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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.

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This study looks at how ocean surface water masses developed in the southwestern Pacific through the Eocene and Oligocene, using both stable isotopes and radiolarian faunal patterns. The main conclusion is that regional latitudinal gradients increased and the biogeography became more strongly differentiated in the late Eocene to early Oligocene. It is a useful, if perhaps somewhat incremental advance on several earlier studies using the same proxies (stable isotopes, radiolarian faunas), partially from the same set of deep-sea sediment cores. The main new contributions of this study are:

integration of isotope with radiolarian faunal data; time-series of both from a Site (277) with a fairly long, if not very well recovered section through the time interval; interpretation that places the individual sections more explicitly in the regional geographic context than earlier studies have. There is also possibly a substantial new synthesis of Paleogene radiolarian species' biogeographic patterns (but see below). The ms is mostly well documented with taxonomic notes, plates, and tables of raw data.

Although the ms needs major revision, it is in principle well worth publishing. I will not attempt to review the extensive literature on Paleogene stable isotope studies here and instead concentrate my comments mostly on the radiolarian faunal analyses which are less frequent and a more distinctive component of the current ms.

Cenozoic circum-polar Southern Ocean biogeographic patterns were first summarised from literature data by Kennett (1978). Several subsequent more detailed studies of regional change in plankton across specific time intervals by later deep-sea drilling were synthesized by Lazarus and Caulet (1993) into a set of circum-polar maps. These authors also did a first general synthesis of radiolarian biogeography, including patterns of change across the Eocene-Oligocene. Nelson and Cooke (2001) provided a very detailed synthesis of various paleoceanographic proxies for the Cenozoic in the southwestern Pacific. Funakawa and Nishi (2008) gave a detailed radiolarian time series study across the Eocene-Oligocene boundary in the Atlantic sector of the Southern Ocean. Lazarus, Hollis and Apel (2008) did a more detailed study of radiolarian biogeographic patterns and trends of the Eocene-Oligocene interval in the southwest Pacific, and included all sections used in the current ms by Pascher et al. Lazarus et al.'s (2008) biogeographic analysis however was still somewhat limited in time resolution (data was binned to ca substage level only) and did not provide any new primary radiolarian faunal biogeographic data, although it did present a comprehensive circum-polar synthesis of Southern Ocean radiolarian biodiversity and turnover patterns in the Eocene and Oligocene, based on a very detailed study by Apel (documented in an unpublished but freely available PhD thesis). The main conclusions of all these prior

studies are that the Southern Ocean first formed in the late Eocene, based primarily on the biogeographic patterns and taxonomic composition of the plankton, although the distinctiveness of the patterns strengthened in the early Oligocene.

My first substantial criticism is that too little of this prior work is made visible to the reader of the current ms. Although several of these prior studies are cited, they are cited without providing any real information as to their content. Indeed, one might get the false impression from the Pascher ms that rather little has been known until now of southwestern Pacific plankton biogeographic evolution, or that the timing of ocean water mass origins is a new discovery, or that the sections used in this study have not been extensively examined already for radiolarian faunal characteristics. A brief but proper review of prior studies and the significance of the new ms results in the context of this prior work needs to be added to the introduction section of the paper.

A second substantial criticism is the assignment of individual radiolarian species to biogeographic categories, e.g. Antarctic, Tropical, etc. Paleogene radiolarian biogeography unfortunately is not at all well known for most species, in contrast to the more extensively studied, and far less diverse groups like foraminifera and calcareous nannofossils. The early synthesis by Lazarus and Caulet was based primarily on the subjective but extensive experience of the two authors (Lazarus for the Neogene, Caulet for the Paleogene) as there were at the time no methods available to easily synthesise the scattered primary literature. Many tools are now available which in principle allow a more rigorous, objective basis for biogeographic interpretation, and I had hoped that this ms would provide this as a new, better foundation for current and future research. The authors unfortunately do not provide any details as to how biogeographic assignments were done for individual species, nor is this available from the SOM or the other papers cited. This is a missed opportunity at the least. Nor are the biogeographic assignments given always plausible. For example, Lithelius minor is definitely cosmopolitan, although it does seem to be less common in tropical than temperate sections in the late Neogene. Also problematic are the two species (A. murryanum

and A. prolixum) which are the basis in the ms for inferring a tropical water conditions extending into polar regions in the Eocene. Both these species are reported in the literature in a wide variety of locations in the Paleogene: the Russian platform, Poland, Kamchatka, the northern Atlantic, Argentina and even well south of the Pascher study region, from the Kerguelen Plateau. Liu et al. (2011, Palaeoworld) explicitly challenge the claim that these species are tropical indicators. The ms needs to provide (in the SOM) a brief but sufficient explanation as to why a given species is assigned to a biogeographic category. Funakawa and Nishi's (2008) study gives a good example of how this can be presented - a set of sites with radiolarian faunas from the time interval, marked by either presence-absence or relative abundance symbols. The authors have available to them the materials (including the MRC collections used by Funakawa and Nishi, one of which is housed at their institution) and if desired, access to global published occurrence data, either via formal databases such as the NSB system or simple searches of community shared pdf literature archives. Hopefully the authors have in fact done something similar already and only need to provide the documentation.

Another issue is the data analysis. I do not see anything in it that, if done differently is likely to completely change the interpretations, but nonetheless there are some weaknesses that could be improved, and which might well also improve the clarity of the results. The most important here is the use of percent values for biogeographic categories that include the most common category of 'unknown'. This makes the patterns, e.g. the time series changes particularly sensitive to the amount of unknowns. This is problematic because the percent unknown is partially a function of preservation/abundance in the sediment, as can be seen in Figure 6 - taxic richness correlates with % unknown throughout the lower half of the main data series in Site 277, and taxic richness in this interval is certainly a function of abundance since the sample count values are very low (see the spreadsheet in the SOM - a plot of species richness vs total count [attached - not fully kosher as it is not one sample, but at least all from one site] gives a typical sampling curve where true richness values are only seen for counts in excess of ca 2,000 speciemens). The specific mechanism linking this to %

unknown can only be guessed at but probably reflects poorer preservation increasing the number of individuals not identifiable to species level.ï£ij I suggest recalculating the biogeographic affinities to % of individuals that have been assigned to a biogeographic category only. I suspect that the resulting plots will show the trends claimed in the paper even better than the current figures do.

Also, given the the fully typical dependence of raw species richness on sample size, if the authors wish to mention diversity in any other sense than as a synonym for abundance (which is the current state of the ms) they should use some sort of subsampling proceedure, as is long standard in other areas of biology-paleontology. Simple rarefaction might be adequate in this material. I would only include species level identifications in this.

There is an issue with the Site 1172 data, which forms a significant part of the analysis. Unlike the other data this is not in the SOM or cited publications - the 2009 Suzuki paper is just a taxonomic survey. It would be better to have this data in the SOM, and Suzuki added as a junior author for the ms.

Lastly, a comment on the isotopes. I am not very convinced by the correlations shown - the only feature of the new data that seems clear is the oxygen isotope shift equivalent to the E-O boundary. It might be better to show each Site's age models (in the SOM) and plot the isotope data for both 277 and the reference site 689 vs age, adding correlation lines based on biostrat events. This might make the claimed correlations of the very gappy data in 277 to 689 more plausible.

There are many other more minor suggestions or corrections in the annotated version of the ms, also attached.

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/11/C1194/2015/cpd-11-C1194-2015-supplement.pdf

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

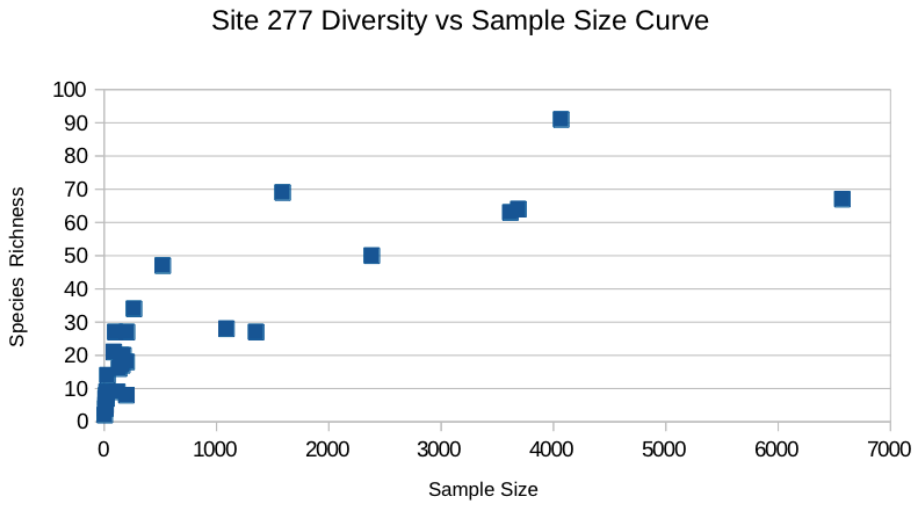

Fig. 1.