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Interactive comment on "Palaeoenvironmental response of mid-latitudinal wetlands to PETM climate change (Schöningen lignite deposits, Germany)" *by* Katharina Methner et al.

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Apologies, but things got scrambled in .pdf conversion. I now also see that Carlos Jaramillo gave commentary. His main critical point is the same as mine: basically, how do you know you are looking at PETM? Recent papers by Slotnick et al. (NZJGG, 2015), Laurentano et al. (COP, 2015), Luciani et al. (COP, 2016) and Westerhold et al. (Paleo. Paleo. 2018) really highlight this issue. The MS should very much be rewritten to address this, although as noted, a very good paper can emerge irrespective.

Jerry

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– Page 1 – Lines 8-9: There remains zero evidence that the massive carbon input caused the warming during the PETM. Rather, all evidence suggests a coupled system where warming and carbon input are linked in positive feedback. Rewrite, especially as it ultimately relates to the MS content. – Page 2 – Line 17: Change “shelfs” to “continental slopes”. Phase boundary constraints do not allow gas hydrate to occur on continental shelves. Lines 20-21. This should be reworded. As highlighted by Sluijs and Dickens (2012), there is a major difference between the local expression of the global CIE across the PETM, and what occurred to exogenic carbon cycle (where mass balance comes into discussion).

Line 27: Importantly note that coastal wetlands are generally a location of transient carbon storage on the time scale relevant to the PETM. Carbon is generally not buried in these environments on the >10,000 year time scale, especially when considering changes in sea level, although there are obviously some good examples (aka the present-work). The current writing sort of recognizes this – it’s the word “sink” that is problematic. – Page 3– **Lines 3-16: It should be noted that spikes in Apectodinium abundance occur during other hyperthermals of the early Paleogene. The key to the PETM is that there is a special marker species (A. augustum) that seems to have existed only during the PETM. Without documenting this dinocyst, the stratigraphic record shown could be one of the proximal hyperthermals (e.g., HI/ETM2).

– Page 5– **Lines 30-31: As above, it has not been demonstrated that this horizon is, in fact, the PETM. Importantly, though, at one level it does not detract from the MS significance, because there is a growing view that the main hyperthermals are related and have similar systemtic repsonses.

If it cannot be conclusively proven that the interval is the PETM, this should be stated. There should then be some key rewriting and an additional paragraph that notes that much is written with an assumption, but most of the basics would also apply if the interval was instead another lesser but major early Paleogene hyperthermal.

Starting about Line 18, the reading gets awkward, because not always clear what constitutes a paragraph. – Page 6– Lines 15-16: Note that the good and intriguing ideas presented by Trampush and Hajek do not really apply to most deep-sea records, from which the duration has been estimated.

**Line 24+: This is interesting, but all hinges on the correct stratigraphy, something that has to be bolstered better.

– Page 11 – Line 2: Okay, except then, the prominent CIE was not caused by the burning of peat. Overall, this is important and relates to the initial comment above – the CIE and associated massive carbon input was very likely mostly/partly a response to major changes in Earth systems not the primary driver.

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