Climate of the Past Discussions

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Comment on cp-2021-18

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Referee 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-RC1, 2021

This is a very comprehensive review and update of the Late Cretaceous-Paleogene climate record of a key high-latitude locality. The authors are to be commended on bring such a wealth of data together in such a thoroughly integrated and informative way. The combination new ways of seeing established proxies and several new proxies gives us important insights into how this part of the planet functioned under a greenhouse climate. Especially interesting is the potential to compare pollen-based and dinocyst-based proxies with a new suite of GDGT and GDMT proxies. And the work to identify runoff indicators in this record (Fig. 16) is really important for understanding how the hydrological cycle is affected by greenhouse climate events.

There are however a few shortcomings with the paper that need to be addressed. Firstly, the authors need to appreciate that this is not a SW Pacific record but a Tasman Sea record. The degree to which it is representative of the wider SW Pacific is still a matter of debate with so few other records for comparison. However, there is one record that warrants more direct comparison, namely the New Zealand mid-Waipara section, which really is on the western margin of the SW Pacific Ocean. The similarities in the GDGT records from the two sites has been demonstrated in numerous publications (most recently Hollis et al., 2019). The trends in both SST and MAAT for the two sites are within the margin of error for the two GDGT proxies. This in my view calls into question the ocean circulation model perpetuated in this paper and recently reproduced in Huurdeman et al. (2020, Fig. 1). It is simply impossible to see how the same SST values can be generated from the cool southern-sourced “Tasman Current” at site 1172 and from the warm proto-East Australian Current that bathed the eastern margin of Zealandia. This applies for both the super-warm SSTs of the PETM and EECO as it does to the cool SSTs of the mid-Paleocene. A much simpler explanation is that the same currents bathed both sites, with the Tasman Current dominant in the Paleocene and later Eocene and the EAC dominant in the early Eocene (e.g. as suggested in Hollis et al., 2012).

Moreover, the Tasman Current is described as “western boundary current” (p. 12). These currents are the deep currents that drive deep-water circulation. This is quite distinct from the surface-water of the Ross gyre. Also worth noting that abbreviations for compass directions are always in upper case, hence SW not sw.

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. 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.

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. 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. It is also worth noting that the thermophilic and opern 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.

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).

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).

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