

Interactive comment on “Southern high-latitude terrestrial climate change during the Paleocene–Eocene derived from a marine pollen record (ODP Site 1172, East Tasman Plateau)” by L. Contreras et al.

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General comments Firstly apologies for the delay in getting the review prepared. Secondly thank you for asking me to comment on this manuscript. In general I found this an interesting piece of research with thoughtful handling of the data and careful construction. This is a polished piece of research. The manuscript is certainly detailed with good information on topics such as the source area for the pollen and spores and

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considerations of how the assemblages from this site relate to those from surrounding regions (if anything, I thought the paper could benefit from rationalising since it felt repetitive in places). The treatments of the data are interesting although I hold serious reservations regarding the use of NLR approaches on such old material (more below). The results of this research will interest the readership of the journal and a suitable revised manuscript would be a good addition. I don't think this will need a great deal of work.

My major criticism is that nearest living relative approaches should not be applied to material as old as the samples here. Several papers have appeared in recent years using such methods on increasingly older material but the central problems of the approach have not been addressed and have not gone away in the past few years. My experience is from the northern Hemisphere and in these cases most genera, let alone species, do not evolve until well into the Eocene. Plants display pronounced mosaic evolution of their organs. The tables of botanical affinity presented in this manuscript clearly show the lack of precision of determining systematic affinity for many of these sporomorphs. In addition gymnosperm pollen and spores have few distinguishing features and in material older than the Mio-Oligocene are difficult to split into families sometimes, let alone genera. The NLR approach here is taken on family level affinity in many cases and also genera for gymnosperms, spores, and some angiosperms. I have little expectation that spores and many gymnosperms will provide reliable estimates. Many families have genera within them with quite different tolerances. In this manuscript some sample points have only 5 NLR that inform the climate estimates and this is far too low for a robust signal even from well documented modern NLR estimates. The other factor is that the count sizes are generally small and so there is no telling whether the sporomorph assemblages are representative of the parent vegetation type or just a winnowed subset that is biased. Many modern plants do not occupy their full ecological niche (an interesting puzzle for plant ecologists) and this has implications for the full climate estimation of the palaeobotanical record. The error bars are very large – are these results really statistically significant? These seem to be steered by a

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few data points only. Personally I don't think MAP in fig 4 is showing anything of note. In summary the results might be right, or they might not. I really think the authors need to justify why they are undertaken such a controversial method on this material so that the reader can decide on whether they can have reassurance in the estimates. The NLR may be useful for indicating relative change but most users of this manuscript will see e.g. MAT 12 degrees c, CMMT 7 degrees, WMMT 18 degrees for the early/middle Paleocene and assume it's a robust indication and run with that figure.

The vegetation change seems reasonable. The shifts really mirror the northern hemisphere records as well. To my eyes it's interesting that the changes in relative abundance of sporomorphs, as reflected in the DCA, are actually far more sensitive to environmental change than presence-absence of sporomorphs – the type of data used for the NLR estimates. The source area is neatly argued. I do think, however, that since these are marine records that mix sporomorphs from many different vegetation types (and potentially from several different regions depending on the rivers that feed onto the continental shelf) that identifying canopy from understory vegetation is a step too far.

Specific comments P.294, ln 17 – this statement is sweeping and needs a citation. p.296, lns 7-10 – the main driver of the paper is to construct a terrestrial climate record but surely this is not going to be any different in relative terms from the marine temperature estimates anyway? p.297, ln 24 – this is sweeping. Surely tectonic changes will drive the paleoceanographic changes will might have a knock-on impact in the terrestrial realm? p.299, ln. 8 – it's usually standard to sieve at 10 microns for pollen and spores. Why 15 microns? Is this because many southern hemisphere/Australian sporomorphs are gymnosperms and pteridophytes and therefore way bigger than 10 microns? Have you lost any small and potentially useful pollen through processing? p.299, ln 17 – the plates are good. I like these. The preservation is clear. p.299, ln. 20 – I think you should make clear that you are using DCA to show the pattern of vegetation change. As it currently stands this isn't clear since you aren't using ordina-

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tion for heuristic purposes. p.300, ln.1 – what do you mean by “drillings”? This needs rephrasing. p.301, section 2.5. “Statistical examination...” I found this whole section far too opaque and difficult to understand. The supplementary has more information on this technique but it didn't help me. I have no idea what this test is doing. A more user-friendly approach is needed here to tell the reader what this technique is doing. At present it reads like the authors don't really understand it either. Since only 6 data points correlate directly with SST estimates, is there really any validity in using this method? What is meant by “we multiplied imputed values of DCA axis 1...” (ln. 13). Are you interpolating? Is this robust and valid? I don't know and I can't independently verify or interpret these results. p.303, ln 29. – is there any macrofloral evidence for cycads and palms on Gippsland, Tasmania or neighbouring terrestrial basins? *Arecipites* is a junk pollen morphotaxa for many monosulcate +-reticulate pollen grain. They appear in the middle Paleocene of the Arctic as well and there is no macrofloral evidence for their presence for the whole Paleocene or early Eocene. At lower latitudes there is agreement that macrofossils and palynomorphs referable to palms are present in the fossil record. p.304 ln 3 – this sentence needs rewording. p.305 ln1 – the palaeogeographic map indicates that the primary current is from the south to north rather than north to south which would accord better than Tasmania as the source for the organic matter. Is this an issue? How do the pollen and spores travel offshore? Fig. 3 – I suggest you ditch the rounded hulls on the DCA clusters and either go with empirically derived ones that fit the actual data points or enlarge the symbols.

Interactive comment on *Clim. Past Discuss.*, 10, 291, 2014.

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