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Climate of the Past Discussions

Interactive comment on "Radiative forcing by forest and subsequent feedbacks in the early Eocene climate" *by* U. Port et al.

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I am reviewing "Radiative forcing by forest and subsequent feedbacks in the early Eocene climate", by Port et al. This is a model sensitivity study exploring Eocene forest changes with a substantial feedback analysis that builds on the prior work of Heinemann et al., 2009. The sensitivity bounds are very gross, a desert world versus a forested world (like Tatooine vs Endor, but with oceans). I am not sure what we are supposed to learn about actual Eocene feedbacks from an analysis like this, but I guess it puts a (model-dependent) upper bound on how important land surface biophysical feedbacks might have been. 5 w/mˆ2 forcing is pretty big, but not at all surprising given the huge change in imposed albedo. The rest of the feedback analysis is well thought out and thorough, but it is not clear to me what we learn from it, given that the results are state-dependent and model dependent.

In general I think the figures and methods and writing up are to this journals excellent standards. The model is fitted to the task and the gregory style analysis is well applied here, although both this technique and a better one (PRP) were applied previously in Caballero and Huber (2013). The results are convincing within the framework which they were generated in. My main suggestions are to improve the framing of the paper and to demonstrate that there is an actual scientific question being answered here (or even asked) and also that more care be taken to make it clear that the results make sense only within the context of the model that created them. Right now I am left asking myself, have we learned anything about the Eocene from this analysis?

I have some suggestions for improving the paper. The coverage of the prior literature and the context which it provides is rather poor. Otto-bliesner and Upchurch's [1997] pioneering study should be acknowledged even though it was Cretaceous. Sewall et al., 2000 is mentioned, but the fact that this showed that plausible changes in Eocene vegetation had little impact on broad scale climate features was glossed over. That study was not in a coupled model, but the resulting vegetation was used in Huber and Sloan, 2001 (also not cited) which showed that accurate reconstructions of Eocene vegetation distributions did not change climate much from modern. Shellito and Sloan 2006a,b which investigated the a variety of dynamic vegetation model simulations for the Eocene is ignored, which is unfortunate since such model provides some context for plausible changes in vegetation. Lopston et al. 2014 also investigate the sensitivity of climate to vegetation changes in the Eocene and is also ignored. Lunt et al., 2012 which applied the same feedback analysis used here to a multi-model ensemble is not discussed–this is strange given how important much the results presented here are likely to be model dependent. Goldner et al., 2013, 2014 are also omitted even those papers are explicitly about how changes in albedo affect global climate (including the issue of state dependence). Caballero and Huber 2013 also show approximately a 4C difference in temperature between modern and Eocene boundary conditions (at constant CO2), some fraction of which has to be explained by land surface boundary condition changes, but that study was not able to tease those factors apartâĂŢthis

study could contribute to understanding that result.

The lack of a ground-truthing of the model by comparison with proxy data is hard to justify given that one of the main conclusions is that the results are state-dependent. If the simulations here are close to those of Heinemann presented in Lunt et al., 2012 then the simulated climate is not a good fit for the early Eocene and probably substantially too cold at high latitudes. Is there snow on those forests? It would be useful to compare the model against the terrestrial paleotemperature data presented in a compilation such as in Huber and Caballero, 2011 to at least place the model within some sort of context.

More generally, much of the tone of the paper should be changed. The authors are talking about one particular model and most of their results are likely to be very sensitive to the model choice (as well as the fact that their basic state appears to be too cold). For example, in the conclusions: "Negative cloud-related feedbacks, however, are also stronger and outweigh the stronger positive lapse-rate and water-vapour feedback. In the sum, climate feedbacks stabilise the early Eocene climate stronger than the current climate."

Well, what you mean is that in this particular model cloud feedbacks stabilize the climate more. But clouds are notoriously uncertain and model dependent.

Similarly, "Polar amplification is still weak in the early Eocene climate due to weak sea-ice related feedbacks." But, there was no sea ice in the Eocene, yet there is good evidence for polar amplification. That suggests several things to me. First, in this model and in these simulations most polar amplification is due to sea ice. Other models have strong polar amplification without sea ice (Caballero and Huber, 2013; Lunt et al., 2012; Huber and Caballero, 2011), so this seems to be a particular weakness of this model. Second, it suggests at the least that this is an area of definite model dependence. Third, it suggests that if this model was run at (presumably higher CO2) conditions that were closer to what they were in the Eocene then the model would have a potentially

different response (since some of the response might be due to high latitude conditions below freezing that are precluded by the data).

Interactive comment on Clim. Past Discuss., 11, 997, 2015.