

Clim. Past Discuss., author comment AC2
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Reply on RC2

Nick Thompson et al.

Author comment on "Vegetation change across the Drake Passage region linked to late Eocene cooling and glacial disturbance after the Eocene–Oligocene transition" by Nick Thompson et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-84-AC2>, 2021

Dear referee and editorial team,

Please find below our response to the comments the reviewer rose. We thank the reviewer for constructive and positive feedback on our manuscript. We propose the changes indicated in the text below.

Kind regards

Nick Thompson, behalf of all co-authors.

Response to Reviewer #2

Rev#2 General comment:

The manuscript presents new terrestrial paleoclimatic data from high southern latitudes. A strength of the manuscript is that it presents and evaluates both palynofloral and geochemical proxies for paleoclimatic change across the EOT. The authors conclude that climate cooling began at 35.5 Ma, coinciding with the opening of the Powell Basin and importantly, prior to glacial onset at 34.1 Ma. The palynofloral evidence is well presented, but the geochemical (n-alkane and TOC) methods and results need reorganizing. One concern is that the authors refer to high-altitude habitats in the text and in Figure 6. I would like them to clarify whether there is tectonic evidence of this or whether it is inferred from the palynofloral assemblages? Although a number of these taxa do inhabit montane areas today because that is where precipitation is highest, throughout the Cenozoic they are known to have proliferated in wet, low-lying areas throughout the southern hemisphere (eg. SE Australia, New Zealand). I suggest the authors carefully reconsider this interpretation. Notably, the wet conditions inferred at the study site might ensure that these plant taxa could have inhabited low land areas. Overall, this study is an excellent contribution to our understanding of southern hemisphere palynofloras and paleoclimates.

Authors response: We thank the reviewer for their constructive comments and suggestions and are pleased to hear such positive feedback. We agree that interpretations of altitude based on taxa alone needs more information to support the suggestion of higher and lower altitude communities. There is evidence that the Antarctic Peninsula was comparable in elevation to the Trans Antarctic Mountains and Dronning Maud Land during the late Eocene (Wilson et al., 2012). Prior to the opening of the Powell Basin the South Orkney Microcontinent (SOM) was still joined to the Antarctic Peninsula (King and Barker, 1988; López-Quirós et al., 2021). This may suggest that exposed parts of the SOM also had a similar elevation. Subsidence of the SOM related to the opening of the Powell Basin (López-Quirós et al., 2021) and erosion have likely reduced the high of these exposed parts of the SOM since the late Eocene. Today topography of the South Orkney Islands reaches a maximum of 1265m (~4150ft; USGS, 2021). We therefore suggest it was likely that there were areas of the SOM that were at higher elevations and supported higher altitude vegetation.

Proposed changes: Lines 371-375: "Prior to the opening of the Powell Basin the SOM was joined to the Antarctic Peninsula (King and Barker, 1988; López-Quirós et al., 2021), which was comparable in elevation to the Trans Antarctic Mountains and Dronning Maud Land during the late Eocene (Wilson et al., 2012). This may suggest that exposed parts of the SOM also had a similar mountainous elevation. Furthermore, the modern topography of the South Orkney Islands reaches a maximum of 1265m (~4150ft; USGS, 2021). Subsidence of the SOM since the late Eocene (López-Quirós et al., 2021), together with erosion likely mean these exposed parts of the SOM were once higher than today, supporting the reconstruction of higher and lower altitude vegetation communities."

Rev#2 Specific comments:

In section 2.3 (Materials and Methods) the authors comment that "The following section will focus on the interpretation of lipid biomarker (n-alkane) and stable isotope data from Site 696". However, I encourage the authors to remove most of this section as it contains background information and therefore seems out of place. Most of the text in sections 2.3.1 and 2.3.2 should be removed and instead incorporated into the discussion or included as background information earlier in the text, not in the materials and methods section. Please only include the equations you need for the results/discussion.

All results in sections 2.3.1 and 2.3.2 should be moved to the results section and placed under relevant subheadings. Please differentiate palynofloral results, n-alkane results and TOC results if the former two are indeed new to this study. If the n-alkane and TOC results are being reporting in Lopez-Quiros et al., in review then please do not report them here and instead refer to them in the discussion. For example, in the results you would state that TOC increased and, in the discussion, list the possible reasons why. This section should outline how you derived these results.

Authors response: The n-alkane and TOC results have been very recently published in López-Quirós et al. (2021) and we therefore agree with Rev#2 suggestions. However, we believe these sections provide useful information to the reader on the background of the

geochemical analysis that was previously carried out as well as illustrating some key trends and patterns in the geochemical data that can and have been used to aid interpretations made in this study. Therefore, we propose the inclusion of Section 2. "Previous Geochemical Analyses" as part of the Introduction between intro and methods

Proposed changes: We have added the additional section "Previous Geochemical Analyses", in order to display the geochemical results and provide the reader with background information (Lines 62-125). Following Rev#2 suggestions, we have also deleted the geochemical paragraphs in the Method section and removed all equations for geochemical analyses.

Rev#2 Specific comments:

For section 3.1 I recommend separating this into two paragraphs, with each clearly distinguishing the differences between Subzone 1a and Subzone 1b. Place the MAT and MAP results at the end of each paragraph too.

Authors response: We agree the inclusion of subzones within a single section may have been confusing and we have amended this section to make our results clearer.

Proposed changes: Lines 237-265 have been restructured following Rev#2 suggestions.

Rev#2 Specific in text comments:

Lines 204-205. Can the authors please clarify why all of these weren't identified to species level?

Authors response: Previously taxa listed in Lines 204-205 (now lines 205-209) were grouped into genera, e.g., all *Nothofagus* taxa identified to species level grouped under the genus *Nothofagus* spp. and so on. We agree that this dilutes the information available to the reader and have rewritten these lines to include species names.

Proposed changes: Lines 205-209: Pollen affiliated with the modern-day genus *Nothofagus* are the most abundant throughout the section, with pollen taxa belonging to the *Nothofagidites lachlaniae* complex, undifferentiated *Nothofagidites* spp., *Nothofagidites rocaensis* and the *Nothofagidites brachyspinulosus* complex being the largest groups. Other major pollen and spore taxa, in order of decreasing abundance include, undifferentiated *Podocarpidites* spp., undifferentiated *Retitriletes/Lycopodiacidites* spp., *Podocarpidites* cf. *exiguus*, pollen belonging to the *Podocarpidites marwickii/ellipticus* complex, *Cyathidites minor* and *Phyllocladidites mawsonii*, which occur commonly throughout the Eocene and Oligocene sections.

Rev#2 Specific in text comments:

Lines 215-216. Can the authors please explain why the rarefaction analysis was based on 50 specimens?

Authors response: There may have been some confusion here. Only samples containing 50 or more in situ sporomorph grains were used, not 50 specimens. We have amended this in text. Furthermore, we selected samples that contained a count of 50 or more grains to be used in analysis because the trends and patterns reveals were similar to those if only using samples that contained 100 or more grains. The use of samples that contained 50 or more grains allowed us to include more samples and fill in some of the gaps left throughout the section if only using samples that contained 100 or more grains. In addition, we have run statistical evaluation of our count sizes based on the methods outlined in Djamali, M. and Cilleros, K., (2020). Statistically significant minimum pollen count in Quaternary pollen analysis; the case of pollen-rich lake sediments. *Review of Palaeobotany and Palynology*, 275, p.104156. Using the Pearson correlation coefficient, we achieved a mean of 0.86 (SD 0.1). These values can be considered excellent and provides confidence in our results.

Proposed changes:

Lines 83-84 "Only samples containing 50 or more in situ sporomorph grains were used for further analysis and evaluation."

Line 100 "Samples containing less than 50 grains were omitted from this analysis."

Lines 172-173 ". Based on rarefaction analysis, the average number of sporomorph species per sample is 13.28 ± 1.05 (mean \pm SD) at a count of 50 grains."

Lines 223-224 "Based on the results of rarefaction analysis the average number of sporomorph species for a count size of 50 grains is 19.63 ± 2.00 .

Rev#2 Specific in text comments:

Please elaborate on any other possible palynofloral sources.

Authors response: Per the reviewers' recommendations we have elaborated on possible sources.

Proposed changes: Lines 310-321: "Sediments may also have been supplied from the southern tip of South America (e.g., the Magallanes Basin and the Fuegian Andes; Carter et al., 2017), due to the more proximal location of the SOM to South America prior to its separation from Antarctica during the Eocene (Eagles and Jokat, 2014). However, detrital zircon ages clearly show a strong dissimilarity between Site 696 samples and South America (Carter et al., 2017). Furthermore, the occurrence of well-preserved palynomorphs and moderate to well-preserved in situ benthic foraminifera, with predominantly angular to subangular terrigenous particles, does not support the notion of long-distance transport of sediments from adjacent sources (e.g., Seymour Island and southern South America; López-Quirós et al., 2021). These observations, together with an expansion of gymnosperm conifers and cryptogams recorded during the early Oligocene (33.5-32.2 Ma) at Site 696, but absent from Antarctic Peninsula and southern South America floras (e.g., Askin et al., 1992; Anderson et al., 2011), suggest that the vegetation of the SOM was unique in character. It is therefore likely that a significant proportion of detrital material, including sporomorphs, was likely of local origin (e.g., exposed parts of the SOM), with some input from the northern Antarctic Peninsula and possibly southern South America during the late Eocene."

Rev#2 Specific in text comments:

Please outline what the significant differences are between SOM and Antarctic Peninsula palynofloras.

Authors response: There may have been some confusion with this point. The differences between the SOM and Antarctic Peninsula are explained in the following sentences (lines 321-327): "In agreement with previous observations by Mohr (1990) the sporomorph assemblage from Site 696 contains a greater diversity of angiosperm pollen compared to late Eocene Antarctic Peninsula palaeofloras (e.g., Anderson et al., 2011; Warny and Askin 2011b; Warny et al., 2019). This higher diversity has also been reported in southern South American Paleogene sporomorph floras (e.g., Romero and Zamaloa, 1985; Romero and Castro, 1986). In addition, the late Eocene Zone Ia assemblage (37.6-35.5 Ma) at Site 696 contains the thermophilic taxa *Arecipites* spp. (Arecaceae), *Myrtaceidites* cf. *mesonesus* (Myrtaceae), and *Polypodiisporites* cf. *radiatus* (*Davallia*) not recorded in coeval Antarctic Peninsula assemblages, possibly due to the more northern latitude of the SOM resulting in milder climatic conditions."

Proposed changes: Minor alterations to the wording of this section to make the comparison between these two regions clearer.

Rev#2 Specific in text comments:

Line 293. Please elaborate on the precipitation requirements of the taxa too, as opposed to focussing on temperature alone.

Authors response: The line in question (now lines 307-310) is within section 5.1 "Sediment Transport and Provenance". We therefore believe that specific references to

climate requirements of vegetation are not required in this section. However, we have taken the reviewers' comments onboard and added clearer links to vegetation, precipitation in modern habitats and palaeoclimate estimates.

Proposed changes: Minor alterations to the wording within section 5.2 "Palaeoenvironment and Palaeoclimate" to elaborate on the precipitation requirements of the taxa and the precipitation regime certain taxa suggest with links to palaeoclimate estimates.

Rev#2 Specific in text comments:

Lines 316-322. Could the authors explain why they dismiss having more than one source area (i.e., one local and one more regional).

Authors response: Per the reviewers' recommendation we have added more information regarding sediment source. We maintain our suggestion that the majority of sediment was supplied from exposed parts of the SOM based on the differences outlined in the pollen and spore assemblage between the SOM and adjoining regions (e.g., South American and the Antarctic Peninsula). We do accept that some sediment was likely supplied from some of these adjacent areas and have indicated accordingly in text.

Proposed changes: Lines 339-358: ". Barriers to the delivery of sediment by long distance gravity flows from the margins of the southern Weddell Sea, further suggested that sediments may have been transported to the SOM by icebergs (Carter et al., 2017). In spite of this, the presence of in situ thermophilic taxa within the early-late Eocene of Site 696 (37.6-35.5 Ma) suggests mild and even ice-free conditions during this overlapping time period. Furthermore, palaeo-sea-surface temperature reconstructions (Douglas et al., 2014) indicate relatively warm conditions (~14°C), and fossil dinoflagellate cyst (Houben et al., 2013, 2019), calcareous nannofossils (Wei and Wise, 1990) and smectite-dominated clay mineralogy (Fig. 2: Robert and Maillot, 1990) support temperate depositional conditions (López-Quirós et al., 2021) not favourable for transport by ice. Unequivocal evidence for ice transport, in the form of ice-rafted debris, at Site 696 is observed within two coarse-grained mudstone intervals within a fine-grained transgressive sequence deposited around 34.1 Ma (Barker et al., 1988; López-Quirós et al., 2021). However, these intervals contain altered glaucony grains most likely sourced from shallower SOM coastal/shelf areas (López-Quirós et al., 2019, 2021). Therefore, these observations and those of this study suggest that transportation by ice from adjacent land areas (e.g., Antarctic Peninsula and Ellsworth–Whitmore Mountains) was unlikely before 34.1 Ma and that a majority of sediments transported to Site 696 are likely of local origin from exposed parts of the SOM as the Powell basin opened isolating the microcontinent from the possible sediment supply of the Antarctic Peninsula and southern Weddell Sea margins."

Rev#2 Figure 3:

Please include a column for the autochthonous pollen sum on Figure 3.

Authors response: We agree with the correction.

Proposed changes: Autochthonous pollen sum column added. Please see Figure 3.

Rev#2 Technical corrections

The reviewer has proposed a number of smaller in text corrections and changes.

Authors response: We agree with the corrections proposed by the reviewer.

Proposed changes: Changes made in text per reviewers' recommendations