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Climate of the Past Discussions

Interactive comment on "The early Eocene equable climate problem revisited" *by* M. Huber and R. Caballero

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I was made aware of this manuscript from a reviewer for a paper I have at Earth System Dynamics. I suppose this proves the value of the open review process! Overall, this manuscript is well-reasoned and should become a standard reference on this topic. I am encouraged that the upshot of the manuscript, that CO2 was probably either very high or climate sensitivity was high, is concordant with a growing number of proxy-based studies that also find high climate sensitivity during the early Cenozoic (Hansen et al., 2008; Pagani et al., 2006; Higgins & Shrag, 2006; Royer 2010 PNAS).

I am concerned with how the authors treat some aspects of the plant-based proxies. For the leaf physiognomy method for inferring paleotemperature, the authors elect to use the Kowalski & Dilcher (2003) equation. This equation is probably not robust.

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Interactive Discussion

Discussion Paper

Peppe et al (2011, New Phytologist , http://onlinelibrary.wiley.com/doi/10.1111/j.1469-8137.2010.03615.x/abstract) show that if a larger and more representative sample of edaphically-wet samples are included, the resultant equation is very similar to standard global equations that most people use (see right column on p.8 in their paper). Most troublesome, Kowalski and Dilcher do not include warm floras in their calibration, so use of the equation to reconstruct warm MATs is a dangerous example of extrapolation. Peppe et al (2011) find that the 'wet' effect is at most 4 oC, not upwards of 10 oC as Kowalski and Dilcher suggest for warm sites.

Second, the authors largely dismiss stomatal-based CO2 reconstructions when CO2 is high. This is because stomatal distributions cease to be very sensitive to CO2 when CO2 is above $\sim\sim$1000 ppm. This is a fair criticism. And the Smith et al. (2009) study is an excellent example of this problem. However, it is fair to ask, was CO2 above 1000 ppm during the entire early Eocene? Stomata are quite sensitive when CO2 is under 1000 ppm, and there is a fair bit of stomatal data (including some of my own) that are in this 'sensitive zone', and thus the reconstructions should be robust. In other words, not all stomatal data from the early Eocene are in the insensitive region of the calibration. I would also suggest that our knowledge of Cenozoic CO2 isn't as poor as the authors imply. Improvements in the methods have led to less variability across methods for the early Eocene ($\sim$two fold variation, as opposed to an order of magnitude variation seen a decade ago). Estimates from long-term carbon cycle models agree with the proxies too. The compilation of Doria et al (2011 AJS; I sent this paper to Matt a few weeks ago) shows this concordance nicely. This requires, however, discarding some earlier CO2 records that are now not considered robust (e.g., Pearson and Palmer, 2000, which the authors unfortunately cite in their manuscript).

Interactive comment on Clim. Past Discuss., 7, 241, 2011.