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).

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.

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.
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 rewritten to paint a much more general picture (see also point 2).

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.

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).

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.

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.

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 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.

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).

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

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

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

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?

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.

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).

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)?

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

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.

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