

is no sufficient proof that these records are time-equivalent as they all have their limitations when it comes to age assignment (in section 4.1, 4.2 and 4.4).

- In addition to this, the comparison to the near-by paleo-North Sea lignite records (Cobham and Vasterival) has interesting implications, in case that the detected CIE is not related to the PETM. The similarities between the records may arise from the fact that they represent the same hyperthermal (PETM or any other Early Eocene hyperthermals) or that different hyperthermals have similar effects to mid-latitudinal wetlands in the paleo-North Sea realm. We now address this in section 4.4 (similar to reply to review#1). However, we only state observations at this point and we try to be as careful as possible without judging upon the pitfalls of other studies.
- A minor remark: The whole lithological column is indeed heterogeneous (alternating lignite seams and marine interbeds) however, the discussed CIE occurs only within the lignite (Seam 1) and independent of any detectable lithological change. According to Collinson et al (2003), the same applies to the Cobham section.

Major concern #1

Dating lignite sequences is notoriously difficult and extreme caution is warranted when correlating these deposits to geologically very short, in this case 50-200 kyr, events. In an earlier publication (Riegel et al. 2012), three authors of this manuscript show that another part of the same sequence (seam #6) is associated with substantial amounts of *Apectodinium*. Also below the here presented *Apectodinium* acme, there is a smaller distinct abundance spike in *Apectodinium* (bottom marine interbed #1). This spike, based on previous correlations that the authors also mention here (p. 3, lines 12-16), could be placed close to the P/E boundary, but also still **above** the P/E.

Importantly, high percentages of *Apectodinium* in other Paleocene and Eocene successions from mid and high latitude settings are not strictly limited to the PETM or even hyperthermal events (examples include Bijl et al., 2013; Frieling et al., 2018; Heilmann-Clausen, 2018; Sluijs et al., 2011). I think the authors claim in p. 3, lines 8-12 should be rephrased to accommodate these observations and potential implications thereof.

- This is a valid request and has similarly addressed by the other reviewer and in the short comments. In order to address the problematic age constraints and to discuss the pitfalls of using the Apectodinium acme to infer the PETM, we included a new section (section 2; p. 3, ln. 13 – p. 4, ln. 10).

Assuming the CIE is not an artifact of preservation or source changes (see also below) there is as much evidence to connect the carbon isotope excursion here to the PETM as to any other early Eocene hyperthermal. If the age of the analyzed section cannot be constrained sufficiently, a detailed comparison with other lignites (Vasterival / Cobham) including the carbon isotope changes would become more complicated and would require more nuanced statements.

- We fully agree with this statement. We, hopefully, fulfilled this requirement and provide more clarity to the reader by (1) describing the CIE in a more general way (section 4.1), (2) discuss the possible assignment to the PETM vs. any other Early Eocene hyperthermal (section 4.2), and (3) make the comparison to the other lignite records pointing to the striking similarities, but with a more tentative interpretation of these (section 4.2 and 4.4) (see reply to review#1 and short comment #2).

Hopefully, the authors can show the presence of PETM marker species, either dinocysts (*Apectodinium augustum*, high variability of morphology within the *Apectodinium*/Wetzellioid group (e.g. Iakovleva, 2016) or pollen (comparison with Eldrett et al., 2014; Willumsen, 2004). Likewise, if there are identifiable ash layers within the sequence this could be a welcome addition to resolve the local stratigraphy (e.g. (Heilmann-Clausen, 2018; Jones et al., 2019; Westerhold et al., 2009). If the correlations to other localities cannot be made with confidence, I think the comparison with other lignites and pollen studies should be rewrittento paint a much more general picture (see also point 2).

- Firstly, as already stated above, we now address the available age constraints more prominently in the new section 2 (p. 3, ln. 13 – p. 4, ln. 10).
- Secondly, there is no ultimate proof that our detected CIE is correlative to the CIEs in the lignite records (that have been assigned to the PETM). However, we feel confident that a comparison still yields interesting results (see reply above).

Major concern #2

The carbon isotope signal is from integrated bulk organic matter, implying that large changes can occur if any of the following factors play a role.

1. There is likely to be a difference in the marine/terrestrial fraction within the bulk organic matter across the lithological transitions, but this may not be entirely limited to these transitions. Hopefully, the authors can show from their palynological assessment how the marine/ terrestrial fraction varies across the lithological transitions. This is of vital importance as marine and terrestrial organic matter sources can be offset by ~4‰ (cf. (Sluijs and Dickens, 2012)). The palynological data already allows a preliminary assessment of marine/terrestrial fractions, possibly without any further analyses.

- This is an important point when assessing bulk organic carbon isotope values. There are several lines of evidence to exclude mixing between marine and terrestrial derived organic matter as the main reason for the CIE:
    - The palynological data records rather defined transitions between the terrestrial peat deposits and the marine clastic interbeds.
    - The transitions between the lignite seams and the marine interbed are rather abrupt within a cm-dm scale.
    - Samples that experienced possible mixing (base of Interbed 2) have been discussed in the manuscript (p. 5, ln. 26-29, now p. 7, ln. 6-7). Most obvious is the %TOC data with %TOC > 50% typical for the lignite seams.
    - The deduced CIE occurs fully within the lignite Seam 1 not showing any indication of mixing at this stratigraphic level.
- As this is a potential major pitfall for the interpretations, we discuss this issue in more detail in the manuscript (p. 6, ln. 29 – p. 7, ln. 5 and p. 7, ln. 15-19)

2. The preservation-regime may be very different in marine and terrestrial environments, which could also skew the relative fractions of marine (aquatic) and terrestrial OM, the latter being more resistant to oxidation (Huguet et al., 2008).

- Similarly to the arguments above, the discussed onset of the CIE occurs within the lignite seam, unrelated to the marine interbed.
- The $\delta^{13}C_{TOC}$ values during the inferred CIE (upper part of Seam 1) and the marine interbed are indeed different. We attributed this to a possible mixing, however, following the comment of the reviewer (see below), we reevaluated this statement and now elaborate on the possible different fractions of the OM (p. 6, ln. 29 – p. 7, ln. 5).

3. The authors mention potential bacterial influence on carbon isotope signals across the "CIE" events and show a comparison with other sources, showing lignites are essentially recording a muted isotope signal. This also ties in with point #1 and should be assessed in more detail. I encourage the authors to explore alternative possibilities of forming a CIE in a lignite-marine intercalated sequence.

- As mentioned above, we cannot detect any marine influence in our CIE. However, to improve/clarify the information to the reader, we now included an additional paragraph in the main text (p. 6, ln. 29 – p. 7, ln. 5 and p. 7, ln. 15-19).

4. The completeness of the section is not addressed in detail at the moment and should be expanded upon. In laterally heterogenous sequences, which include sharp lithological transitions, it seems likely that there are smaller and/or larger hiatuses, which appear in the record as a sharp isotope shift, if imposed on a long-term isotope trend.

- This is a valid request. As mentioned above, the transitions between the seams and the marine interbeds are abrupt, however, there is no evidence for any major hiatus. But again, the onset of the CIE occurs within the lignite seam and we could not detect any marine influence at this stratigraphic level. We now address this issue more upfront in the method section (p. 4, ln. 20-22) and again in section 4.1, p. 7, ln. 15-19.

5. The carbon isotope signature of charcoal can be depleted by up to 2‰ relative to the source material (Ascough et al., 2008). As such, even without source or vegetation changes, a change in fire regime could result in a negative CIE in a lignite record. This is particularly worrying if the CIE onset coincides with a charcoal spike in the record and raises the question whether such smaller carbon isotope trends in these deposits are perhaps always locally induced.

- This is an interesting remark that we appreciate very much. Indeed, an increased amount of charcoal has been detected in Seam 1 (and Seam 2) compared to the subsequent seams (Robson et al. 2009).
- However, this would mean that almost the entire CIE (89% given the CIE onset of -1.77‰ and 63% given a CIE of -1.26‰ by the means) would be due to the presence of charcoal in the uppermost samples of Seam 1. Such a change in the charcoal content in the upper samples of Seam 1 has not been observed. Instead, a similar amount of charcoal has been recorded in all of these samples (see Appendix SI1).

This also applies to most other previously analyzed lignite / marine sandstone records that have been interpreted in similar manners, but without scrutiny of the isotope trends. I think with the current knowledge, the authors can contribute significantly to a much more solid discussion on interpreting carbon isotope records in lignite sequences.

- We thank the reviewer for the detailed comments on the stable isotope data. We included new paragraphs in section 3.1 (sampling) and 4.1 (CIE discussion) to address the issues raised above.

Minor comments

P2. Lines 6-9. The global warming is around 4-5 oC, see (Dunkley Jones et al., 2013; Frieling et al., 2017). Local warming is occasionally amplified to ~10oC (e.g. Schoon et al., 2015).

- We followed the suggestion of the reviewer and rephrased this together with a major part of the introduction to account for the shifted foci of the manuscript (p. 1, ln. 8-10).

P2. Line 11. “two-step” is confusing here, can be removed.

- We followed the suggestion of the reviewer and changed the wording accordingly (p. 2, ln. 11).

P2. Lines 23-24. The transition between these paragraphs is rather abrupt and the two seem somewhat disconnected. Can you clarify the reasoning here?

- We appreciate this comment very much. Due to major restructuring of the manuscript (see also above), this issue has hopefully been resolved (c.f. Introduction, p. 2/3).

P2. Lines 25-26. On what time scales are these wetlands important for carbon cycling?

- This is not a trivial answer. Wetlands act on very different timescales either as sources or as sinks of carbon. This depends on the climatic regime, the organic input (down to species level, e.g.), hydrological regime (groundwater influences or fluvial export of DIC), as well as the depositional environment. In general, wetlands have high carbon turnover rates (tens of days). Whereas, burning of peat releases vast amounts of carbon to the atmosphere within days, weeks or even month. Carbon storage acts on longer timescales by building up peat and burial of peat to form lignite (and coal + natural gas), e.g. Eocene or Miocene lignite deposits.
- Given that wetlands can act as both sources and sinks/storage of carbon (dep. on timescale and climatic as well as depositional regimes, we feel that including such discussions into the manuscript would be beyond the scope of the paper.

P3. Lines 12-21. I have some difficulty following the reasoning here: at first, you state the correlations placed the PETM within or below the main seam but then quote an age (54.8-54.4 Ma) which does not align with that statement or the analyses of a lignite/marine interbed above the interval that was originally correlated to the PETM?

- As stated above, we address this in a new section (section 2, p. 3, ln. 13 – p. 4, ln. 10).

P4. Lines 28-29. Palynological treatment with hydrogen peroxide and KOH will result in loss of some fragile palynomorphs, including dinoflagellate cyst species. If present, Peridinioids with hexa-2a archaeopyles (e.g. *Senegalinium*, *Phthanoperidinium*, *Lejeunecysta* etc.) are probably affected worst (e.g. Zonneveld et al., 2019). Unfortunately, these are also low-salinity tolerant species. While it is difficult to assess what the exact influence of this is on the assemblage study, it should at least be acknowledged that there may be an effect.

- We think that this is a justified remark of the reviewer and addressed this in the method section (p. 5, ln. 20-21). In several previous projects we have received rich and diverse dinocyst assemblages by the same procedures without any sign for selective degradation (Lenz 2005; Lenz et al 2007; Riegel et al. 2015).

P7. Lines 28-30. The exact opposite should be the case for the bulk organic matter, given that Paleogene marine organic matter is more 13C-depleted (see Sluijs & Dickens, 2012).

- We appreciate this comment and elaborate on this in the manuscript text (p. 6, ln. 29 – p. 7, ln. 5) as we feel that this observation even strengthen the description of an early Paleogene CIE.
- Our $\delta^{13}C_{TOC}$ values from the marine interbed hit exactly the given "background marine endmember" $\delta^{13}C$ value (Sluijs and Dickens, 2012), which may call for a fully marine but non-PETM signal. Marine PETM-related $\delta^{13}C_{TOC}$ values (at least at the Lomonosov Ridge) are ~3 ‰ lower if they are largely unaffected by terrestrial carbon input, but tend to be almost identical to the "background marine endmember" value under high terrestrial TOC input (~ -26 to -27 ‰).
- Given the near coastal setting of the Schöningen locality during the early Paleogene we can expect a significant terrestrial contribution to the TOC. Indeed can be observed by the presence of terrestrial palynomorphs in the marine interbed (Fig. 5). The average $\delta^{13}C_{TOC}$ value of -27.7 ‰ of interbed 2 falls exactly into the range of values for PETM-related $\delta^{13}C_{TOC}$ values with high (~80%) terrestrial contribution. Thus, the "elevated" (compared to the lignite $\delta^{13}C_{TOC}$ values) $\delta^{13}C_{TOC}$ values of the clastic interbed are in good agreement with the CIE/PETM-related marine $\delta^{13}C_{TOC}$ values of Sluijs and Dickens (2012) and thus support our finding that the CIE at Schöningen comprises the whole interbed 2 and extended into seam 2.

P9. Line 22-30. How similar are these assemblages to other localities in the same area across the PETM (e.g. Eldrett et al. 2014, Willumsen, 2004)?

- Both papers are now considered in the main text (p. 10, ln. 22 and p. 12, ln. 14). We report now that in some Late Paleocene/Early Eocene records around the North Sea basin significant influences of short-term thermal events such as the PETM on the composition of the vegetation have been recognized (e.g. Eldrett et al., 2013). However, for the Schöningen record, respectively, for the relatively short section that we have analyzed so far we have indications that the vegetation changes only follow natural successions and are not related to other factors such as climate influences. Therefore, we mention that we need the long-term record of the Schöningen succession to identify the climate influence (p. 12, ln. 20-23). This is part of our ongoing study of the Schöningen record.
- The palynomorph assemblages from Schöningen reveal close similarities with the Danish flora reported by Willumsen (2013) (p. 10 ln. 21-22). However, this is only briefly touched upon as the main focus of the paper lies in the comparison of the lignite records/wetland deposits.

P10. Lines 17-20. Can it be excluded that every signal here is autocyclic, and simply reflects the natural progression of a wetland system?

- We do not intent to exclude this. Instead we clearly state in the manuscript we state that this follows a natural transition (p. 1, ln. 21-22; p. 12, ln. 22-23; p. 13, ln. 26-27).

Figures 2, 3 & 5: It would be helpful for the reader to have a detailed correlation between the section analyzed for carbon isotopes and palynology and/or the carbon isotope profile should be included in Figure 5. At present, the different height/depth scales of the two analyzed sections make it difficult to see what is connected to what.

- We thank the reviewer for this valid remark and the opportunity to improve the figures and provide better understanding for the reader. We changed the figure 5 accordingly.
- In particular, Figures 2 and 3 show the detailed carbon isotope data with the identical stratigraphy. Thus, we did not change these figures. However, we follow the suggestion of the Reviewer and included the isotopic data. The sections have been correlated according to the top of Seam 1 and base of Seam 2.

References

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