

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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Dear Dr. Rosseau,

Please find below our detailed, point-by-point response to the comments of Referee 1 (R1; Dr. Guy Harrington). In order to alleviate the tracing of the improvements made in the manuscript, we have highlighted all changes in green (see cpd-10-C372-2014-supplement).

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Response to Referee 1

In his introductory comments, R1 refers to our study as “an interesting piece of research with thoughtful handling of the data and careful construction. This is a polished piece of research”. We appreciate R1’s overall very positive assessment of our paper. In the following, we discuss every point raised by R1 and line out the improvements made in the revised version of our manuscript.

R1 states “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”. We fully agree with R1 that the NLR approach can be compromised by several factors. To provide maximum clarity for the readers on potential drawbacks of the method, we have added a new paragraph in Section 2.4 of our revised manuscript (lines 254-269) that discusses potential shortcomings and also provides new, additional references (Mosbrugger and Utescher, 1997; Mosbrugger, 1999; Pross et al., 2000; Utescher et al., 2000; Poole et al., 2005) on different aspects of the methodology. Although we respect R1’s view that NLR approaches should not be applied to material as old as our samples (i.e., Paleocene – Eocene), we do not share it in full. In a broader sense, the discussion of this issue is connected to the validity of the actualistic principle and its application in “deeper-time” geoscientific problem solving. While we agree with R1 that the connection between living and fossil organisms becomes generally weaker the further one goes back in time, the underlying premise of the NLR approach is straightforward: If a fossil organism (or an assemblage of fossil organisms) has a close living relative, it appears a priori reasonable to assume that the climatic requirements of the fossil organism (or of the fossil assemblage) were close to those of its nearest living relative. Thus, morphological or systematic similarity is assumed to reflect similarity in climatic tolerance. As early as the 19th century, Heer (1855 - 1859) systematically followed the NLR concept to

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quantitatively reconstruct Paleogene and Neogene paleoclimates from the Swiss Molasse Basin. In a qualitative, very generalized way, this approach is even used for the Mesozoic and Permo-Carboniferous (compare reviews of Vakhrameev et al., 1978; Behrensmeyer et al., 1992); an obvious example comes from ferns, which are generally considered to indicate more humid climates, whereas seed plants are regarded to be more common under drier conditions. Nevertheless, the basic assumption of the NLR approach as outlined above (i.e., that morphological or systematic similarity between living and fossil taxa reflects similarity with regard to climatic requirements) may admittedly be wrong, and it also appears reasonable to assume that the chance for errors generally increases with the age of the fossil taxa. These potential shortcomings can, however, be addressed, identified and corrected via the application of multi-proxy approaches. With regard to the application and verification of quantitative NLR approaches on paleobotanical remains from the Paleogene of the high southern latitudes, several studies have become available over the past decade demonstrating that the NLR concept yields robust, reproducible climate data even for the early Paleogene:

-For the late Paleocene to middle Eocene, Greenwood et al. (2003) combined bioclimatic analysis with leaf physiognomy and leaf margin analysis to estimate MAT from different locations in southeastern Australia. They obtained largely consistent results from all approaches. Moreover, they showed that their temperature estimates are also consistent with the major shifts in vegetation composition during the early Paleogene.

-For the early Eocene, Carpenter et al. (2012) estimated MAT using a multiproxy approach based on macrofloral and sporomorph records from Tasmania. They applied the coexistence approach, mean leaf length method, and a newly pioneered method using the dependence of leaf sizes with respect to climate parameters in the widespread conifer genus *Podocarpus*. Again, the independent proxy estimates yielded consistent results, underscoring the applicability of the NLR.

-For the early and middle Eocene, Pross et al. (2012) generated NLR-based climate

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estimates for a number of temperature and precipitation parameters from sporomorph assemblages retrieved off eastern Antarctica. Their estimates, which are derived from two NLR-based methods that yielded internally consistent results, are fully consistent with the output of climate model runs (Huber and Caballero, 2011) and notably the results from other, completely independent paleoclimate proxies. These proxies include the organic geochemical terrestrial MBT/CBT paleothermometer (Pross et al. 2012; Bijl et al. 2013), detrital geochemistry (Passchier et al., 2013), organic geochemical marine TEX-86 paleothermometry data (Bijl et al. 2013), and mass occurrences of the tropical dinoflagellate cyst genus *Apectodinium* (Bijl et al., 2013). The validity of the NLR results obtained by Pross et al. (2012) is further underscored by new data on the biogeography and phylogenies of palms (Trénel et al., 2007; Baker and Couvreur, 2013).

In our view, these multi-proxy studies yield compelling evidence that the application of a quantitative NLR approach to early Paleogene floral remains from the high southern latitudes yields robust results. Hence, they provide a sound basis for the methodology followed in our paper.

R1 further points out that “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.” We agree with R1 that in many cases the NLR approach used in this study is based on the family level. The climatic ranges as derived from family-based NLRs may become wider and therefore perhaps less ‘meaningful’ than if genus- or species-based NLRs are used, but they certainly

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do not become wrong. With regard to families that contain genera with quite different climate tolerances, the climatic profiles for these taxa are particularly wide, but again this does not mean that they cannot be used meaningfully for climate estimates. All pollen and spore affinities given in our study are firmly established based on a multitude of studies from the high southern latitudes (see e.g., Cookson, 1957; Macphail et al., 1994; Mohr, 2001; Raine et al., 2008; Truswell and Macphail, 2009). With regard to the identification of gymnosperms to the genus level, we fully understand the concern of R1. However, different from the situation in the northern hemisphere, some gymnosperm pollen from the southern latitudes can be connected to their parent plants on the genus level with very high confidence, which yields particularly meaningful climatic profiles. This is, for instance, the case for fossil taxa connected to the genera *Lagarostrobos*, *Podocarpus*, *Dacrydium*, and *Phyllocladus*. Their established affinities are based on the thorough examination of fossil and recent specimens by Cookson (1953) and Cookson and Pike (1953b; a), and they are well accepted in early Paleogene sporomorph studies from the southern latitudes (see e.g., Macphail et al., 1994; Raine et al., 2008; Truswell and Macphail, 2009).

R1 further writes that “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”. Although we understand the point raised by R1, we do not share it in full: As portrayed in our supplementary information table, the climate estimates presented in our study are on average based on eleven NLRs. This number is close to the minimum (10–12) suggested by other studies employing NLRs approaches on late Cretaceous to Oligocene material (see e.g., Poole et al., 2005; Eldrett et al., 2009). Only in one sample from Site 1172 were only 6 taxa with known NLRs identified. For this sample (i.e., 611.36 m), the results are nevertheless highly significant due to the presence of *Nypa* within the sporomorph assemblage. The presence of this taxon represents an important constraint for the climate conditions as also stated in our manuscript (compare lines 655-656) of revised manuscript). Moreover, we believe that the error bars shown in Fig. 4 of our manuscript and the error

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values presented in the supplementary information for all climate estimates provide an in-depth assessment of the robustness of our data.

R1 writes 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”. Our results are based on samples with counts >100 individuals, with an average of 170 individuals per sample. The reduced counting sums represent a tribute to the marine nature of the samples from ODP Site 1172, an issue that holds true for most marine-based sporomorph studies. As mentioned already in the submitted version of our manuscript, it was necessary to count up to six slides per sample (with large cover slides) to reach counts between 100 and 300 individuals. We understand that 100 grains might not represent all members of an assemblage due to the potential absence of very rare taxa that only reach low percentages (i.e., ~ 3 % or less). However, even if some of these very rare taxa are not registered, the results will still allow to confidently identify larger-scale vegetation groups, and this is exactly the approach followed in our paper. Indeed, we observe that samples with close to 100 individuals and samples with counts >250 individuals show similar trends with regard to the main vegetation changes discussed in our study. Hence, we may conclude that counts of 100 individuals are enough in order to obtain the proportional distribution of the main taxa, as it has also been suggested by micropaleontological studies (see Fatela and Taborda, 2002).

R1 further points out that “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”. We fully agree with this point of R1. This assumption is an important issue when applying an NLR-based approach on the fossil record. Hence, in order to enhance clarity, we have added the existence of this potential biasing factor to Section 2.4 (lines 259-260 of revised manuscript).

R1 suggests that “the error bars are very large – are these results really statisti-

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cally significant? These seem to be steered by a few data points only. Personally I don't think MAP in fig 4 is showing anything of note". We do not follow the personal view of R1, and we are admittedly also uncertain what R1 may mean by "really statistically significant". We believe that showing the error bars (whether they are large or not) means showing the significance of the data. As a consequence of the uncertainties of the method used, the error bars for some samples are admittedly quite large. Specifically, the MAP values presented in our manuscript have potentially important implications for modeling studies (see e.g., Abbot et al., 2009). The precipitation data presented in our study, even if they have large error bars, represent a first step towards assessing the role of high humidity within the complex climatic conditions of the high southern latitudes. Further comparisons of these data with information from other proxies (e.g., deuterium/hydrogen [D/H], alkaline major element geochemistry) are certainly desirable in order to better constrain the precipitation results presented in our study. In any case, we find it essential for the discussion presented in Section 4.3 to portray the MAP values in Fig. 4.

R1 further points out "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 estimate. 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". We respectfully disagree with R1 on this point. As elaborated upon higher up in our rebuttal letter, a number of multi-proxy studies provides sound evidence that the application of a quantitative NLR approach to early Paleogene floral remains from the high southern latitudes yields robust results. Hence, they provide a sound basis for the methodology followed in our paper. Nevertheless, in an effort to provide maximum clarity to the readers we have now incorporated a discussion of the potential biasing factors in Section 2.4 (see lines 254-269 of revised manuscript).

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R1 states that "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". Again, we respectfully disagree with this view of R1: Our discussion on the structure of the forests mainly focuses on taxa that include members of Podocarpaceae, Araucariaceae, *Nothofagus*, Cyatheaceae, Proteaceae, Casuarinaceae, and Euphorbiaceae/Malvaceae. They are widely known not only from marine, but also from terrestrial records of early Paleogene age from Australia (e.g., Macphail et al., 1994; Greenwood et al., 2003), Antarctica (e.g., Askin, 1990; Truswell and Macphail, 2009), New Zealand (e.g., Pocknall, 1990; Raine et al., 2009), and southern South America (e.g., Barreda and Palazzesi, 2007; Quatroccio et al., 2011). In light of this extensive body of literature, it appears reasonable to assume that their habitats and life strategies are sufficiently constrained in order to at least tentatively assess which elements were part of the canopy and which were part of the understorey. The most straightforward example for this notion may be the role of Araucariaceae within a vegetation unit – given the growth form of these plants it appears more than reasonable to connect them to the canopy rather to the understory. Moreover, similar reconstructions of the vegetation (including a differentiation between canopy and understorey) using these taxa has been performed successfully on early Paleogene (Macphail et al., 1994; Truswell and Macphail, 2009; Contreras et al., 2013) and late Cretaceous (Specht et al., 1992) records from southeast Australia and Antarctica.

The specific comments of Reviewer 1 are discussed in the following:

p. 294, ln. 17: "this statement is sweeping and needs a citation." In order to comply with this suggestion of R1, we have added a reference (see Thomas et al., 2003) for this statement in the revised version of our manuscript.

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

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marine temperature estimates anyway?” We do not follow R1’s line of argument here: R1 is correct in the sense that the main purpose of our study is to reconstruct the terrestrial climate conditions on Tasmania during the early Paleogene. However, we respectfully disagree with R1’s view that a “terrestrial climate record surely is not going to be any different in relative terms from the marine temperature estimates anyway” – such a view relies on the a priori assumption that there is always a direct, one-to-one coupling between marine (surface-water) and terrestrial temperatures. In our opinion, such a view does not do full justice to (pale)oceanographic processes and may be somewhat too simplistic (compare also discussion in Pancost et al., 2013).

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?” We agree with R1 that paleoceanographic changes are likely to have a strong impact on the terrestrial realm. We have rephrased this sentence to alleviate R1’s concern.

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?” As mentioned already in the submitted version of our manuscript, the palynological samples used in our study were processed at the Laboratory of Palaeobotany and Palynology of Utrecht University (Bijl et al., 2011) and were here reanalyzed for terrestrial palynomorphs. Based on our previous experience on early Paleogene records from the high southern latitudes, and in line with R1’s suspicion, only very few pollen grains have a size $<15 \mu\text{m}$. Therefore it is highly unlikely that the results have been compromised by any processing-induced bias.

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 ordination for heuristic purposes.” We agree with this point made

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by R1. Following the suggestion of R1, we have rephrased the respective part of the manuscript, making clear that the purpose of using DCA in our study is to show the pattern of vegetation change (see lines 217-218 of revised manuscript).

p. 300, ln. 1: “what do you mean by “drillings”? This needs rephrasing.” Following R1’s suggestion, we have changed the work drillings into drillholes in the revised version of the manuscript.

p. 301, Section 2.5: “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”. We agree with R1 that in principle it would be desirable to be more “user-friendly” about the method used. Following his suggestion, we have made some re-arrangements in Section 2.5 (see lines 281-290 of revised manuscript). The state space model has been implemented by our co-author R. O’Hara, who has long-standing expertise on the numerical analysis of ecological communities. In essence, the technique examines the correlation between DCA Axis 1 scores with SSTs derived from TEX86 using the best model possible for this kind of data. As portrayed in Fig. 4, both variables (i.e., DCA Axis 1 scores and SSTs) appear to closely mirror each other, and the state space model was applied to mathematically verify this correlation. We understand that the method appears complex to understand, also because it (or similar approaches) are not yet widely used in our field. Unfortunately, however, it is difficult to break down the method to a level that a non-expert user can easily understand the details. This is because several statistical concepts would have to be addressed – an endeavor that is beyond the scope of a paper to be published in “Climate of the Past”. As an alternative solution, we have therefore added an explanatory reference (Gelman et al., 2003) in the revised version of our paper (see lines 289-290 of revised manuscript); that way together with other references already added in our previous version of the manuscript (West and Harrison, 1997; Petris et al., 2009), readers interested in in-

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depth background information can easily access further reading.

p. 303, ln. 29: “is there any macrofloral evidence for cycads and palms on Gipp-land, 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.” There is macrofloral evidence for *Nypa* (mangrove palm), cycads and others palms for the Lower Eocene of Tasmania (Pole and Macphail, 1996; Pole, 2007), and macrofloral records of cycads for the Eocene of southeast Australia (Greenwood et al., 2003). Undescribed Australasian fossil palms include a *Linospadix* H.Wendl frond from the middle Eocene Anglesea site in Victoria (J.G. Conran, pers. comm. to J.I. Raine, 2014). With regard to the presence of *Arecipites*, we respectfully disagree with the idea that “*Arecipites* is a junk pollen morphotaxa for many monosulcate + reticulate pollen grains”. Following the redefinition of the genus by Nichols et al. (1973), *Arecipites* only includes monosulcate taxa, with the sulcus being long and tapered at the ends, the exine being 1 μm thick and tectate, the columellae being distinct in some specimens, and the sculpture being scabrate to psilate. Moreover, regarding the reticulate sculpture Nichols et al. (1973) point out that “*Arecipites* includes tectate forms, some with fine pitted sculpture on the tectum, but not forms with true reticulate sculpture”. Reticulate and monosulcate pollen grains can belong either to *Liliacidites* or *Monocolpopollenites*, which are not strictly related to Areaceae (palms). In our study we have followed the well-structured definition of Nichols et al. (1973) for the genus *Arecipites* that is connected to the family Arecaceae (palms). We understand that sometimes it may be difficult to determine in some pollen grains whether they have narrow endings of the sulcus or not and to differentiate the exine structure. Therefore, whenever it was not possible to differentiate these taxonomical features, we did not use the term *Arecipites* for the respective pollen grains.

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p. 304, ln. 3: “this sentence needs rewording.” In compliance with this suggestion of R1, we have reworded this sentence (see line 354 in revised version of manuscript).

p. 305, ln. 1: “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?” The sporomorphs were transported offshore both through aquatic and eolian transport. With regard to both factors, Tasmania is the landmass located closest to ODP Site 1172; sporomorphs transported by the Tasman current from Antarctica would have required a surface-water transport of nearly 2000 km. With regard to the currents, these were reconstructed using numerical models with Eocene boundary conditions with regards to tectonic plate configuration, paleotopography and bathymetry, atmospheric CO₂ concentrations. The result for the Southern Pacific Ocean is a clockwise circulating gyre, and a strong western boundary current bringing surface water northward along the east Australina Margin (e.g., Huber et al., 2004). This ‘theoretical’ paleoceanographic configuration is well supported by microfossil-derived paleobiogeographic patterns, notably of dinoflagellate cysts (Bijl et al., 2011; 2013). The combination of field-based and physics-based evidence provides a robust support for this paleoceanographic reconstruction. When discussing the point made by R1, we realized that the original version of Fig. 1 did not clearly show that the East Tasman Plateau was located in a shallow-water area (<300m). We therefore have slightly modified this figure such that it now gives an indication of the paleo-waterdepth on the ETP during the studied interval.

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”. We agree with the suggestion made by R1, and we have adjusted Fig. 3 accordingly.

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