March 13, 2010

Reviewer #1 – A. Sluijs

We thank Dr. A. Sluijs for his comprehensive and thorough review. We start with replying to general comments made by the reviewer, or those that are relevant to more than one of his comments. Thereafter, we refer to the specific comments made by the reviewer. References to the comments of the reviewer are marked in *Italic* font. Text from the manuscript is given in font size 10.

The reviewer states that: *"... 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.*".

Given the fact that the Ba-MAR record presented by Bains et al. (2000, *Nature*) is the main line of evidence that is repeatedly used to argue for a productivity feedback process during the PETM, we think this is a highly relevant observation. As the reviewer points out and as we discuss in the manuscript, the debate regarding the mechanisms of recovery from the PETM event is ongoing and unresolved. Essentially, the two most likely processes involving CO_2 drawdown are a productivity feedback or an increase in silicate weathering, with arguments for and against each of them in a large number of publications. Since the Ba-MAR record at ODP site 690 is a major line of evidence supporting the productivity feedback hypothesis, and is (still) widely cited as such, our contribution presents a significant step towards resolving this discrepancy.

The reviewer continues to note that: "*This observation does not likely have implications for the global ocean.*". We agree with the reviewer that observations from a single site (in this case, from the Southern Ocean) are limited in their global implications and conclusions. At the same time, we submit that site ODP 690 represents a 'type section' analogue for PETM studies and a considerable amount of the knowledge about the PETM is based on this site. We have revised the manuscript to clarify this point. Specifically, we have added a paragraph to section 3 'Barium MARs and discrepancies regarding biosphere feedback'.

The reviewer adds that: "*Many marginal marine sediment records have increased organic carbon content in the PETM interval*". While we accept this comment we note that even in these cases (i.e., marginal marine environments), which are most likely more sensitive to local-regional conditions, the evidence is ambiguous and identified in some sites (e.g., John et al., 2008), while not in others (e.g., Bolle et al., 2000).

The reviewer comments that: "*... 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*". In the revised version we rephrase the text, explicitly stating that our conclusions are derived from a site in the open ocean environment, and point out that it would be desirable to confirm these results by performing similar studies in other open ocean sites, thereby establishing a reliable global framework for the PETM event.

Specific comments

Page 2399 + page 2396 (15 and further) (we provide a combined reply to both these comments):

The reviewer was confused by the connection we suggest between the occurrence of multiple PETM-like events over several millions of years and the possibility that these were triggered and thereafter terminated by changes in ocean circulation. Following the reviewer's advice, we removed in the revised version most of the speculative discussion focusing on changes in ocean circulation as drivers of the onset and recovery from the PETM.

One of the outcomes of our work is that there was no noticeable increase in Ba-MARs and hence in primary productivity in the Southern Ocean for at least 70 ka after the onset of the PETM. The implication of this finding is that silicate weathering was the dominant process driving down CO_2 levels.

As such, it might be expected that the shift into the recovery phase would be a long and gradual one, corresponding to the nature of silicate weathering processes, which are controlled by the interplay between tectonics, hydrology and erosion rates. Yet, it is intriguing that the record at ODP site 690 and other locations indicates an abrupt, almost discrete, transition between these two patterns of evolution. The driving processes for

this change remain unclear and speculative; previous studies suggested that the above environmental shift between the warming and recovery phase is related to a shift in the directions of ocean circulation, which enabled CO_2 uptake in the ocean and subsequent reduction of atmospheric CO_2 levels (Bice and Marotzke, 2002; Tripati and Elderfield, 2004, 2005; Nunes and Norris, 2006). We mention this possibility in our section 'Conclusions and Implications' (in the revised version: page 9, lines 236-239).

The "*…vast increase in clay fluxes at the onset of the PETM*", supports, according to the reviewer, major silicate weathering processes, which took place at the onset of the event, rather than much later, as suggested in the manuscript. We would like to comment that these clays reflect, for the most part, the dissolution of carbonate due to ocean acidification (e.g., Zachos et al., 2005). This issue is discussed in the text (page 2396, lines 16-21) and is also mentioned by the reviewer himself in a later part of his review *("…the dissolution primarily occurred during the onset of the PETM*").

The reviewer also refers to the work of Kelly et al. (2005) who discussed weathering/runoff and surface ocean carbonate production during the recovery stage of the PETM. These authors conclude that the recovery post-dates the onset of the event by ~80 ka (corresponding to our assessment of ~70 ka for this transition). At this time, the sediment facies shift from a dominance of clays to a dominance of carbonates. Furthermore, Kelly et al.'s reconstruction of kaolinite abundances (Fig. 4E in their publication, based on Robert and Kennett, 1994) shows clearly that the increase in kaolinite occurs only after the start of the recovery phase.

We emphasize that we do not suggest what the exact time of the start of the silicate weathering process was; what we mean to convey is that the collective observations indicate that while the recovery phase continued for a relatively long time period, the transition from a 'warming' world to a 'cooling' world, was most probably a very short one.

Technical and specific comments chronologically with the MS

2392 20: Kennett & Stott (1991) mentioned the possibility of organic carbon transfer from the continents to the oceans as a cause of the CIE, but did not favor it and did not

develop the idea. We thank the reviewer for pointing this out to us and modified the text accordingly.
The reference of Tripati and Elderfield (2005) was removed in accordance with the reviewer's comment.

2393 5: ok, corrected.

2393 10: corrected.

2394 7: The reference to Dickens et al. (2003) was removed.

2394 17: "*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).*"
We were not previously aware of this book chapter, which is clearly of great relevance to our work, and refer to it and its findings in the revised version. The reviewer's comment emphasizes the point made in our manuscript: the main line of evidence supporting the productivity feedback hypothesis, i.e., the rise in atmospheric CO2 and synchronous rise in Ba-MAR, as suggested by Bains et al. (2000), is an artifact of the age model available at the time. Ba-MARs do not change throughout the larger part of the PETM, if at all. Hence, while the discussion involving the degree of preservation of primary productivity patterns by the Ba-MAR is an important one, it is not of primary importance or the current discussion. As the reviewer mentions in his comments and in previous publications (e.g., Sluijs et al., 2007), and as we detail in the manuscript, the debate surrounding the productivity feedback hypothesis versus a silicate weathering mechanism, is ongoing and for the most part unresolved (pages 2397/8, line 24 and further: "… This conclusion sheds new light on the ongoing debate over the response of the marine biosphere to carbon release at the PETM and resolves some discrepancies between different studies. …Yet these lines of evidence are ambiguous. For example, increased productivity during the CIE was suggested on the basis of a limited increase of Sr/Ca ratios in some (but not all) calcareous nannofossil assemblages

(Stoll and Bains, 2003; Stoll et al., 2007). … Clearly, the revised Ba-MARs records presented here warrant re-consideration of some of the ideas detailed above.“).

Thus, the present manuscript is an important contribution towards resolving the above discrepancies. Furthermore, it reveals that a large part of the debate involving the Ba-MAR record during the PETM (primarily based on results from ODP site 690) is irrelevant because it was based on the out-dated Ba-MAR record of Bains et al. (2000). Hence, the current work also warrants a future revision of the characteristics of the global marine Ba cycle during the PETM.

2395: we thank the reviewer for this relevant comment and the reference to Sluijs et al. (2007) has been added to the revised manuscript.

2396 15 and further: see our reply to the first specific comment.

Figure 1: we added the appropriate reference to Hay et al. (1999)