

Clim. Past Discuss., author comment AC1
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Reply on RC1

Peter K. Bijl et al.

Author comment on "Maastrichtian–Rupelian paleoclimates in the southwest Pacific – a critical re-evaluation of biomarker paleothermometry and dinoflagellate cyst paleoecology at Ocean Drilling Program Site 1172" by Peter K. Bijl et al., *Clim. Past Discuss.*, <https://doi.org/10.5194/cp-2021-18-AC1>, 2021

Reply to Reviewer 1 (Chris Hollis)

The effort the authors have made to compare the geochemical results with other approaches to temperature reconstruction are commendable. This should be standard practice in studies of this sort where so much uncertainty surrounds the absolute values generated by GDGT-based proxies.

Response to reviewer: We thank the review for his positive overall assessment of the paper

However, I find the introductory section on p. 4 poorly organised and a little misleading. It jumps from marine calcitic proxies to terrestrial pollen-based proxies and then back to marine TEX86. The problems that affect calcitic proxies are raised as a source of significant uncertainty but the much greater (in my view) uncertainties associated with applying a modern analogue approach to Eocene pollen grains is not mentioned at all. And no mention at all of brGDGT-based terrestrial approaches. I understand that this is covered in detail below, but some reorganisation is needed in these introductory paragraphs to set the scene for what follows.

Response to reviewer: We understand the point raised about the mismatch between introduction and discussion when it comes to fully appreciating the uncertainties of the absolute temperature reconstructions from existing proxy records.

Proposed adjustments to the paper: We will carefully review the introduction section on existing proxy records (Lines 86-102) to better reflect the uncertainties and improve the order of the proxies mentioned.

Along these lines, a greater issue arises when comparing the dinocyst ecogroups with SST estimates. No mention is made of the fact that two of the ecogroups have several taxa in common – open ocean and thermophilic (Table 3). So, more consideration needs to be given to the argument that an increase in open ocean taxa signals sea-level rise during warm events, notably the PETM.

Response to reviewer: This is a good point, and we will mention this in the discussion. Generally, we see no problem in this co-variance, but we do acknowledge that the uncertainty in distinguishing between open ocean and warm conditions is not immediately apparent to the reader and provide further clarification:

The most important open-ocean dinocysts (e.g., *Impagidinium* spp., *Operculodinium* spp., *Nematosphaeropsis* spp.) are very low in abundance, which supports our view that the depositional setting was on the continental shelf. In the absence of the few genera that can be considered indicative of open ocean conditions (Zonneveld et al., 2013; Prebble et al., 2013), the remaining dinocysts are largely those that are also commonly associated to warmer conditions (according to Frieling and Sluijs, 2018). This overlap shows that within the range of genera and data studied by Frieling & Sluijs (2018) 'warm' and 'open marine' conditions cannot be easily distinguished based on dinocyst assemblages. However, this is perhaps not unexpected, as a general co-variance of higher sea level and temperature on continental shelves in the ice-free Eocene is deemed likely. The risk of circular reasoning is countered by the extra evidence for deeper marine conditions in the middle Eocene, from the lithology (e.g., more CaCO₃; Rohl et al., 2004).

Proposed adjustments to the paper: We will add the discussion above to the discussion section 5.3.1

Also, I am surprised that there is no comment of the mismatch between the abundance of thermophilic taxa and SST. Fig. 15 shows there is a general correlation, but a very weak increase in thermophile during the EECO that is quite at odds with the major SST increase. The authors will know that a similar muted response is recorded at mid-Waipara, for both dinocysts and nannofossils (Crouch et al., 2019). This begs the oft-posed question, are the fossils recording an annual signal and TEX86 a summer one. This warrants some comment together with reference to the NZ record.

Response to reviewer: We agree with the reviewer that there is no perfect correlation between TEX86 and thermophilic dinocyst assemblages, and that this requires better explanation and embedding into existing data from other microfossil groups (e.g., the recent paper by Shepherd et al., 2021). We however argue that a multitude of explanations, seasonality being just one of them, could explain this mismatch. Another explanation could be a reduced response of dinocyst assemblages at these high temperatures, and as such a non-linear behaviour to the temperature forcing. There are simply no cold-water species left to respond under such warm conditions. This has been observed in other microfossil groups as well, e.g., during the EECO on Maud Rise (Site 690; Thomas and Shackleton, 1996). What remains unexplained, however, is that the onset of proliferation of endemism occurs at the end of the EECO, when SSTs are still very warm (see also Bijl et al., 2011). We further note that dinocyst assemblages are sensitive to more environmental factors than just temperature: salinity, nutrients, thermocline depth, and seasonality, many which tend to have strong gradients on an inshore-to-offshore transect.

Proposed adjustments to the paper: We will add the above discussion to section 5.3.1.

It is also worth noting that the thermophilic and open ocean dinocysts decrease rather abruptly at 50 Ma, whereas TEX86 decreases more gradually. The major rise in endemics directly above the peak in SST, to me suggests a greater influence of the Ross-gyre from ~50 Ma.

Response to reviewer: We agree this is an interesting observation.

Proposed adjustments to the paper: we will add some notes on the termination of the EECO, and the dinocyst response to the paper, in section 5.3.1 (thereby citing Bijl et al.,

2011; see also a note on this above).

In general, the text would benefit from a thorough edit to simplify sentence structures. Some of the references are cited out of context and others have been superseded by later work (Huber and Cabellero 200 vs Lunt et al. 2021; Huber et al. 2004 vs Sijp et al., 2016).

Response to reviewer: We disagree that because these papers are superseding one another, that they should therefore be replaced by the most recent one. Each of these papers use a different model, with different boundary conditions and functional feedbacks (e.g., ocean only versus fully coupled). More importantly, these simulations are quite sensitive to input parameters, and should therefore be considered as experiments, rather than full representation of reality, with the most recent one being the most accurate. Consistency in the outcome of these experiments strengthens the inferences from them, just as more proxy records strengthen inferences. This justifies citing all of them instead of just the most recent one.

Proposed adjustments to the paper: We will carefully reconsider the text to simplify sentence structures and reconsider referencing to properly reflect the current state of the literature, by including both older and newer references.

The figures are very informative but suffer from being too small with a too limited colour range and lacking guidelines to help match the text descriptions to the records. In some cases more explanation of methods and legends for symbols are needed (e.g. Figs. 15 and 16).

Response to reviewer: we will make sure figures adhere to the guidelines of the journal, and expand the captions for clarity.

Numerous additional comments and edits are provided in the annotated MS. Please also note the supplement to this comment:
<https://cp.copernicus.org/preprints/cp-2021-18/cp-2021-18-RC1-supplement.pdf>

Response to reviewer: Please find responses to the comments in the annotated text. We thank the reviewer for his thorough assessment, which allows us to improve our manuscript.

Please also note the supplement to this comment:
<https://cp.copernicus.org/preprints/cp-2021-18/cp-2021-18-AC1-supplement.pdf>