Clim. Past Discuss., 5, C962–C965, 2009
www.clim-past-discuss.net/5/C962/2009/

Climate of the Past Discussions

CPD
5, C962–C965, 2009

Interactive Comment

Interactive comment on "Productivity feedback did not terminate the Paleocene-Eocene Thermal Maximum (PETM)" *by* A. Torfstein et al.

A. Sluijs (Referee)

a.sluijs@uu.nl

Received and published: 1 December 2009

Torfstein et al compile published data and age models from the PETM at Site 690 to conclude that the termination of the PETM was not caused by a productivity feedback. Bains et al., 2000 proposed this idea based on an increase in Barium concentrations in the sediments concomitant with the event. Later contributions changed the age model (Farley and Elrgroth, 2003; Rohl et al., 2007). Particularly, this work recognized the carbonate dissolution horizon at the onset of the PETM. This resulted in an increase in the concentration of all other sedimentary components, including, as the authors indicate, Barium. Several authors have argued against the Bains hypothesis and I'm not sure how many still believe in Ba-MARs as a good indicator for productivity in the case of the PETM. There are all kinds of problems, including those mentioned by the authors, but there are even more. I guess nobody has specifically made the point the authors indicate before. As far as I can see, this seems a solid point made in this paper.

It remains unclear, however, how the authors arrive at their conclusion that productivity did not play a role during the recovery. The only thing the authors show is that the newer age models imply that it cannot be argued based on Ba-MAR that the export of organic matter to the sea floor increased during the PETM at Site 690. This observation does not likely have implications for the global ocean. Many marginal marine sediment records have increased organic carbon content in the PETM interval (e.g., Speijer and Wagner, 2002 GSA spec paper; Bolle et al., 2000; see overview in John et al., 2008). In fact, Sluijs et al., 2008a (Paleoceanography) and John et al., 2008 (Paleoceanography) have shown that organic carbon (and also even carbonate) burial increased significantly in most studied shelf areas during the PETM. At least at a couple of these sites, paleontological and geochemical data have suggested an increase in regional productivity (e.g., Speijer and Wagner, 2002 GSA spec paper; Sluijs et al., 2006, Nature; Gibbs et al., 2006, Geology; Sluijs et al., 2008a, Paleoceanography; Sluijs and Brinkhuis, 2009 BG). Hence, this data would argue that part of the carbon was indeed sequestered as organic carbon, primarily in marginal marine environments, partially caused by an increase in regional productivity resulting from a change in the trophic resource continuum, causing increased export of organic matter, dysoxia and organic carbon burial. This published work is, hence, in direct conflict with the conclusion of this paper and thus good arguments against this hypothesis need to be made if the authors want to stick with their main conclusion.

Specific comments Page 2399 is rather confusing. It starts with the observation that multiple PETM-like events occurred in the Eocene, potentially triggered by the same carbon injection mechanism. In line 9, this observation suggests to the authors that ocean circulation was the most likely control on the recovery process. I don't understand this connection. Subsequently, it is suggested that ocean circulation triggered carbon release, thereby apparently contradicting the previous point. In line 20, it is argued that silicate weathering only started after 70 kyrs. Also this hypothesis is inconsistent with observations from marginal marine sequences, which show a vast increase in clay fluxes at the onset of the PETM (e.g., Gibson papers; Cramer et al., 1999; Sluijs et al., 2007, Nature, Sluijs et al., 2008, Paleoceanography, John et al., 2008 Paleoceanography, Sluijs and Brinkhuis, 2009 BG).

Technical and specific comments chronologically with the MS

2392 20: Kennett and Stott 1991 did not mention carbon input as a cause of the carbon isotope excursion. I think the first to do that were Dickens et al., 1995 23-24: Tripati and Elderfield, 2005 did not report on sea surface temperatures, only deep water temperatures

2393 5: See Schouten et al., 2007 EPSL for a compilation of magnitudes of the CIE measured on various substrates. The variation is much larger than 2-3 ‰ 10: circulation change as a cause was first proposed by Dickens et al., 1995 and Thomas & Shackleton, 1996

2394 7: I'm not sure if Dickens et al., 2003 should be in this list. If anyone has argued against the barite argument, it is Jerry Dickens. 17: also on the preservation of barite. Dickens' argument is that much of the deep ocean is undersaturated with respect to Ba. Importantly, but hardly discussed in the PETM literature is that barite preservation is probably dependent on carbonate ion concentration, which is obviously critical with respect to the PETM (as far as I know it is only mentioned it in our 2007 PETM review paper; Sluijs et al., 2007 in: Williams et al (Eds) Geol. Soc., London).

2395 Our review paper (Sluijs et al., 2007 in: Williams et al (Eds) Geol. Soc., London) also included a critical review and recalculation of the Farley and Eltgroth age model, which is I think of relevance to the present paper.

2396 15 and further: Dissolution is here proposed to have caused the discrepancy between the two age models for the recovery period. This is strange because the

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

dissolution primarily occurred during the onset of the PETM. Also, carbonate fluxes in this interval go up in both age models indicating an increase in preservation. In fact, in their abstract Kelly et al., 2005 (Paleoceanography; cited by the authors) state the exact opposite to the statement made by the authors: the CIE recovery period, which postdates the CIE onset by 80 kyr, is represented by an expanded (2.5 m thick) interval containing a unique planktic foraminiferal assemblage strongly diluted by coccolithophore carbonate. Collectively, the micropaleontological and sedimentological changes preserved within the CIE recovery interval reflect a transient state when ocean-atmosphere chemistry fostered prolific coccolithophore blooms that suppressed the local lysocline to relatively deeper depths. A prominent peak in the abundance of the clay mineral kaolinite is associated with the CIE recovery interval, indicating that continental weathering/runoff intensified at this time as well (Robert and Kennett, 1994). Such parallel stratigraphic changes are generally consonant with the hypothesis that enhanced continental weathering/runoff and carbonate precipitation helped sequester carbon during the PETM recovery period.

Figures 1: derivation of the map needs the appropriate reference

Interactive comment on Clim. Past Discuss., 5, 2391, 2009.