

Interactive comment on “Constraints on ocean circulation at the Paleocene–Eocene Thermal Maximum from neodymium isotopes” by A. N. Abbott et al.

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This manuscript is interesting. I leave detailed commentary to the formal referees.

However, should the manuscript be published, I strongly encourage the authors to rewrite the Introduction. It currently does not give a good and sensible view of the PETM in which to frame their work.

– Page 2558: Lines 21–26 – The opening sentence perpetuates two misconceptions that permeate the literature and, as such, this sentence should be changed. First, it is by no means clear that the PETM was the warmest time of the Cenozoic. This is because intervals of EECO (~3 to 6 Myr after the PETM) may have been warmer. Ba-

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sically, the PETM occurred during a time of warming heading into EECO, and the time slice of true maximum global warmth is not certain. Second, there is ZERO evidence that the warming was the response to a rapid release of ^{13}C -depleted carbon. This is because all evidence presented to date suggests the opposite: the carbon release, at least that manifested by the $\delta^{13}\text{C}$ excursion, responded to the warming. In fact, even the present submission suggests this possibility in the next paragraph and at the end. Carbon input associated with warming and a feedback to such warming – yes; carbon input causing the warming – no.

– Page 2558/9: Lines 26-5 – This sentence makes little sense as written. Neither volcanic emissions nor a change in ocean circulation can directly explain the global negative CIE across the PETM for reasons of mass balance and carbonate chemistry, as known for over 20 years. Both these possibilities may be the trigger for another carbon source, such as seafloor methane, peat or permafrost, but they cannot be the root cause of the event, at least without clear elaboration. Things become more problematic, at least in terms of writing, because it is only in the next paragraph that part of reason for invoking a massive input of carbon during the PETM arises (i.e., the prominent $\delta^{13}\text{C}$ excursion). The other key part of the rationale, which is missing, is that the PETM is marked by prominent, though importantly variable, carbonate dissolution on the seafloor of all ocean basins.

– Page 2558/9: Lines 28-4 – Lines 28-3: As above, this sentence also makes little sense. This is because volcanic emissions per se do not provide a “highly fractionated carbon source”, and because only one of the references (Kurtz et al., 2003) is applicable to concepts presented.

Good and bad introductions to the PETM have been presented numerous times over the last 20 years. I suppose I am now on a crusade to make sure mostly good introductions appear. Basically, the authors should set the key aspects of the PETM correctly, and present their findings, which are interesting, within this framework. They will see that such a change actually enhances and eases the discussion.

Sincerely,
Jerry Dickens

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