Review 1

Anonymous Referee #1

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The manuscript by Methner et al. present new interesting datasets that represent a great contribution to the available terrestrial records from Eocene sections. The authors present new d13C_TOC and TOC (%) data, as well as palynological data from the mid-latitude coastal site of Schoningen. The main conclusion is the identification of the PETM in the CIE represented by the onset of a negative excursion in d13C TOC in Seam 1 and a positive shift in Seam 2, as well as the presence of Apectodium in the marine interbed comprised between the two lignite seams.

The paper is well-written and properly structured and addresses interesting scientific questions which fit the scope of publications in Climate of the Past. Figures and tables are of good quality and relevant to the manuscript. However, I do recommend to rethink and reframe the manuscript because of the following problems.

1) The identification of the PETM. My main concern is that the evidence brought forward to define the CIE as the PETM should be discussed with care. The authors briefly acknowledge that the identification of the PETM in this interval has been suggested before by Riegel et al., (2012) but fail to discuss the possible pitfalls of this assumption:

a) Ages: possible age discrepancies in the dating of the section should be discussed in the manuscript. I refer to the detailed comment by Carlos Jaramillo, who has already noted that “independent” dating by Brandes et al (2012) is relying on ages related to the dinoflagellate zones. This should be addressed in a section of the manuscript

- This is a valid and important request and in concert with the concerns of review #2 and the two posted comments stating that the age constraints for the Schöningen record is rather weak and suggesting the possibility that the detected CIE reflects another Early Eocene hyperthermal event, e.g. the ETM2, rather than the PETM.
- Given that this is a major concern of all reviews/comments, we now address this problem more upfront and therefore, included a new section (section 2; p. 3, ln. 13 – p. 4, ln. 10) to give detailed information about the available age constraints and difficulties in local correlations (between the Schöningen open pit and the Emmerstedt drill core).

b) Thickness: a 10-m thick CIE; this requires a very high sedimentation rate (as noted in line 13) but is this reasonable? How is this changing from one lithology to the other in the transition from lignite to the marine interseam? Also, if we accept an almost linear sedimentation rate (_0.5 Myr/m) for the whole section, then the duration of the CIE doubles. How can you exclude that this is not the case?

- For the sedimentation rates we refer to the basin modeling study of Brandes et al. (2012), who detected a high mean sedimentation rate of 60-80 m/Ma. (see also reply to comment of Carlos Jaramillo, issue 3). We cannot exclude that the sedimentation rates differ from the reported ones, but currently cannot refine these as this would require further sedimentological and/or geochronological investigations that go beyond the scope of this study. However, Brandes’ basin model relies on the same

age constraints as our study, the argument that the duration of the CIE agrees with the duration of the PETM is circular.

- Similar to the reply to comment#1 and #2: Due to major rewriting of the manuscript now focusing on the description of the CIE rather than its assignment to the PETM, we do not feel that calculation the event duration in such an environment is robust or required and, therefore, we deleted parts of this paragraph.

And how can you exclude that the CIE is not, for example, the expression of ETM2/H2 hyperthermals (which would together last about 400 kyr)?

- Currently, we cannot exclude this possibility that the CIE reflect an Early Eocene hyperthermal (ETM2 or any other) (see discussion about the weakness of the available age constraints). Please see also the reply below (general suggestions) and the reply to comments by Gerald Dickens.

c) Hyperthermals: the acme of the Apectodium is the strongest evidence use to correlated this interval with the PETM. This is quite a compelling evidence, however, as noted by Jerry Dickens in his comment, Apectodinium augustum is the diagnostic species found in PETM section. Is there evidence for its presence?

- Unfortunately, there is no evidence for Apectodinium augustum (now assigned to Axiodinium; see text) in the marine Interbed 2, but we discuss this and also the possible reasoning for the lack of the species in the newly added section 2. The lack of A. augustum persists in the over- and underlying Interbeds. This may, therefore, arise from the paleoecological setting (see discussion in section 2). At the moment we cannot resolve this question and, thus, address this issue more prominently (new section 2) and in an open way to the reader (p. 7, ln. 20-24; p. 7, ln. 31 – p.8, ln. 6; p. 8, ln. 16-20; p. 12, ln. 25-29).

In general, I recommend incorporating those points in the discussion, discussing more in detail all the possible pitfalls associated with a univocal identification of the PETM as well as the strong evidence in support of it.

- We appreciate this suggestion very much. As stated above and in the replies to the two comments, we now address the problematic age assignment more prominently (own section 2).
- To provide more clarity to the reader, we first describe the CIE in a more general way, not relating it to a particular hyperthermal and then discuss the possibility that the detected CIE reflect another Eocene hyperthermal (not the PETM) (section 4.1, p. 7, ln. 20-24).
- Especially the comparison to the near-by paleo-North Sea lignite records (Cobham and Vasterival) has interesting implication, 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 clearly state that there is no unequivocal proof that the three lignite records reflect the same event (section 4.2: p. 8, ln. 16-20 and p. 7, ln. 31 – p.8, ln. 6 and in section 4.4: p. 12, ln. 25-29).

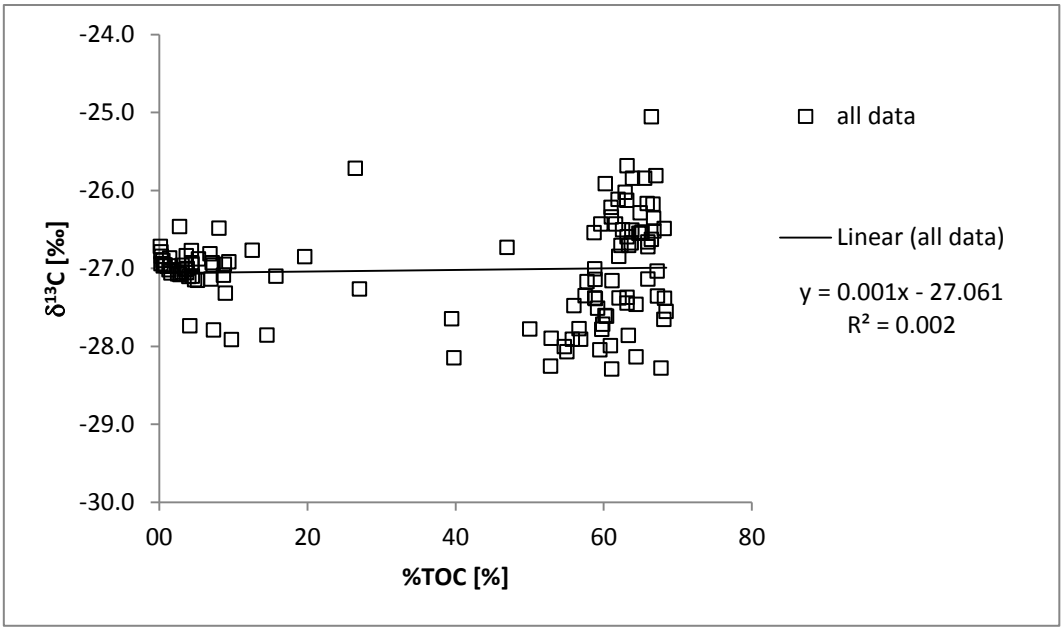
I also suggest following Carlos Jaramillo's comments on the raw palynological counts and the TOC vs d13C TOC correlation plot.

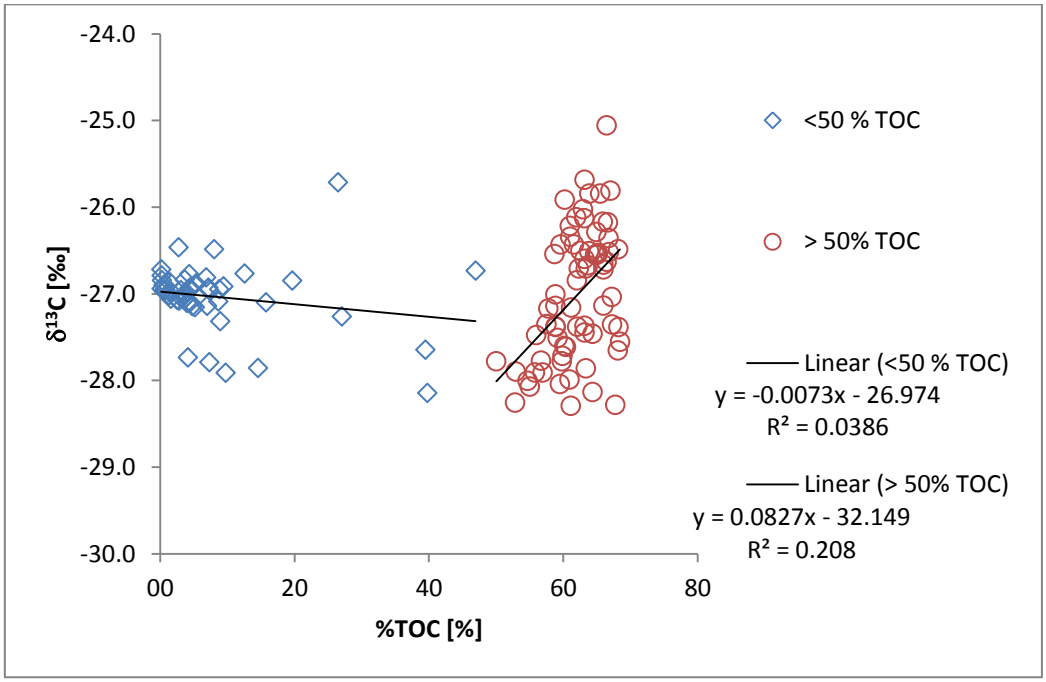
- Concerning the raw palynological counts, we appreciate the valid request and now provide the raw palynological counts in the Appendix (section SI3, table SI3).
- Concerning the %TOC vs $\delta^{13}C_{TOC}$ correlation plot, we gave a detailed response in the comment to Carlos Jaramillo (see copy below):

- We appreciate this comment as this method is a valid approach for carbon isotope ratios of soil TOC and %TOC (Wing et al., 2005) and very interesting to apply this to our dataset.
- In brief, we find that a **direct assignment of this approach is not possible due to the very different nature of the two settings** (lignite seams/marine sediments and mud-rock paleosols). We deduced the %TOC vs. $\delta^{13}C_{TOC}$ relationships in our data set and found very different results to Wing et al. (2005). In particular, we found no relationship across the whole data set (R^2 = 0.002), a moderate positive linear relationship for samples with %TOC >50% (lignite seams) (R^2 = 0.208), and again no relationship for samples with %TOC <50% (marine interbed) (R^2 = 0.039). However, if applying these "transfer functions" (for details see below), we **maintain the deduced negative carbon isotope excursion**.
- In summary, we feel that exploring on such a relationship in lignite samples and/or modern peat samples would go beyond the scope of this paper, but consider this an interesting approach that needs further investigation.

In detail:

- Wing et al. (2005) found a strong negative exponential relationship between %TOC (independent variable) and $\delta^{13}C_{TOC}$ in mud-rock paleosol samples in the sub-groups of PETM (R^2 = 0.623) and Paleocene-Eocene (R^2 = 0.618) samples. For comparison, modern soil samples show a coefficient of determination of 0.952.
- Plotting our data (with the %TOC as the independent variable and $\delta^{13}C_{org}$ as the dependent variable), **no such relationship could be determined** (see plot below). The coefficient of determination R^2 is 0.002 and a F-test revealed no statistical relationship between these two variables.
- Only considering lignite values with %TOC > 50% yields a moderate positive linear relationship (R^2 = 0.208) that is statistically significant. Considering values of %TOC < 50% (marine interbed, typical %TOC <10%), we found a weak negative linear relationship (R^2 = 0.039) that is statistically not significant (F-test).
- Applying the deduced regressions (as transfer functions) -*even though the second regression is not significant*- to our data set in order to plot the residuals from expected $\delta^{13}C_{TOC}$ values, **maintains the deduced negative carbon isotope excursion**. The magnitude is slightly reduced at the CIE onset with -1.22 ‰ (compared to -1.66‰) and CIEs of -1.01‰ (-1.27 ±0.29 ‰, "mean-mean") or -1.37‰ (-1.74 ±0.46 ‰, "mean-most negative value").





Minor comments:

Page 1 Lines 6-9: The release of isotopically light carbon was most likely a feedback of the long-term warming rather than the cause.

- Agreed. We acknowledge that this phrasing is misleading and has been changed accordingly. In addition to this, the major revisions of the manuscript required major rewriting of the abstract.

Rephrase. Line 21-23: another problem associated with the interpretation of sources and mechanisms is local signals in different records

- We followed the suggestion of the reviewer and rephrased this sentence (see also the reply to a similar comment by Gerald Dickens).

Page 2 line 9: A. augustum

- We followed the suggestion of the reviewer and inserted “augustum” (p. 3, ln. 23).

Page 5: line30 to Page 6 line 16: this part should be rewritten with more care, in the view of the main comment about the definition of this interval as the PETM

- We appreciate this valid request. As stated above, we rewrote the manuscript in order to 1) describe the detected CIE, 2) give hypothesis about the interpretation of the CIE and 3) make a more tentative comparison of the existing lignite records (see reply above).

Page 6 line 12: add a reference here for the PETM duration

- Due to the major changes in this section of the manuscript, the questionable paragraph has been deleted.