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Climate of the Past Discussions

# *Interactive comment on* "Atmospheric circulation and hydroclimate impacts of alternative warming scenarios for the Eocene" *by* Henrik Carlson and Rodrigo Caballero

**Anonymous Referee #1**

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General comments

Carlson and Caballero address an important and relevant question: Could changes in cloud properties have contributed to Eocene warming, in ways that might be reflected in the proxy record? The paper focuses on analyzing a new simulation where changes in cloud radiative properties are altered from a more conventional Eocene forcing scenario, and a smaller greenhouse gas forcing is needed to achieve the same change in global-mean temperature change.

The paper makes an interesting contribution to the literature. The question and analysis are well within the scope of Climate of the Past. The abstract provides a concise and complete summary. The authors thoroughly reference existing literature and ap-



propriately highlight their contribution. The presentation is well-structured and clear, as is the language, and the wording is sufficiently precise.

The paper shows that the greenhouse-gas and cloud driven mechanisms of Eocene warming have similar temperature changes (both of which are consistent with the proxy record), but differ in their hydroclimate and circulation changes. The implication is that proxies of circulation and hydroclimate would be able to differentiate between these two forcing mechanisms.

The case for how hydroclimate and circulation proxies could differentiate the two warming scenarios is not completely clear-cut. In addition to the caveats already addressed in the concluding section, another important caveat is that this study is based on only one model. Different climate models have different biases, which are often larger for hydroclimate and circulation than they are for temperature. Also, the changes in cloud effective radius employed here are probably too large to be realistic (according to the discussion of Kiehl and Shields 2013). Another concerning feature of the simulation is that the radiative properties of clouds are altered but the microphysical properties are not, so in a sense the simulation is not completely self-consistent. This is particularly important because the conclusions are largely focused on hydroclimate, which might be affected by changes in microphysical properties of clouds. This lack of change in cloud microphysics is not described in the methods section (where it should be mentioned), it is discussed extensively in the conclusions.

In light of the implications for differentiating forcing mechanisms from the proxy record, a more extensive discussion of potential proxy records for regional climate and hydroclimate would be warranted (the beginnings of a discussion is included at the end of section 6). Is there existing relevant proxy evidence? Are there existing methods with which new records might be identified in new locations, or would new reconstruction methods need to be developed? Is it likely that useful records could be found over land, and if so in which regions? Or would useful records need to be identified in the ocean?

The title of the paper could be improved in two very minor ways. The first is that the analysis focuses on just one alternative mechanism for Eocene warming, but the title implies there are multiple. Second, hydroclimate seems to be a bigger focus than circulation, and is also is a more likely candidate for the focus of new proxy evidence than circulation. The order of the terms might be switched to reflect this.

The quality of the figures is generally adequate. Given the focus, it would be useful to see absolute fields for things like precipitation and aridity in the LCTC simulation, rather than just their differences from the Control case. This could be accomplished by showing the LCTC absolute fields rather than the control fields, but it would also be useful to have both in addition to the differences. Some of these could be included as a supplement. If one figure were to be omitted, I would suggest Figure 6. Its discussion is already limited in the text. In Figure 7, the caption states that fields are shown over land only, but panels a and c seem to have some faint signal over the ocean; it would be worth removing this, otherwise it gives the impression that changes over ocean are included and are uniform but small.

Specific comments

Page 3 Line 3: A more descriptive name than “control” might be devised for the greenhouse-gas driven Eocene scenario. More descriptive names for both scenarios might be “GHG” and “Cloud.”

Line 4-5: Rather than cloud droplet radius, the relevant variables is the effective cloud droplet radius. It represents a weighted average over the distribution of cloud droplets.

Page 4 Line 5-8: Just how similar is the temperature for the two simulations? You might quantify this as the maximum difference in zonal-mean temperature. Figure 2b makes it seem like they could be non-trivial.

Page 5 Line 10-15: The jet streams vary among climate models and between models and observations in the present-day climate, and these variations are related to



changes in the jet with climate (see Barnes and Polvani 2013 for the differences across models). The appropriate comparison with present-day jet streams would be to CAM3 with modern boundary conditions and forcings, whereas Shaw et al (2016) focus on reanalysis data.

Line 21: Caution should be exercised in interpreting the MJO in coarse resolution climate models. There are many aspects of it that they are not able to represent with particularly good fidelity (e.g. Hung et al 2013).

Page 6 Line 19-23: It seems to me that 6 out of 17 Wmˆ-2 contributed by changes in cloud radiative effect is an important fraction of the total change in atmospheric radiative cooling.

Line 23: Rather than “cloud abundance,” the cloud radiative effect might change due to the changes in cloud properties that are specified (effective cloud droplet radius).

Page 8 Line 6-7: Why is Fig 5d noisier than Fig 2d?

Minor comments Page 4 Lines 5,12: Figures 1 and 2 are shown in a different order from their reference in the text.

Line 22: “cyclones” should be “anticyclones”

Line 25: “essentially identical” is difficult to quantify, “very similar” might be more defensible

Page 5 Line 5: “cloud abundance” should be “cloud fraction”

Page 12 Line 24-26: The reference information for Carmichael et al (2015) seems to be out of date. The final published version of the paper came out in 2016 and has a different title.

Figure 3: I expected each color to balance across each panel, but they do not. The figure presentation might be a little bit clearer.

Additional references

Barnes, E. A., & Polvani, L. (2013). Response of the midlatitude jets, and of their variability, to increased greenhouse gases in the CMIP5 models. Journal of Climate, 26(18), 7117-7135.

Hung, M. P., Lin, J. L., Wang, W., Kim, D., Shinoda, T., & Weaver, S. J. (2013). MJO and convectively coupled equatorial waves simulated by CMIP5 climate models. Journal of Climate, 26(17), 6185-6214.

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