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Climate of the Past Discussions

Interactive comment on "Expansion and diversification of high-latitude radiolarian assemblages in the late Eocene linked to a cooling event in the Southwest Pacific" *by* K. M. Pascher et al.

Anonymous Referee #2

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This manuscript explores the evolution of biogeographical gradients in radiolarian assemblages south of Zelandia and in the Tasmanian Gateway from the middle Eocene to the early Oligocene. The drill sites investigated are largely from the DSDP cores 277 and 280, 281, and 283 with some results from ODP Site 1172. The results of the radiolarian assemblage reconstructions are intriguing, as they show a preponderance of high latitude and cosmopolitan taxa throughout the region and a few low latitude taxa. Results are discussed in the context of the evolution of global climate change as climate cooled progressively from middle Eocene warmth to the cool early Oligocene.

If the authors can address the comments by David Lazarus regarding the biogeographic assignments of certain species without nullifying the main conclusions, this manuscript should be published. The results address a period of time in the early Cenozoic that is increasingly being recognized as an important part of the well documented long term global cooling trend starting in the middle Eocene. Some of the paleoceanographic reconstructions of the Southern Ocean during this time period are controversial, with some geochemical and faunal records calling for extreme warmth close to Antarctica well into the late Eocene, with others suggesting cooler temperatures and even an episode of small ice sheet development in the late Eocene.

Overall I think the authors have done a nice job framing the questions to be addressed and giving adequate background of the current understanding of the global climate state. The long oxygen isotope record from Site 277 is in itself a very nice contribution. Even with the poor recovery at Site 277, a stable isotope record from the southwestern Pacific that can be correlated to other Southern Ocean sites is very important.

The authors mention, almost in passing, that their faunal and geochemical results are in conflict with the geochemical records of Bijl et al., 2010 and Liu et al., 2009. The authors should at least attempt an explanation of why this discrepancy exists.

My biggest issue with the manuscript is the tectonic reconstruction and inferred circulation patterns shown in Fig. 8. First off, the Matthews et al. {2015} includes rotations for 85 to 45 Ma. There are no plate motions before or after this time. Why are the authors citing this study? Also, it is unclear how the authors created the maps shown, as the continent polygons and continent ocean boundary polygons (COBs) in Seton et al. {2012} show a connection between the conjugate margins until 35 Ma at the earliest. Separation of the conjugate margins across the Tasmanian Gateway can be explicitly reconstructed using the continent-ocean boundaries (COB) identified in {Williams} and with the rotation parameters of Cande and Stock {2004}. Another thing that suggests the reconstruction needs to be revisited is the apparent overlap of the North and South Islands of Zelandia, which is in a submitted manuscript (Müller), but not in the cited

Seton et al. {2012}. What I think has happened is that the authors have used a different polygon file with updated rotation files. Whatever the reason is, the conjugate margins must be shown to be connected until 35 Ma if Seton et al., 2012 is to be cited. The upshot of all of this is that only a shallow (read, shelf depths) existed until 35 Ma. If the extreme seaward boundary of Williams' COB is used the connection can be said to persist until 32 Ma (see Scher et al., 2015). The paleogeography of any reconstruction shown should accurately reflect the source of the reconstructions. If the authors are using a different polygon file from Seton et al. (2012) they should be sure to cite it correctly. Otherwise the reconstructions should be remade with the polygon files from Seton et al. (2012). Moreover, the location map in Fig. 1 should be updated with a polygon file that includes the continuous COB between Tasmania and the South Tasman Rise.

Also the authors need to clarify what they mean in reference to the Tasmanian Gateway being fully open (example is line 15 page 24). Fully open meaning to deep waters? The geophysics of the COB and tectonic reconstructions allow for fairly explicit ages for a deep connection. A deep connection was established 33.5 +/- 1.5 Ma. I would also like the authors to consider that water mass reconstructions of Scher et al., 2015 support the hypothesis by Bijl et al., 2013 that the first current to flow through the gateway flowed westward (from Pacific to Indian), probably under the influence of the polar easterlies, as the gateway was in a more southerly position. I do not think that the flow regime described by Scher et al., 2015 is inconsistent with the faunal and geochemical results presented here, though the authors should consider this.

The above flow regime persisted until 30-29 Ma, when the northern margin of the gateway appears to have crossed into the westerlies. The arrow currently drawn through the gateway in the reconstructions in Fig. 8 is not consistent with the recent water mass reconstructions (Scher et al., 2015). I also think that the label ACC should be removed from panel D. The large scale homogenization of water mass tracers throughout the Southern Ocean, pointing to establishment of the ACC, does not occur until after 29

Ma.

Overall, I like this manuscript. My issues with the reconstruction are obviously not critical for the main conclusions, however I would like to see a consistent reconstruction with appropriate citations before the paper is published.

Interactive comment on Clim. Past Discuss., 11, 2977, 2015.