

## ***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 response to the comments of Referee 2 (R2; Dr. Lydie Dupont). To alleviate the tracing of the improvements made in the manuscript, we have highlighted all changes in yellow (please see supplement to this comment).

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**R2 states that our manuscript “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.”** We appreciate R2’s positive assessment of our manuscript. In the following, we discuss the general and specific comments made by R2.

**R2 indicates: “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.”** We appreciate that R2 is satisfied with our detailed response to R1’s comments and our adding of a new paragraph on this issue as already contained in the previous revised manuscript (see lines 255 – 270 of our revised manuscript; supplement to this comment).

**R2 states: “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”.** We agree with R2 that our data may not unambiguously warrant such a conclusion, although we note that such a potential bias is interpreted from many temperature proxy intercomparison studies (e.g., Zachos et al., 2006; Hollis et al., 2012; Bijl et al., 2013a; Pancost et al., 2013). However, to enhance clarity for the readers and to alleviate R2’s concern, we have modified the revised manuscript such that we now indicate that TEX86 based SSTs are likely biased towards warm conditions when applied to early Paleogene records, particularly from the southern latitudes; in addition, we now further cite papers that have already come up with such an interpretation before (see lines 793 – 796 of our revised manuscript).

**R2 indicates that “In the abstract and throughout the paper three vegetation zones are mentioned where actually there are four: the middle and the late Pale-**

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ocene 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).” This view of R2 appears to be based on a misunderstanding – we find are indeed THREE (as already stated in our previous manuscript version) rather than FOUR (as claimed by R2) distinctly different vegetation types on Tasmania during the middle Paleocene to early Eocene. These are: (i) warm-temperate forests dominated by gymnosperms that were dominant during the middle and late Paleocene (excluding the middle/late Paleocene transition); (ii) cool-temperate forests dominated by southern beech (*Nothofagus*) and araucarians that transiently prevailed across the middle/late Paleocene transition interval ( 59.5 to 59.0 Ma); and (iii) paratropical forests rich in ferns that were established during and in the wake of the Paleocene–Eocene Thermal Maximum (PETM). However, to maximize clarity on these matters, we have added explanatory statements where appropriate in the revised version of our manuscript (see lines 39 – 41 and 318 – 320 of our revised manuscript).

**R2 suggests to “Please consider, if the use of stratigraphic names (Selandian and Thanetian) would help.”** We had discussed this option when furnishing our manuscript, but ultimately have decided not to do so because these names are rarely used in the southern high latitudes.

**R2 states that “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.”** To comply with the point made by R2, we have added an additional reference (i.e., Kim et al., 2010) in the introduction of the revised version of our manuscript (see lines 134 – 135) that readers interested in the differences between the two calibrations can easily access for further information. With regard to the abstract, we mention the two calibrations within this section to point

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out the potential warm bias in both calibrations of TEX86, which is an important and novel conclusion of our study. Therefore we have decided to maintain the mentioning of both calibrations, similar to other studies from the region (Hollis et al., 2012, 2014; Bijl et al., 2013a, 2013b; Pancost et al., 2013). We hope this reasoning finds the approval of R2.

**R2 suggests to “please shift Section 2.1 “Regional setting and paleoceanography” into the “Introduction” as section 1.2; it does not belong in “Material and methods”.** In agreement with the suggestion of R2 we have transferred this section into the Introduction as Section 1.2.

**R1 states that “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 mic. 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”.** As already discussed in our reply to R1’s review, based on our previous experience on early Paleogene sporomorph records from the high southern latitudes, only very few pollen taxa have a size <15  $\mu\text{m}$ . These are *Myrtaceidites parvus* (botanical affinity: cf. *Leptospermum* and some other Myrtaceae) and *Triorites minor/minisculus* (unknown affinity). These two pollen taxa are the only ones that have been noticed to be reduced by sieving at 15  $\mu\text{m}$  (J.I. Raine, pers. comm., 2014). Sporomorph taxa that typically represent tropical and subtropical settings during the early Paleogene in the high southern latitudes are: *Arecipites* (palms), *Spinizonocolpites* (*Nypa*), *Bombacidites* (Bombacoideae), *Cupanieidites* (Cupanieae), *Anacolosidites luteoides* (*Anacolosia*), *Santalumidites cainozoicus* (*Santalum*), *Beaupreaidites* (*Beauprea*), and *Bluffopollis scabratus* (*Strasburgeria*). All these sporomorph taxa consistently have sizes >15 $\mu\text{m}$  (e.g.,

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Stover and Partridge, 1973, and references therein), which implies that their representation in palynological residues is not reduced by sieving at 15  $\mu\text{m}$ . *Myrtacidites parvus* is nested within the family Myrtaceae, which today has a wide distribution from tropical to cool temperate settings (Wilson, 2011). Hence, the mesh width used in our processing can be excluded to have caused a reduction in pollen taxa from plants that are indicative of tropical to subtropical settings in the high southern latitudes during the early Paleogene. With regard to *llex*, we agree with R2 that some *llex*-type pollen might have been lost using the ultrasonic bath. However, we would like to note that a number of different studies have shown the recovery of *llex*-type pollen after using an ultrasonic bath during the sieving process (e.g., Luo et al., 2005; YunLi and S., 2012).

**R2 suggests to “Please indicate in what medium the slides are stored – glycerine jelly or silicon oil or whatever you use.”** Following this suggestion by R2, we have inserted a new sentence in our revised manuscript indicating the mounting medium (see lines 203 – 205 of our revised manuscript).

**R2 points that “the low sporomorph taxon numbers during the PETM might partly be due to the sieving over 15 mic. if the vegetation changed to a tropical forest producing smaller pollen grains.”** As outlined above, pollen derived from thermophilous taxa that thrived in the high southern latitudes during the Paleogene consistently have a size  $>15 \mu\text{m}$ . Moreover, as already discussed in our submitted manuscript, the palynological record for the PETM from the Tawanui section, which is based on samples processed through a 10  $\mu\text{m}$  mesh (Crouch and Visscher, 2003), yields similar results as our study (see lines 18-23, page 314 of the published version of the manuscript, CPD 10, 291-340, 2014). Hence, this leads us to believe that our PETM results exhibit a taphonomical bias due to the high sea level during that time rather than a bias related to the mesh width used in the preparation process.

**R2 suggests to “please specify the amount of variance explained (eigenvalues of DCA1 and DCA2) by the DCA”.** Following R2's suggestion, we have specified the amount of variance explained by the DCA in the figure caption of Fig. 3 (see lines 893

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and 896 of the revised version of our manuscript). We do not further discuss these results in the text because DCA is not a method for decomposition of variation, and therefore these proportions would not make sense (e.g., Oksanen et al., 2011).

**R2 suggests to “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.”** Following the suggestion of R2, we have shifted Section 3.2 to the Discussion.

**R2 suggest that “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.”** We are admittedly not sure if we fully understand this point made by R2. As shown in Figure 4, the higher SST values during the PETM are directly connected to the floral turnover detected through the DCA Axis 1 scores results.

**R2 states that “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 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”.** As stated above, the potential bias on the sporomorph assemblages during the early Eocene as well during the PETM appears not to be related to the mesh width used in the palynological processing; instead, it is most probably connected to a taphonomical bias. With regard to the discussion made by R2 related to TEX86-based temperatures, we respectfully disagree with follow R2's view. Based on the TEX86-derived SSTs presented in our study, the higher SSTs values are recorded during the early Eocene. This pattern is not evident in the sporomorph-based temperatures, and we argue that the early Eocene sporomorph record is probably biased during this time interval due to a sea-level rise; this sea-level rise increases the shoreline distance of our study site, favouring the pres-

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ence of the more easily transported grains over those that are less easily transported (see page 318, lines 5 – 20, of the published version of the manuscript, CPD 10, 291-340, 2014). From the proxy side, evidence for a warm bias comes from SST proxy intercomparison studies on Paleocene-Eocene sediments. They show consistently that TEX86 (and particularly TEX86H) suggests the warmest SSTs (Zachos et al., 2006; Hollis et al., 2012). This suggests an overall bias in this proxy towards higher SSTs, which we here tend to relate to a warm (summer) bias of the proxy (see lines 791 – 796 of the revised version of our manuscript).

**R2 states “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.”** We fully agree with the point raised by R2. Accordingly, we have modified Table 3 including weighted averages and their corresponding propagated errors (see also the revised version of the Supplementary Information for further information of the equations used for these calculations). In the revised version of our manuscript we now also present the range of the estimated climatic parameters in Section 3.2. When discussing this point, we realized that we had erroneously not supplied the calibration errors of the TEX<sub>86</sub> data presented in our study, which are required when absolute SSTs are inferred (e.g., Bijl et al., 2013a). We therefore have supplied the corresponding calibration errors in the revised caption of Table 3.

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Please also note the supplement to this comment:

<http://www.clim-past-discuss.net/10/C618/2014/cpd-10-C618-2014-supplement.zip>

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