

## ***Interactive comment on “Southern Ocean warming and hydrological change during the Paleocene–Eocene thermal maximum” by A. Sluijs et al.***

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Preface: Appy and I are very good friends. We also like to argue about science. Moreover, we both see potential strengths in the open review format, in large part because it can be educational and because it can move good science forward faster if various aspects of a problem are discussed openly and critically. Appy has asked me to review his paper objectively; the following should be taken in this context.

Overview: The Paleocene-Eocene thermal maximum (PETM) has become a magnet for studies of past climates because the interval was characterized by a geologically fast and massive input of  $^{13}\text{C}$ -depleted carbon, and because it is marked by pro-

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nounced environmental change, including a major rise in temperature. It is arguably our best past time interval for understanding how Earth systems respond to (relatively rapid) global warming and massive carbon input (although it should be recognized that any analogy to future climate change comes with several caveats).

The manuscript provides new data across the PETM recovered at Ocean Drilling Program (ODP) Site 1172. Although never really emphasized in the manuscript, at least in my opinion, an overarching problem with understanding the PETM is that records at different sites give different signals. Some of this is interpretation, and the nature of working with proxies extracted from the rock record; some of this (perhaps most) is regional, and the fact that various places on Earth will respond differently to global warming and massive carbon input. In any case, many records from numerous locations are needed to understand the cause of the PETM and, more importantly, the range of environmental responses. In this sense, the new records from Site 1172 are welcome. The site lies in a region that has been relatively unstudied for Paleogene climate change.

However, I have several criticisms and comments on the current manuscript. In my opinion, it needs major work before publication.

Gerald (Jerry) Dickens Stockholm University (and Rice University)

\*\*\*\* Major and General Comments (in no particular order):

(1) The data needs to be tabulated. Such tabulation also needs to come with proper IODP labeling (core, section, interval, depth) as well as related information in the paper (rmbfsf). This can be placed as supplementary information.

(2) The idea that TEX-86 (or various derivatives thereof) records mean annual temperature (MAT) at high-latitudes has always seemed problematic to me because of major seasonal changes in light, temperature and productivity. Basically, what are the crenarchaea eating in the winter? I am open to the possibility that they do, in fact, record MAT

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at high latitudes, but it sure would be helpful to have a mechanistic rationale for why this is the case, as well as supporting data. The recent Kim et al. paper (GCA, 2010) is enlightening but leaves an unresolved problem. They show, at least in my opinion, that TEX-86 does NOT accurately record temperatures at high latitudes ( $>60^\circ$ ). The open issue is whether this is because of cold water, because of high latitude (for example, changing food supply), or some combination of both (or other factors). This is obviously problematic for studies of the Early Paleogene, where surface water at high latitudes was much warmer than present-day (Comment 3). Does one use a TEX-86-temperature calibration for high latitudes, warm temperatures, or something different? Comparisons of foraminifera oxygen isotope and TEX-86 records at low-latitudes are not helpful here (contrary to what is implied in the writing, e.g. Page 1712, Lines 10-12). Until this is resolved, I do not think it appropriate to convert TEX-86 to MAT at high latitudes (let alone comment on climate and modeling from this regard); the a priori assumption for TEX-86 values at high latitudes should be that they are skewed toward summer temperatures. Certainly, modeling of the early Paleogene world would become easier if the high latitude temperature records were, in fact, summer temperatures.

The present submission (and several recently published papers) has largely omitted this crucial issue, using various correlations without discussion; any presentation of TEX-86 derived temperatures from high latitudes should come with major caveats. (It should be stressed here, though, that while absolute temperatures before, during and after the PETM may have large uncertainties, the relative change in temperature could be very accurate).

(3) Related to the above, there is a basic constraint on high-latitude southern hemisphere water temperature before and after the PETM (and sort of during the event, although this is complicated because of “missing” tests): the benthic foraminifera oxygen isotope record. This record suggests an intermediate water temperature of nominally  $\sim 11^\circ\text{C}$  before and after the PETM. Presumably, as in the modern world, this largely represents the temperature of water where it sank, which from various records,

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appears to be the southern ocean. So how can benthic foraminifera records suggest 11°C for surface waters when the TEX-86 suggest 26°C?

There are several possibilities when one considers seasonality, latitude, etc. The problem with the present manuscript is that this important point is missing.

(4) The timing of carbon input during the PETM is, in my opinion, a wonderful and open issue. The dogma is that carbon input occurred within 10,000 years; however, recent records have emphasized that some of it (maybe 50%?) came out much slower (e.g., Zeebe et al., *Nature Geoscience*, 2009; Murphy et al., *GCA*, 2010; Nicolo et al., *Paleoceanography*, in press).

It is certainly possible that the entire carbon input was indeed very fast, and that contrary suggestions are incorrect because of poor interpretation. The problem with the present paper is that the traditional view is assumed, and they do not show (or discuss) the details of their records with this issue in mind (and without the tabulation of data, nobody can expand the records and examine within the context of other information, Comment 1).

(5) The current text, map and references give the impression that this is the first study of a margin sequence in the region, except for palynological investigations at Tawanui, North Island, New Zealand. However, there are numerous records at Tawanui, including some geochemical records comparable to those in the present submission (Kaiho et al., *Paleoceanography*, 1996; Crouch et al., *P3*, 2003). Upper Paleogene slope sequences now uplifted and exposed in Clarence Valley, South Island, New Zealand (e.g., Mead Stream and Dee Stream) also contain expanded intervals of the PETM (as well as other Paleogene hyperthermals) with important information pertaining to the hydrological cycle (Hancock et al., *NZJGG*, 2003; Hollis et al., *P3*, 2005; Nicolo et al., *Geology*, 2007, *Paleoceanography*, in press).

These papers are problematic in two regards. First, the present manuscript is certainly not the first examination of temperature and environmental changes across the PETM

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at a margin location at high southern latitudes. Second, and more crucially, interpretations from the New Zealand sites conflict with those in the present study (Comment 6). This needs to be addressed.

(6) The PETM is a clay-rich horizon at Tawanui, Mead Stream, Dee Stream and Muzzle Stream (Crouch et al., 2003; Hancock et al., 2003; Hollis et al., 2005, 2006; Nicolo et al., 2007, in press). Like “early Paleogene continental margin sequences” in other locations, such as Spain, Italy, New Jersey, and California (as referenced in Giusberti et al., Bull. Geol. Soc. Am., 2007; John et al., Paleocyanography, 2008), the clay-rich horizons are caused by elevated accumulation of terrigenous material. This has been interpreted as representing a world where the hydrological cycle accelerates and precipitation becomes more seasonal (i.e., more rain but in a shorter time; Schmitz and Pujalte, 2007, Nicolo et al., 2007, in press; John et al., 2008).

The problem here is that this sort of contrasts with the interpretation in the present paper, although this is not entirely clear.

(7) The stratigraphy should be presented as a figure (new Figure 2). This should have the paleomagnetic constraints and lithology, as well as the core photos for the interval of interest. It may be appropriate to have the long record in one panel, and the shorter interval of interest in an adjacent “expanded” panel.

(8) It should be noted that the  $\delta^{13}\text{C}_{\text{TOC}}$  record at Tawanui also shows a relatively small excursion (<2 per mil) across the PETM. It is possible that the base of the excursion is missing at Tawanui. However, it is also plausible that, given other records at Tawanui (e.g., C/N ratio), this represents changing proportions of terrigenous and marine organic material (Crouch et al., 2003). Interestingly, the comparison to the proximal record at Tawanui is missing, and this possibility is not discussed.

(9) The Ca and Fe records are not discussed very thoroughly. In particular, during the PETM, the Fe goes up and the Ca goes down, but these changes are not in tandem as expected for a terrigenous/carbonate system. So, what are each of these actually

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recording?

Other comments

– 1702 – Line 5: Change to “. . .<sup>13</sup>C depleted carbon, reflected. . .”

Line 23: Bijl et al. did not show global warming.

Line 26: Do not use “i.e.” in the middle of sentences.

– 1703 – Lines 1-5. This is poor referencing and not necessary in any case.

Line 15: Remove “traditional”. (Note that there are more Tex-86 records across the PETM than Mg/Ca records).

Lines 10-11: What records? Note also that the d<sup>13</sup>C excursion alone is insufficient to suggest massive carbon injection.

Lines 27-29: What sections? The multiple use of “these sections” is confusing.

– Page 1704 –

Lines 11-14: I’m not sure if you want to cite the Higgins and Schrag (2006) paper, as it unnecessarily muddles things. Like the suggestion of a cometary impact (Kent et al., EPSL 2003), attributing the carbon isotope excursion to sudden exposure and oxidation of marine organic carbon in epeiric seas has never made sense. This is because the PETM in known epeiric seas (for example in Asia) are marked by a rise in water level and excess organic accumulation (e.g., Bolle et al., 2000; Iakovleva et al., 2003).

Line 17: This is confusing as written. Was the excursion documented in other post-cruise studies or here? If the former, this needs a reference; if not, it should be rewritten.

– Page 1706 –

Line 17: Adapt Sluijs? Rewrite.

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– Page 1707 –

Lines 15-20: The “mbsf” should stay the same; that is, don’t “change” the mbsf. However, it is okay to offer revised depths below the seafloor, as long as it is clear in a table where the sample comes from.

– Page 1710 – Lines 15-16: This sentence is largely unnecessary and can be merged with the following one.

Lines 22-25: This sentence is awkward; rewrite.

– Page 1711 – Lines 6-7: Note that the d13CTOC excursion at Tawanui (New Zealand) is also small.

– Page 1712 – Line 25: Change placement of “also”.

– Page 1713 – Lines 14-16: Yes, I think that there is a strong possibility that the proxies or recording different temperatures (see comment, 2).

– Page 1714 – Line 9: The word “dominates” is not entirely correct. As I recall, Apectodinium species do not exceed 20% of the PETM dinocyst assemblage in the Arctic.

– Page 1715 – Lines 16-19: This is confusing as written. When I read the John et al. paper, they suggest terrigenous dilution not carbonate dissolution. The Bolle et al paper does suggest carbonate dissolution for the margin sections in Asia; I suspect that this idea may need revisiting given the findings of more recent studies. In any case, there is little evidence (or rationale) for an extreme rise in the CCD within the context of existing studies; in other words, the finding that the CCD does not rise onto the shelf is not unexpected but consistent with abundant other work.

– Page 1716 –

Line 7: Misspelling.

Lines 16-24: I do not follow this.

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Line 21: Change “where” to “were”

– Figure 1 –

Following from Comments 5 and 6, I would add the locations of the Tawanui and Clarence Valley sections. Because ODP 690 is also discussed, add this location, too.

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Interactive comment on Clim. Past Discuss., 6, 1701, 2010.

CPD

6, C782–C789, 2010

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