Reply to reviewers

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.
Adjustments made: please see the marked-up file, all co-authors contributed in reshaping the introduction into a more coherent narrative.
Along these lines, a greater issue arises when comparing the dinocyst ecogroups with SSTestimates. 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, we will mention this in the discussion. Generally we see no problem in this co-variance. The most important open-ocean dinocysts (e.g., Impagidinium spp., Operculodinium spp., Nematosphaeropsis spp.) are very low in abundance, which signals that the depositional setting was on the continental shelf. In the absence of those, indeed the leftover open ocean dinocysts are those that are also associated to thermophilic conditions (according to Frieling and Sluijs, 2018). We argue that may be is the result of a general co-variance of sea level and temperature on continental shelfs in the ice-free Eocene. 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 $CaCO_3$; Rohl et al., 2004).
Proposed adjustments to the paper: We will add the discussion above to the discussion section 5.3.1
Adjustments made: We added this in lines 350-355.
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. We however argue that a multitude of explanations, seasonality being just one of them, could explain this mismatch. Another explanation could be the reduced response of dinocyst assemblages at these high temperatures, and as such a non-linear behaviour to the temperature forcing. 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.
Adjustments made: We added this in lines 735-740.
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.
Response to reviewer: This is a good point
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.

Adjustments made: We added this in lines 745-750.

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.

Adjustments made: We did throughout. We refer to the track changes file.

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.

Adjustments made: We expanded some of the captions.

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.

Adjustments made: We refer to the track changes file.

Reviewer 2 (anonymous)

General comment

The manuscript presents new paleoclimatic data from high southern latitudes that is consistent with previous interpretations for the region. A strength of the manuscript is that it also evaluates the strengths and weaknesses of proxies for sea surface temperature (SST) including isoGDGTs and mean annual air temperature (MAAT) including soil-derived branched GDGTs. The authors conclude that MAAT is consistently lower than SST during the early Eocene, independent of the calibration chosen and moreover, that the proxies fail to document a rise in MAAT during the PETM and MECO. The factors contributing to this discrepancy (i.e., a change in GDGT source) are discussed, however the incorporation of mixing models may help demonstrate this now that new data (see Lauretano et al. 2021, Nature Geoscience, accepted) is available for the peat/coal of interest.

Response to reviewer: We thank the reviewer for this suggestion. As of today (Jul 6, 2021) the cited paper is not yet available, therefore we cannot use the information as yet.

Adjustments made: The paper now has become available indeed. We do refer to it throughout the paper.

Specific comments

The authors discuss the potential contribution of terrestrial material from Australia throughout the manuscript. As such, reference pollen-based vegetation reconstructions from southeastern Australia should be included in lines 94-96.

Response to reviewer: This is a good suggestion.

Proposed changes to the ms: we will incorporate an overview of pollen-based vegetation in the suggested section.

Adjustments made: We are now much more confident that the largest and most dominant source of terrestrial OM is from Tasmania, and therefore found it not relevant to add detailed reviews of the vegetation of Australia. This will be part of follow-up studies in which the pollen from Site 1172 will be more elaborately investigated.

In lines 172-175 the authors detail the incorporation of “substantial terrestrial input”. Could you please clarify whether the source of the terrestrial input is deemed contemporaneous or reworked or both?

Response to reviewer: The research done on the record (e.g., Willard et al., 2019; Sluijs et al., 2020) do seem to suggest that that terrestrial OM is quasi-contemporaneous to the marine sediments it was found in.

Proposed changes to the ms: we will add this to the section.

Adjustments made: We added this.

Could the authors please elaborate on why smaller Eocene hypothermal events do not stand out clearly at Site 1172. Is it for the same reasons as the PETM and MECO or other factors?
Response to reviewer: Continental shelf records in the Southern Ocean have a general tendency not to show much climate change associated to the early Eocene post-PETM hyperthermals (e.g., Bijl et al., 2013; PNAS), e.g., in TEX86-based SST and bulk organic carbon isotopes (P.K. Bijl, unpublished data). The reason for this is unexplained.
Proposed changes to the ms: no changes made.
Can you please elaborate on possible mechanisms facilitating the warm bias for TEX86-based SSTs in the sw Pacific?
Response to Reviewer: We are unsure where the reviewer wants us to elaborate on this, in the introduction or in the discussion? It either represents an overestimation of temperature by the SST proxies, or an underestimation of regional SSTs by the coarse-resolution fully coupled climate models, or a combination of both.
Proposed changes to the ms: We will make sure that this is adequately presented in the paper, e.g., at lines 79-83.
Adjustments made: this was adjusted in the restructuring of the introduction.
The inability to document a MAAT rise during the PETM and MECO is attributed to a switch in brGDGT sources, namely from soils and peaty lakes, that dampened the proxy response. Here you cite Holdgate et al. 2009 and say the source could be peats in SE Australia. However, earlier in the manuscript you mention that "rivers flowing from southeast Australia drained into the Gippsland and Bass Basins, and that terrigenous
material is unlikely to have reached the ETP." Can you please clarify whether or not you think material from SE Australia could have reached site 1172?
Response to Reviewer: We thank the reviewer for pointing out this contradiction, and we understand the confusion. We feel it is unlikely that all terrigenous matieral found at ETP came from Australian hinterland: a source from Tasmania is more likely. There might however have been a minor contribution of clay, and clay-bound organic matter, from further sources, like the Australian hinterland.
Proposed changes to the ms: We will make the uncertainties in the sources of the terrigenous material clearer in Section 2.4, which leaves plenty of room for further interpreting our results in the discussion.
Adjustments made: As said, we now think that Tasmnia is by far the dominant source of the terrigenous material and terrestrial OM.
In addition, have you considered incorporating new brGDGT data (see Lauretano et al. 2021, Eocene to Oligocene terrestrial Southern Hemisphere cooling caused by declining pCO2, Nature Geoscience) derived from co-eval peats and deriving a mixing model (see Baczynsk et al. 2016 or Lyons et al. 2020 for mixing model examples)? This way you could test whether shifting sources of brGDGTs could be contributing to the absence of MAAT responses to the PETM and MECO.
Response to Reviewer: As much as we are looking forward to seeing the paper by Lauretano in Nature Geoscience, the paper has not yet been published, unless we overlooked.
Proposed changes to the ms: If it becomes available in time, we will incorporate that work into our paper, and consider the approach of a mixing model, provided that we think that would be appropriate for our investigation.
Adjustments made: Applying a mixing model is extremely complicated due to the uncertain independent proxies for end members, as we discuss in the paper for the BIT index, for instance.
You mention that diversity and TEX86 have a modest correlation for long-term trends and short-term trends (PETM), but not the MECO. Can you please elaborate on why this trend doesn't hold true for the MECO?
Response to Reviewer: The why of this was left intentionally open, because we fail to have an explanation. Possible causes might be that the climate shift of the MECO is of critically slower time scales, leading to a less-dramatic ecological disruption. It is then strange, however, that longer-term climate changes, such as tat leading into the EECO, is represented in the diversity.
Proposed changes to the ms: We will add this discussion the text, to at least highlight the paradox..
Adjustments made: Since the entire paper is already quite long, we decided in the end to leave this additional angle in looking at our data unmentioned.
You regularly refer to "Australian hinterland" and "hinterland catchment". Can this please be illustrated on one of your maps?
Response to Reviewer: I am afraid we can put no bounds or limits to the hinterland catchment, because of the uncertainties described above, and because of the fact that the mountain ranges in se Australia are younger than our record (HOLDGATE G. R., WALLACE M. W., GALLAGHER S. J., WAGSTAFF B. E. & MOORE

D. 2008. No mountains to snow on: major post-Eocene uplift of the East Victoria Highlands; evidence from Cenozoic deposits. Australian Journal of Earth Sciences 55, 211–334.).
Proposed changes to the ms: However, we agree that that is a fair point to make, and so we will describe the uncertainties of the hinterland better in the site description.
Adjustments made: We incorporated this in the material section.
How do the authors know the site drifted out of the zone of intense precipitation? Is there fossil or geochemical evidence for this? Latitudinal zones have shifted through time so there is no guarantee the northward movement of the Australian plate would have shifted the site into a new latitudinal/precipitation zone.
Response to Reviewer: Our inference of a drying of the hinterland comes purely from the northward drift of Australia, and prevailing climate conditions at those latitudes today. The coincidental evidence for a more seasonal concentration of precipitation in the middle-late Eocene we derive from our records confirms a climate shift that is not directly related to temperature (the Paleocene was cool and wet, while the middle-late Eocene was cool and drier).
Proposed changes to the ms: We will make that more explicit in the discussion.
Adjustments made: We did rephrase this part of the discussion to make this point more clear.

Technical corrections
Line 186 – please add 'in' after brGMGTs
Line 942 – please change 'bothe' to 'both'
Line 1249 – please change 'prodcution" to "production"
We thank the reviewer, and will adapt these technical errors.