

Interactive comment on “A new interpretation of the two-step $\delta^{18}\text{O}$ signal at the Eocene-Oligocene boundary” by M. Tigchelaar et al.

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Paper: A new interpretation of the two-step $\delta^{18}\text{O}$ signal at the Eocene-Oligocene boundary

Authors: Tigchelaar et al.

Journal: Climate of the Past Discussions

Reviewer: Dorian S. Abbot

Overview: This is a nice study that demonstrates a creative idea for the Eocene-Oligocene $\delta^{18}\text{O}$ transition. My only major general comment is that I think the authors

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are a bit overly enthusiastic in their comparison of model results with data. Since this is really a conceptual model, it is misleading to compare quantitatively model temperature and CO₂ values to data. It also opens you up for attack if you allow the debate to be pushed toward specific comparisons because there are always some data you won't be able to explain. I outline more specific thoughts and comments for the authors below.

Comments:

1. **Modes of circulation:** The authors claim that there is strong evidence for a switch in the MOC from an SPP pattern to a TH pattern at the start of the Oligocene. How confident are the authors that the data really do suggest this? I have not read the cited papers and do not have time to right now, but I ask because interpretations of the type of data mentioned can be very difficult. Perhaps something changed, but must we except this exact story? Is it possible that the switch was from the NPP pattern to the TH pattern? I ask because to me, Fig. 5 looks like a much better representation of Eocene climate at the surface (see comment 2 by Dr. Coxall, for example). I think the rest of the authors' story would be the same if the switch were from NPP to TH rather than SPP to TH.

2. **Eocene Climate:** As the authors note, their Eocene climate does not look much like the climate of the Eocene. One issue may be my first comment. Either way it might help to add a statement like, "It is not surprising that our model is not in exact agreement with the proxy data for the Eocene, since it is a conceptual model that does not include such potentially important effects for explaining Eocene climate as increased polar stratospheric cloud formation (Sloan et al., 1992), the effects of hurricanes on ocean circulation (Emanuel, 2002), and the formation of tropospheric convective clouds in the Arctic (Abbot and Tziperman, 2008)."

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3. **CO₂**: The authors spend a fair amount of time comparing the CO₂ at which things happen in their model to estimates of Pagani et al. (2005). I do not think this is terribly useful because of the huge uncertainties associated with the estimates of Pagani et al. (2005) and the simplistic representation of radiative transfer in the model. I think it would be better to admit that the model “CO₂” should really be thought of as rough changes in radiative forcing, and that the data, though highly uncertain, tend to indicate a decrease in CO₂ around this time, and that this makes sense given other evidence. This justifies the basic exercise of the paper (decreasing CO₂ to make things happen) and avoids confusing people by making such specific comparisons between two things that are so uncertain.

4. **Sea Ice**: The authors disable sea ice in the model because they feel that the sea ice-albedo feedback is overactive. What is the ice albedo in the model? What is the ocean albedo? Maybe the problem is that the contrast between them is too high. The reason this is worth thinking about is that the NP temperature is at freezing (or below?) in the SPP state (Fig. 2). There are already issues with this state not really being a reasonable representation of qualitative features of Eocene climate (which is why I like NPP better), but if sea ice were allowed to form, the SPP state would really look nothing like the Eocene (which is presumably the real reason sea ice is turned off!). If the authors decide to keep sea ice turned off then I suggest they admit that part of the reason they do this is that otherwise their Eocene state looks really wacky. I also suggest they remove sea ice from their schematic in Fig. 1b, since it's misleading to include a process in such a diagram that's not actually in the model.

References

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