Dear referee and editorial team,

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

Best regards

Michael Amoo (on behalf of all co-authors).

Response to Reviewer #1

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 is 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 explained further our choice in the method section of the revised manuscript (as pointed out at Lines 157-160).

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: In the revised manuscript, we have added the percentage of *Microcachryidites antarcticus* as a component of total gymnosperms (as seen at line 209).

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 added the reference to the revised manuscript. (e.g., Lines 281 and 286)

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 added the doi link to the revised manuscript (as pointed out at Lines 103-104 and 482)

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 have elaborated on this discussion in the revised version of the manuscript (e.g., Lines 371-372)

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 extended this discussion in the revised version and further mentioned the absence of charcoal particles (e.g., Lines 381-382)

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

Response: We have incorporated the species names (e.g., *Deflandrea antarctica*, *Vozzhennikovia* spp., and *Enneadocysta dictyostila*) in our revised manuscript (Line 360). 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 abundances 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 subsequently added this point to the discussion in the revised manuscript at lines 375-376.

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.

Response to Community Comment

Community comment: Ian Sluiter

Discussion comment: I write … to provide a few short comments on the paper by Amoo et al. on Late Eocene to Early Oligocene vegetation of the Tasmanian Gateway Region in the hope that it may improve their paper, but also set some ground rules for future authors from the Northern Hemisphere who wish to work on pollen records from this paret of the Southern Hemishpere. My name is Ian Sluiter, and I am happy for you to provide the comments to the authors with my name known. I have been working on, and will publish very soon, a terrestrial pollen record from Gippsland (Victoria) which lies ~730km to the NNW of ODP1172. That is ~5.5degrees latitude north. The general thrust of a warming episode at our site mirrors the changes recorded by Amoo et al., but I am not exactly sure how this has come about.

The major criticism I have of the paper are outlined below.

Response: We thank the author of these discussion comments for giving us the opportunity to clarify important aspects of this study. We are pleased to hear that their unpublished findings from the coal seams of the Gippsland Basin largely mirror the results from ODP Site 1172 presented in our study. The potential issues raised in his comments are not “hemisphere-specific”, but rather result from differences between the site-specific depositional environments and related sedimentology of their terrestrial and our marine site presented in this study (see below).

Discussion comment: *The Methods of the paper declare that a 15-micron sieve was used to presumably clean unwanted organic and inorganic matter. To do so with Australian pollen records (Quaternary or Tertiary) is a disaster. For the Eocene to Early Oligocene vegetation and climates with which this paper is concerned, sieving excludes anywhere between 2-30% of the pollen sum; and potentially 5-70% of the Miocene pollen sum if the authors propose any work on these age sediments. This means all Cunoniaceae taxa bar larger Weinmannia, all Elaeocarpaceae, many of the small Myrtaceae including core taxa like Syzygium, along with Quintinia, Macaranga/Mallotus, Bluffopollis (Strasbergeria), Ulmaceae and even small Proteaceae would be washed down the sink!*

Response: Sieving marine palynological samples is a standard technique and is required to remove unwanted organic/inorganic matter, and to increase the pollen concentration which is much lower in marine sediments than in terrestrial peat samples. Like Contreras et al. (2014), who covered the Early Eocene vegetation record of site 1172, our Eocene-Oligocene study reanalysed slides from the same batch of samples that were originally processed for marine palynology at the Laboratory of Palaeobotany and Palynology of Utrecht University. We systematically compared samples sieved at 15 and 10 µm from the adjacent site ODP 1168 (western Tasmanian margin, Amoo et al. in prep) and ODP site 696 (Drake Passage; Thompson et al., 2021) and found no differences. Small pollen grains from e.g., *Myrtaceidites* and *Cupanieidites* are present in small quantities (<1-2%) in all batches.

Unfortunately, the author of the discussion comment provides no reference for his estimate of up to 70% loss of pollen grains due to sieving, but we can certainly exclude such a loss in our labs, as we also regularly control the sieving residues. However, we agree that sieving (like many other lab procedures which are unfortunately necessary) potentially increases the risk of losing small pollen grains. Following their comments, we added respective remarks to our method section (e.g., Lines 108-111)

Discussion comment: *This is a very serious loss of data and necessarily simplifies any vegetation reconstructions. Moreover, the use of diversity indices like S-W and Evenness become redundant.*

Response: We do not have any evidence for a “loss of data”. How vegetation is reflected in a pollen record is strongly influenced by the location, depositional environment, and sedimentology. In this regard, a pollen record from a marine core provides indeed a “simplified” picture of the terrestrial vegetation if compared to a terrestrial peat bog record which archives the full diversity of the local swamp vegetation. Regardless of the geological time or continent, particular caution should be exercise when comparing biodiversity from pollen records from different depositional environments. We therefore use the index to record relative changes at this ODP site only. In response to concerns raised in the discussion comments, we made this restriction clearer in our method section and explained the particular limitations of marine pollen records (Lines 111-115).

Discussion comment: *This is a marine pollen record from 250km ESE of Tasmania. A Lat/Long at Line 64 would be appreciated.*

Response: The coordinates of Site 1172 are already provided in chapter 2.1 which includes the site description. In case of a misunderstanding, we slightly reworded this part to make it clearer that these coordinates refer to the coring site, and not to the East Tasman Plateau (e.g., Lines 80-83). We could not find any reference for the estimate of 250 km, and we therefore keep our estimate of ~170 km southeast of Tasmania with reference to the 1172 Shipboard Scientific Party (2001).

Discussion comment: *The record seriously over-records the importance of Pteridophytes, at least when compared with terrestrial records from Tasmania and Gippsland.*

Response: We do not understand why our sporomorph record “seriously over-records Pteridophytes”. The late Eocene total spores at site 1172 on average accounts for about 11% of all non-reworked sporomorphs, and this resembles the spore percentage abundance trend of the terrestrial T1 Coal Seam (late Eocene/Middle *N. asperus* Zone) records from the Gippsland Basin and Otway Basin in southern Australia (Holdgate et al., 2017).

Discussion comment: *The vegetation descriptions about Tasmanian vegetation at the time lack substance. I have never seen an Australian Tertiary record without Myrtaceae! I presume they must have been washed down the sink which makes the description a whole lot easier, I guess!*

Response: Mrytaceae (i.e., *Myrtaceidites*) and *Dacrycarpus* (i.e., *Dacrycarpites*) also occur in the pollen record of Site 1172. We think there might be a misunderstanding. The pollen diagram (Fig. 2) does include selected taxa only, as indicated in the figure caption. A full list of all taxa (raw dataset) including those occurring with percentages <1% can be found at https://doi.org/10.5281/zenodo.5924930 (e.g., Lines 482). Here we followed Climate of the Past publication guidelines and uploaded the raw count dataset onto Zenodo data repository.

Discussion comment*: This is an Australian - Not New Zealand Tertiary Record*

Most of the pollen ID's seem to be aligned with NZ Tertiary taxa, and not with Australian

pollen taxa. It is easy to quote Ian Raine I guess, as Australia do not have a compendium like NZ has. But to do so comes with some basic flaws and oversights.

Response: Raine et al. (2011) is to date the most complete and detailed illustrated pollen and spore catalogue available for pre-Quaternary studies of the southern high latitudes. We therefore prefer to stay with this catalogue as the first reference. We are certainly happy to adapt the Australian nomenclature if this helps to improve our reconstruction (see response below regarding *Nothofagidites* nomenclature).

Discussion comment: *It is possible the Sapotaceae palynomorph may be the smaller Sapotaceoidaepollenites cf latizonatus, but I doubt it. I would suggest it is more likely to be S. rotundus*

Response: The pollen at 1172 is a small grain and we therefore keep *Sapotaceoidaepollenites cf. latizonatus*.

Discussion comment: *Comments on Particular Pollen Taxa*

I also have some comments on the Nothofagaceae. Nothofagidites flemingii is a Fusca Type B (sensu Dettmann et al.) but belongs in the modern genus Nothofagus. I would not be combining it in with Fuscospora. Fuscospora and Nothofagus need to be separated in the pollen diagram.

I would not be using the NZ Brassospora pollen types Nothofagidites mataurensis and N. cranwellae in an Australian pollen diagram. They are very similar morphologically to N. emarcidus and this is the pollen taxon I would combine them with. UNLESS one or both of the pollen types were the square sided N. heterus (Cookson) Stover and Evans 1973 which is very common at times in contemporaneous age fossils from Gippsland to the north. If N. heterus has not been recorded – I ask why not?

Nothofagidites lachlaniae? Another NZ pollen type which is probably best aligned in an Australian context with N. incrassatus (Cookson) Dettmann.

Response: We identified clear morphological differences between the *N. matauraensis, N. cranwelliae* and *N. emarcidus* taxa. Morphologically, *N. cranwelliae* and *N. emarcidus* complex do have the same slightly concave to straight sides, however, whiles spines on *N. cranwelliae* decrease in density from poles toward the equator, *N. emarcidus* has these spines evenly distributed across the grain (Dettmann et al., 1990; Prebble, 2016). *N. matauraensis* can mostly be distinguished from *N. cranwelliae* and *N. emarcidus* by its characteristic pentagonal shape.

To simplify our taxonomy and make it easily relatable to Australian taxonomy, we followed the author of this discussion comment's suggestion and combined *N. emarcidus*, *N. cranwelliae* and *N. matauraensis* into the *Nothofagidites emarcidus* complex (contained in the dataset set whose link is posted at Line 482; Table 1 and Figure 2). For *Nothofagidites lachlaniae*, we respectfully disagree that this taxon is more aligned to New Zealand pollen types as previous studies have also found them in Australia (e.g., Pole, 2001). However, to address the concerns raised, we modified the *N. lachlaniae* to *Nothofagidites lachlaniae* complex (e.g., Line 482; Table 1 and Figure 2) to make it more comparable to a previously published Paleocene to Eocene record of Contreras et al. (2014) from Site 1172.

With respect to *Nothofagidites flemingii*, we are reluctant to add it to the modern subgenus *Nothofagus*. As this is a Paleogene record, we prefer to use the conventional nomenclature and keep taxonomic distinction between modern plants and the fossil palynomorphs. Also, we mentioned in our manuscript (Lines 271-273) that we categorised our *Nothofagidites* pollen taxa into *Brassospora*, *Lophozonia*, and *Fuscospora*-types following Dettmann et al. (1990). We therefore would like to keep our categorisation this way as *Fusca* Type B is considered *Fuscospora* according to Dettmann et al. (1990).

It is however important to note that, the above-described taxonomic revisions do not affect the vegetation and climate interpretations.

Discussion comment: *A comment on Phyllocladidites mawsonii. This pollen taxon hits some pretty big numbers further north in Gippsland at the same time. I am seriously curious as to its relatively low representation at ODP1172, especially given the loss of the smalls down the sink. Perhaps it is due to over-representation from the spores.*

Response: Based on our records from Site 1172, *Phyllocladidites mawsonii* (NLR is *Lagarostrobos*) represents one of the common pollen taxa as we mentioned in Section 4 of our manuscript, and in some instances making up to 17% of our non-reworked sporomorphs. The differences in representation of *Phyllocladidites mawsonii* may be site specific, and most likely independent of relative percentages of Cryptogams. This is further supported by representation of *Phyllocladidites* from previously published Eocene records in Site 1172 (Contreras et al., 2014) being comparable to those presented in our records. Nevertheless, it is apparent that *Phyllocladidites mawsonii* may have been overrepresented in terrestrial Gippsland Basin records as compared to same taxon recovered from the Middle *Nothofagidites asperus* Zone in the Groper-1, Mullet-1 and Bluebone-1 wells, offshore Gippsland Basin, southeast Australia (Partridge, 2006b). Coincidentally, the percentage abundance of *Phyllocladidites mawsonii* presented from our marine pollen record on the ETP (ODP Site 1172) is comparable to those recovered offshore Gippsland Basin. We added this information to our revised manuscript (e.g., Lines 294-297)

Discussion comment: *What is Spinizonocolpites sp.? This genus ?Nypa is well gone (extinct) by the Late Eocene from southeastern Australia. No evidence is forthcoming for it at the same time in Gippsland further to the north. Can the authors be absolutely sure that this is not Early Eocene re-working?*

Response: Our *Spinizonocolpites* pollen grain is significantly smaller than 30 microns and we therefore preferred to call this Spinizonocolpites sp. (*Spinizonocolpites*-type; Martínez et al., 2016) with Arecaceae being the nearest living relative (NLR), rather than calling it *Spinizonocolpites prominatus* with *Nypa fruticans* being the NLR. However, according to Macphail et al., (1994), the middle Eocene extinction of *Nypa* in the Gippsland Basin are mostly local and not representative of a region-wide event. Region-wide extinction of megathermal taxa such as *Nypa fruticans* and *Cupanieae* are reported to have occurred in the late Eocene and they never crossed into the early Oligocene (Read et al., 1990; Macphail. et al., 1994). These are consistent with our pollen record as our *Spinizonocolpites sp.* pollen do not cross into the Oligocene. The taxon also occurs together with other warm loving taxa throughout the Late Eocene, providing further support for a synsedimentary deposition. For further clarification, we will add these comments to the revised manuscript.

Discussion comment: *I also question what the entity/identity of Malvacearumpollis mannanensis might be?*

Response: *Malvacearumpollis mannanensis* has an overall circular or spherical shape with circular pores that are usually found in the equatorial region (Prebble, 2016). The pollen grain is covered with conical spines that are evenly distributed. Morphologically, though this taxon may be similar to *Malvacipollis*, its larger (44-85 µm) size separates the two taxa. This taxon forms one of the minor components in Australian Paleogene forests (Macphail and Hill, 2018).

Discussion comment: *Figure 1: Tasmania is a small island compared to the mainland of Australia, but not as small as the scale bar would indicate. This shows a measurement of 50km, when the actual distance is nearer to 120km!!*

Response: This might be a print calibration issue. Our scale shows a measurement of ~110 km and not 50 km. Fig. 1 is certainly not for scale, and to avoid future issues, we removed the scale bar entirely from the figure (e.g., Figure 1a; Line 925).

Discussion comment: *Figure 3: Do not quote Holdgate et al. 2017, for the Gippsland Basin spore pollen zonation. This belongs with Stover and Partridge (1973); Partridge (2006). The pollen zonation age is also wrong. Please have the authors correct the position of the Middle and Upper N. asperus Zone bouindary to 33.9Ma as Partridge (2006) places it. I would also like to see a small stratigraphic zonation discussion of how this boundary was arrived at.*

Response: We used the pollen zonation of Partridge (1999) where they placed the boundary between the middle and upper *N. asperus* Zone at 33.7 Ma. We agree this needs to be updated and the boundary shifted to 33.9 Ma as in Partridge, (2006a). However, the age of the core is well-constrained by magnetostratigraphy, dinocyst, nannoplankton, and diatoms (see Bijl et al., 2021; Houben et al., 2019). We therefore removed the pollen zonation from Fig. 3 as it is not relevant to this study (e.g., Figure 2; Line 945)

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