

Interactive comment on “Equilibrium state and sensitivity of the simulated middle-to-late Eocene climate” by Michiel Baatsen et al.

Anonymous Referee #1

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This manuscript presented simulations of the middle-to-late Eocene climate using the CESM1.0. The model resolution was $\sim 1\text{-}2^\circ$, higher than most of the previous Eocene simulations. A new set of geographical boundary conditions was employed. The authors claimed that their modeling results were in good agreement with proxy records. They also described many aspects of ocean/atmosphere states and circulations and climate sensitivity in the simulations.

Simulations in the study could potentially contribute to our understanding of the past warm worlds, but I do not find this manuscript in the current form satisfactory. To make this manuscript more accessible to readers, the writing should be improved greatly. Vague expressions should be avoided. To substantiate many of the authors' arguments, new analyses need to be conducted. Please see detailed comments below.

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Major comments: 1. The authors used many vague expressions in their manuscript. I only list a few here for illustrative purpose. I suggest the authors carefully go through the manuscript and improve the writing.

Line 285: "... in good agreement with the model."

Line 286–287: "A mixed agreement is seen at other latitudes, with model results being too warm in the northern middle latitudes, too cold in southern middle latitudes and good at high latitudes."

Line 300: "Good agreement between proxy and model results ..."

Line 442: "... agree fairly well ..."

Line 674: "... makes a fairly good match ..."

2. This is a long manuscript with 18 items (figures + tables). I would suggest the authors simplify the figures, shorten the results but highlight and provide in-depth analyses on the differences from previous modeling results. For example, in Figure 1-2, it is not very interesting to show the time series of individual basins (Pacific and Atlantic). In fact, this detailed information is not much discussed in the manuscript. I suggest the authors focus more on the relevant findings and major difference from previous simulations, to make this manuscript more interesting to read.

3. The authors need to do a better job describing their model and experimental setup, to ensure that results are reproducible by others. For example, CESM1.0.5 has active land and sea ice model, which the authors did not mention in the manuscript. Also, what values are used for the orbital parameters, solar constant, and other greenhouse gases (N₂O and CFCs)? How is the land surface configured? How are the lakes and rivers treated? What is the soil color in the Eocene simulations? How is the ocean mixing (including tidal dissipation) parameterized? I know this list is long, but it is essential for other researchers to reproduce and correctly interpret results in this study.

4. I find results on model-data comparison and analyses on climate sensitivity more

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interesting and relevant than descriptions of many detailed aspects of atmospheric and oceanic state/circulations. I think, when describing the modeling results, after a brief discussion of the model spin-up process, it is better to show modelled results compared with proxy records first. This essentially gives readers a general ideal about the model performance and its advantage/disadvantage compared with previous modeling results.

5. Aside from the analyses on climate sensitivity, this manuscript is highly descriptive with many statements requiring more in-depth analysis to substantiate.

Example 1: when explaining the temperature difference between the present study and Goldner et al. (2014), the authors listed a few possible factors (e.g., model resolution, dynamic core, radiative forcing from CH₄, aerosol, . . .) but fail to, at least try to, quantify contribution from any factor.

Example 2: the authors ascribed the different climate sensitivity between the Eocene climate and the present day to “fast feedback processes”. They failed to explain further what these fast feedback processes are? Are there other processes involved, like clouds, albedo and lapse rate? Have you quantified the feedback strength (e.g., using the partial radiative perturbation) to make the argument?

Example 3: Around Line 22–23, the authors stated that “. . . without the need for greenhouse gas concentrations much higher than proxy estimates”. What are the estimated ranges of CO₂ and CH₄ from reconstructions? How different are these reconstructions compared with values used in simulations in this study?

There are many other unsubstantiated statements all over the manuscript.

6. Using simplified equations in Etminan et al. (2016) for radiative forcing calculation in a 3D climate model is not justified. For example, the adjusted radiative forcing for a CO₂ doubling at preindustrial is 3.5 W m⁻² from off-line radiation calculation (Bitz et al., 2012), rather than 3.8 W m⁻² from equations in Etminan et al. (2016). It is not

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clear how much the difference could be under high CO₂ and CH₄ levels. The authors need either to calculate the radiative forcing from the off-line version of CESM radiation code, or to conduct slab ocean circulations to calculate and compare the equilibrium climate sensitivity.

Minor comments:

1. To distinguish model simulation from proxy reconstruction, I suggest the authors use the word “simulated”, when talking about simulation results, e.g., Line 15.
2. Line 4–5 & Line 660: Simulations with $\sim 2^\circ$ atmosphere and $\sim 1^\circ$ ocean are not high resolution, but IPCC-like model resolution.
3. Line 89–95: Those are details of model setup, sort of redundant with descriptions around Line 115. Please consider moving details about model setup to Section 2.
4. Line 129–130: “global mean, volume weighted ocean temperature”. Delete “average”
5. Line 205: In terms of an oxygen . . . ? CHANGE therms to terms
6. Line 240: . . . ocean eddies (Viebahn et al., 2016).
7. Line 367: 30C \rightarrow 30°C
8. Line 369–371: “Still, most of the lower estimates are best matched by the annual mean SST in the 38Ma 4x PIC case while also meeting the higher estimates when considering summer maxima.” Please rephrase.
9. Line 544: SST’s \rightarrow SSTs?

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