Clim. Past Discuss.,
https://doi.org/10.5194/cp-2017-76-RC2, 2017

Climate of the Past Discussions

Interactive comment on "Tropical Atlantic Climate and Ecosystem Regime Shifts during the Paleocene-Eocene Thermal Maximum" *by* Joost Frieling et al.

Anonymous Referee #2

Received and published: 28 July 2017

Reviewer 2 Comment on Frieling et al. 2017. Tropical Atlantic Climate and Ecosystem Regime Shifts during the Paleocene-Eocene Thermal Maximum. Clim. Past Discuss., https://doi.org/10.5194/cp-2017-76

This contribution provides detailed new dinoflagellate assemblage data through the PETM from the western tropical Atlantic, supported by additional new bulk sediment chemistry and magnetic susceptibility measurements. Overall it is clearly presented, well written and a solid contribution to the dataset of surface ecosystem responses to the PETM warming event. I do have a couple of concerns and some minor comments that need to be addressed by the authors before publication.

Printer-friendly version

Discussion paper

1. One of the key points made within this paper is that, to quote from the abstract: "The combined paleoenvironmental information from Site 959 and a close by shelf site in Nigeria implies the general absence of eukaryotic surface-dwelling microplankton during peak PETM warmth is most likely caused by heat stress."

My concern is that evidence presented from selected sites is framed to make inferences about global responses and environmental drivers: "Site 959 and a close by shelf site in Nigeria.... implies the general absence of..." . Within a few words they've gone from local and specific ("close by") to a "general absence".

This is a problem because this group of authors are foremost in the analysis of PETM dinoflagellete (and other) records. The quality of their regular outputs, and regard within the community, gives them a very strong influence on shaping the accepted narrative and interpretation of data. With this is mind, I think they have to be exceptionally careful about the claims that are made and that these fully take into account uncertainty in going from the observed data to interpretation.

In this case they may well be correct and are presenting a substantive account of the true ecosystem responses, but my concern is that only references that support this "heat stress" and tropical exclusion of eukaryotes are cited – self-citations and Aze et al. (2014) and Yamaguchi & Norris (2015). A stronger case would include a wider overview and consideration of tropical sites where there is less or no evidence for the exclusion of eukaryotes. For example, the Tanzanian section discussed by Aze et al. (2014) also has records of coccolithophore communities and calcareous dinoflagellates throughout the PETM – calcareous dinoflagellates are actually shown to increase in abundance during the PETM (Bown and Pearson, 2009). Similar records of persistence of coccolithophore communities and increase in calc. dinos are shown from the tropical Pacific, ODP Site 1209 (Gibbs et al., 2006b). In Site ODP 1209 there is an increase in phytoplankton turnover (Gibbs et al., 2006a), which may be related to heat stress, but there is little evidence for a total exclusion of eukaryotic microplankton from this tropical location. There may be reasons for this increase in calc. dinos. in both the Tanzanian and Pacific tropical sites, and this might support some of this groups' interpretations, but there needs to be some recognition that these other records exist and then an integration of data to form a more solid interpretation of the wider (/global) patterns of change.

In this instance, is there a case for any ecological exclusion of dinoflagellates be limited to the (eastern) equatorial Atlantic? I don't think there is strong evidence (yet) to extrapolate from these two relatively close sites (Nigeria and ODP 959) to a global response in the tropical oceans. Any associated sea surface temperature records from these locations might also just represent localized effects that aren't replicated in either the tropical Pacific or Indian Oceans.

2. The use and referencing of a submitted manuscript "Frieling et al. submitted" is frustrating. This was not provided to reviewers. Although I don't think the conclusions of this manuscript rely on what may be contained within this other submission, one feels that we're being asked to review this paper with 20% of the interpretation (and data?) hidden from view. Ideally, I would rather this manuscript was not published until either the "submitted" manuscript was published or made available for reviewers and editors of this submission. For example, key interpretation of the CIE, its onset and the temperature data are all likely contained in this other submission. I would recommend that the editor at least be able to see this other submitted manuscript in confidence prior to any final publication of this paper, so that they can judge the degree of overlap.

3. Related to the development of a narrative for PETM dinoflagellate records presented by this group over a number of years, I'm intrigued by the interpretation presented of changes in abundance of key indicator species that previously have been used to infer sea level change through the PETM in shelf sites (page 8).

"From 804.4 mbsf, we find an increase in abundance of dinocysts belonging to, or closely related to the genus Areoligera (Areoligera complex sensu Sluijs and Brinkhuis, 2009). A relative abundance increase of this genus was previously interpreted to reflect sea level rise at several shelf and slope sites during the PETM (Sluijs et al., 2008). However, Site 959 is located in an open ocean setting, which means water depth and shore proximity proportionally do not change as much as may be expected from sites on the continental shelf, especially if estimates of the amplitude of sea level rise across the onset of the PETM (5-20m, e.g., (Speijer and Morsi, 2002; Sluijs et al., 2008) are considered. The increase in Areoligera is further associated with a decrease in Spiniferites, consistent with other PETM records (e.g., Sluijs et al., 2008), including a recently published record from Nigeria (Frieling et al., 2017). Since we cannot distinguish between transported and local signals, we may either record a signal that is transported off the shelf, or a local signal that is similar to, but not related to sea level."

I find this a little odd. If the dinoflagellate records are so subject to transport across shelf to the slope and deep ocean, what use are they in reconstructing relative position, from the marginal to oceanic? Which I thought was a substantial component of dinoflagellate paleoenvironmental interpretations? The other option presented is that this assemblage change is: "similar to, but not related to sea level." This seems more likely than pervasive long distance transport. But if there is an alternate environmental cause of this assemblage change in the open ocean sites, then doesn't this also somewhat question whether the interpretation - of the same assemblage changes through the PETM from shelf-records - as being caused by sea-level is open to some reinterpretation? Could there rather be a broader dinoflagellate assemblage change (increase in Areoligera) that is rather related to the wider environmental changes in the tropical / sub-tropical Atlantic and less controlled by sea level? If there are such major PETM environmental changes in the tropics / sub-tropics, such as the heat stress the authors propose plus potential changes in stratification and nutrient supply, wouldn't these be more likely the drivers of dinoflagellate assemblage changes than a relatively modest change in sea level? If so, then this seems like an appropriate place to put the previous interpretations into this new context for the non-expert reader. Again, I'd emphasize, that when this group of authors dominate the generation of PETM dinoflagellate records and the interpretation of them, it's also their responsibility to the external readership to directly address such questions as new data and interpretations arise.

Other comments:

1. Given that Thomas Westerhold is a co-author, I'm surprised that there is no mention, use or citation of the latest age model assessment for the PETM: Westerhold et al. 2017. Clim. Past Discuss. https://doi.org/10.5194/cp-2017-74. And specifically the durations provided for the PETM in this paper appear to be at odds with Westerhold et al. 2017.

2. Use of capitalization for informal sub-epochs / sub-series: e.g. Page 2, line 3: "during the Late Paleocene and Early Eocene…". See Pearson et al. 2017. Episodes, http://dx.doi.org/10.18814/epiiugs/2017/v40i1/017002

Bown, P., and Pearson, P., 2009, Calcareous plankton evolution and the Paleocene/Eocene thermal maximum event: New evidence from Tanzania: Marine Micropaleontology, v. 71, no. 1-2, p. 60-70.

Gibbs, S., Bown, P., Sessa, J., Bralower, T., and Wilson, P., 2006a, Nannoplankton extinction and origination across the Paleocene-Eocene Thermal Maximum: Science, v. 314, no. 5806, p. 1770.

Gibbs, S., Bralower, T., Bown, P., Zachos, J., and Bybell, L., 2006b, Shelf and open-ocean calcareous phytoplankton assemblages across the Paleocene-Eocene Thermal Maximum: Implications for global productivity gradients: Geology, v. 34, no. 4, p. 233-236.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2017-76, 2017.