Climate of the Past Discussions

Clim. Past Discuss., author comment AC2
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Reply on RC1

Michael Amoo et al.

Author comment on "Eocene to Oligocene vegetation and climate in the Tasmanian Gateway region were controlled by changes in ocean currents and pCO_2" by Michael Amoo et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-131-AC2, 2022

Reply to Rev#1

Dear referee and editorial team,

Please find below our response to comments and suggestions reviewer#1 raised on our manuscript. We would first like to thank the reviewer for their positive and insightful comments on our manuscript. We respond to each comment raised by the reviewer in the text below.

Rev#1 General comment

This well written manuscript presents new palynological insight from high southern latitudes from the Eocene-Oligocene transition that is consistent with previous proxies and interpretations for the region. A strength of the manuscript is that it details palynofloral change from a continuous section across the transition and uses the NLRs of palynotaxa to estimate temperature shifts across the Eocene-Oligocene boundary. The authors conclude cooling of ~2-3 C occurred across the Eocene-Oligocene boundary. I recommend publication following minor revisions.

Response: We appreciate greatly the positive and insightful comments by the reviewer aimed at improving our manuscript.

Rev#1 Specific comments

The authors conduct diversity analysis on samples with counts >75. It would be useful for the authors to elaborate on why they chose this number of individuals as opposed to higher counts, i.e., >100 or >200 palynomorphs.

Response: The diversity trends shown by rarefaction analyses using ≥75 and ≥100 are very similar. However, including samples with counts ≥75 individual grains offered an added advantage of increasing the resolution of the studied section. To address Rev#1 comments, we have now added to our supplementary data file both calculated diversity indices for lower (≥75) and higher (≥100) counts and will explain further our choice in the method section of the revised manuscript.

In lines 197-199 the authors mention that Microcachryidites antarcticus forms an important component of the gymnosperm assemblage. Can you please detail the percentage of the gymnosperm proportion this species makes up?

Response: We will add a sentence in our revised manuscript that mentions the percentage of *Microcachryidites antarcticus* as a component of total gymnosperms.

In lines 268-274 the authors detail why they assign Myricipites harrisii the NLR Gymnostoma. I recommend they cite Hill et al. 2020 "Fossil evidence for the evolution of the Casuarinaceae in response to low soil nutrients and a drying climate in Cenozoic Australia" during this discussion as it will strengthen their argument.

Response: We thank Rev#1 for this comment and will add the reference to the revised manuscript.

The authors mention in lines 111-112 that non-pollen palynomorphs were recorded. Where is this data? It would be particularly useful to support their suggestion for increasing environmental disturbance (line 353) in PZ 2 which also requires further explanation. I recommend that the authors discuss the type of disturbance they think would result in fluctuations in gymnosperm/cryptogam abundances?

Response: A full list of all taxa (raw dataset) including reworked, contaminants, and non-pollen palynomorphs (NPP) can be found at https://doi.org/10.5281/zenodo.5924930. Here, we followed Climate of the Past publication guidelines and uploaded the raw count dataset onto an appropriate data repository. We will add the doi link to the revised manuscript.

Our NPP records do not provide evidence for environmental disturbance. Although sporomorph-based climate estimates (which relies on presence or absence of taxa) do not mirror the strong and rapid cooling shown by the lipid biomarker estimates, fluctuations in gymnosperms, increase in cryptogams, and an increase in taxa diversity might be linked to the recorded changes in lipid biomarkers which are strongly controlled by the tectonic setting of the site under study. The cooling therefore creates an opening/gap in the canopy which most likely triggers cryptogams to take over, hence their increase close to the top of PZ 2. Following Rev#1 suggestion, we will elaborate on this discussion in the revised version of the manuscript.

In line 361 the authors again interpret a period of disturbance, this time due to an increase in fern spores. Do the NPPs, perhaps charcoal records, support this suggestion? Please elaborate on what kind of disturbance you think this might represent (i.e., environmental or climatic).

Response: We have no evidence for an increase in fires between 35.50-34.59 Ma due to the absence of charcoal particles in the record. We already mentioned in line 362-365 that these disturbances are most likely climatic and compared them to records of cooling and warming in the regional Australo-Antarctic area. Following Rev#1 comment, we will extend this discussion in the revised version and mention the absence of charcoal particles

In line 344 the authors mention an endemic-Antarctic dinoflagellate cyst. Please include the species name in the text.

Response: We will incorporate the species names (e.g., *Deflandrea antarctica*, *Vozzhennikovia* spp., and *Enneadocysta dictyostila*) in our revised manuscript. However, since our study primarily focuses on terrestrial pollen and spores, we limited the discussion on dinoflagellate cyst to their biogeographical location and palaeoecology, hence treating them as a group (e.g., endemic-Antarctic dinoflagellate cyst) and how their abundance are affected by changes in currents and sea surface temperature (SST). A full discussion of the dinoflagellate cyst record for ODP Site 1172 can be found in Houben et al. (2019) and Bijl et al. (2021).

In line 333 the authors discuss that the cooling indicated by both independent proxies is not reflected by the lipid biomarker-based terrestrial MAT estimates and that the reason for this disparate trend remains unknown. Earlier in the manuscript (line 89) the authors mention Permo-Triassic reworked elements. It would be interesting for the number of Permo-Triassic reworked elements, if quantified, to be provided to see if this could be contributing to the disparate trends.

Response: This is an interesting line of argument and we previously thought of it. However, the number of Permo-Triassic reworked elements as a percentage of total pollen and spores were consistently low (averaging < 1%) within the interval of interest (i.e., ~37.5-35.60 Ma), thereby making it highly unlikely for the reworked elements to be influencing the lipid temperature estimates.

Please also acknowledge that pollen and spores can also be transported in water in Line 357.

Response: We agree that palynomorphs can also be transported in water. However, the distance between our study site (ODP Site 1172) and mainland Tasmania (more than 100 km), and no known larger river input at this time in the Eocene make a major influence of river/water transported sporomorphs rather unlikely. We will add this point to the discussion

Rev#1 Technical corrections

The reviewer suggested minor in-text corrections and technical changes.

Response: We will address all suggested corrections and technical changes in our revised manuscript.