

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

The study of Contreras et al. concerns a rare record of vegetation and climate change of a very interesting period of the Neogene. It helps to understand the climatic developments in the Paleocene/Eocene during the hothouse period of the Cenozoic. The MS reads well, it is clear and concise. The supplementary plates are very beautiful and

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I suggest incorporating them into the main text.

Of course, there is the question – also raised by Dr. Harrington – about the applicability of the nearest living relative (NLR) method to periods as old as the Paleogene. I fully agree that the quantification and the problems that go with it should be better discussed. Meanwhile, the authors already did so in their new version given as supplement to their answer to Dr. Harrington.

I think the study suits *Climate of the Past*. Two important shortcomings have already been remedied in the new version. Only the conclusion that TEX-temperatures are likely biased towards summer conditions is, to my mind, not warranted in the light of the systematic uncertainties in the sporomorph-derived temperatures. Apart from this some minor points, listed below, still need attention.

I do not apologise for this late comment, because I was asked to review 4 weeks ago.

SPECIFIC COMMENTS

In the abstract and throughout the paper three vegetation zones are mentioned where actually there are four: the middle and the late Paleocene periods are separated by a period with a substantially cooler climate and different vegetation. This is somewhat confusing and I suggest that, especially in the abstract, the authors specify four periods (of which two have similar climate and vegetation). Please consider, if the use of stratigraphic names (Selandian and Thanetian) would help.

The terminology of TEXL86 and TEXH86 concerns different calibration curves that are too specialised for an abstract. I think, mentioning that the temperature estimates are TEX-based would suffice. Please explain the differences between the two calibrations in the introduction or in Section 4.4, where they are compared with the palynological data.

Please shift Section 2.1 “Regional setting and paleoceanography” into the “Introduction” as section 1.2; it does not belong in “Material and methods”.

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I assume that the reanalysis mentioned in section 2.3 means that the pollen and spores are counted from samples prepared for dinoflagellate cyst analysis, which would account for the sieving through a large mesh of $15\mu\text{m}$. Please be aware, that you may have lost in this procedure the smaller pollen grains that are so typical for tropical forests (see also the comment of Dr. Harrington). For instance, you might have lost Ilex-type pollen grains, especially if you have used ultrasonic treatment during sieving. This is something you have to take into account in the discussion of the Eocene assemblages.

Please indicate in what medium the slides are stored – glycerine jelly or silicon oil or whatever you use.

I am happy to read that you used rarefaction to evaluate sporomorph diversity - good!

I had problems understanding the statistical procedures. However, this point has already been remedied in the new version.

Section 3.1. The low sporomorph taxon numbers during the PETM might partly be due to the sieving over $15\mu\text{m}$ if the vegetation changed to a tropical forest producing smaller pollen grains.

Please, specify the amount of variance explained (eigenvalues of DCA1 and DCA2) by the DCA.

Please shift Section 3.2 to the discussion. At the end of Section 2.2 you write quite rightly that you'll DISCUSS the issue of pollen and spore source area.

To the comparison of the DCA-scores with the TEX-derived temperatures, one might keep in mind that somewhat of a flora turn-over took place at the PETM, which is well seen in the DCA, but not necessarily in the SST.

In Section 4.2.2 you argue that the sporomorph-temperatures might be biased during the Eocene because of longer marine transport routes. On top of that comes the missing of small pollen grains resulting from the method used, which also might bias

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the sporomorph-temperatures to lower values. However, in Section 4.4 you argue that it is the TEX-temperatures that are too high. That might be, but I think the argument is weak.

Table 3. Because of the large uncertainties in the NLR-method and the resulting large error bars in the given temperature estimates, I flatly refuse to believe the small standard deviations given in Table 3. If you just gave the averaged sample values with the standard deviation per period, then you did not execute error propagation.

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

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