Clim. Past Discuss.,
https://doi.org/10.5194/cp-2020-29-AC2, 2020

Climate of the Past
Discussions

Interactive comment on "The middle-to-late Eocene greenhouse climate, modelled using the CESM 1.0.5" *by* Michiel Baatsen et al.

Michiel Baatsen et al.

m.l.j.baatsen@uu.nl

Received and published: 4 August 2020

RC: The purpose of this work was to study the Middle-to-Late Eocene climate using a coupled model. The Middle-to-Late Eocene represents a key period of the Cenozoic characterized by the demise of the greenhouse period. The manuscript is quite long but clearly written. Its structure is logical despite some overlapping between the sections (for instance between sections 2.6 and 3.4. Moreover, these two sections have the same title). The paper relies on a large number of figures: 10 figures in the main text and 16 figures in supplementary materials. Unfortunately, the authors used a colour scale that makes the figures difficult to interpret. In addition, the superimposition of shading and contours which does not help matters. The authors do not show differences (or very occasionally) between simulations which can be very helpful (with a

Printer-friendly version

Discussion paper

more classical colour scale). The authors should rewrite the abstract to better highlight the key results of this work.

AR: The authors want to thank the referee for this in-depth review. Both referees consistently seem to agree with the general findings and presentation of the results, pointing out that particularly the abstract needs to be re-written and improvements can be made on the conventions (regarding colours, scales and contours) used in some of the figures.

RC: Beyond this general comment, some points need to be clarified. My first comment concerns ice sheet. The authors simulate the Late Eocene climate using a pCO2 of 1120ppmv (4x) and 560ppm (2x). These values are classically used to study this period. However, the absence of ice sheet in Antarctica in the experiment at 560ppmv is more disputable. Indeed, the glaciation threshold is estimated between 560 and 920ppmv. A pCO2 as low as 560ppmv thus represents the lower limit for glaciation threshold. Moreover, it is clearly model dependent. In this work, the simulated mean annual temperature in Antarctica is below the freezing point (figure S6a), which may potentially represent required conditions for the onset of glaciation. Thus, how to be certain that an experiment without ice sheet and a pCO2 as low as 560ppmv is representative of the Priabonian period (when the CESM version 1 is used).

AR: The 560ppm experiment is indeed not a priori suitable to be carried out with a completely absent Antarctic Ice Sheet. The main reason to keep the boundary conditions consistent is to allow for a straightforward analysis of climate sensitivity under these conditions. From the results, it can actually be obtained that no ice would grow even at 560ppm but this can be pointed out beforehand to clarify the choices made (potential melt during the warm season still greatly outweighs any frozen precipitation from the cold season). Note that this does not mean that a climate with an AIS cannot exist at 560ppm, but the results shown here are still consistent with a largely ice-free Antarctica.

Printer-friendly version

Discussion paper

RC: The vegetation biomes for the Late Eocene experiments should be shown at the model resolution (Fig.1c). The cold mixed forest in the Andes seems to spread over Brazilian lowlands. The authors do not indicate how runoff was represented in the model.

AR: The vegetation biomes will be shown at model resolution along with those of the pre-industrial simulation in a supplementary figure. A brief discussion on the treatment of run-off will be added to the methods section.

RC: L135: It can useful to better explain how the CH_4 level in the Late Eocene experiments has been fixed.

AR: The choice to take 2x/4x pre-industrial CH_4 was based on the fact that these levels are at least as uncertain as those of CO_2. The range of values taken here are in agreement with what is suggested by Beerling et al. 2009. This will be better pointed out here.

RC: L163: The distribution of aerosols is calculated using the land surface properties. Can the authors be more precise?

AR: The aerosol distributions are determined using a bulk aerosol model and is consistent with the method used in earlier studies with CCSM3/4. This will be better explained, referring to the relevant literature.

RC: L190: A change in vegetation has been adjusted at the end of simulations causing a significant cooling at global scale. The explanation is not cleared. Which vegetation is shown in figure 1c?

AR: We discovered an issue with the translation of biomes into plant functional types, causing a mix-up between several types of forest. This mainly has an albedo effect due the possibility of snow on vegetation not being implemented correctly. Adjusting the vegetation thus led to a slight cooling along with an increased surface albedo. As this effects only temperatures near the surface over land, the model response to altered vegetation happens quickly. The biomes shown in Figure 1c are thus consistent with the corrected vegetation at the end of the simulations.

RC: L216: the acronym SST is used for the first time in the main text. Replace "SST" by sea surface temperature.

AR: This will be corrected.

RC: L245 and after (section 3.3): The authors compare their results with those of Goldner et al. (2014) and Hutchinson et al. (2018). What vegetation map were used in these two experiments? The authors argue that a lower global land fraction at Eocene induce a lower albedo and thus a global warming. The authors should estimate the changes in earth's albedo between pre-industrial and Eocene experiments. The simulations done by Hutchinson (H18) use the same paleogeography. The only difference is the model. The authors should better explore the impact of model version.

AR: The simulations of Hutchinson et al. 2018 used the same model geography and vegetation biomes, while Goldner et al. 2014 used an earlier version of the same model, a different model geography but a similar version as well. How the geography and vegetation are implemented in the models can be different, but the latter should be comparable. This will be mentioned here. An extensive overview of the radiative responses is shown in Table S1, this will be referred to more clearly. The aim here was not to provide a comprehensive comparison between all the available middle/late Eocene model studies, but rather put the results here into perspective. While we agree that it can be quite useful to do a much more in depth comparison, we would rather leave this out of the scope of this study. This motivation will be better clarified in the methods section.

RC: L370: "smaller but still considerable". The authors should estimate the changes in temperature.

AR: All of the related values are in Table 3, but this paragraph will be re-written to make it more consistent.

RC: L380: The annually averaged (daily) minimum temperature is plotted in figure 3a. The northeastern Siberia is concerned by temperatures below the freezing point (main text) which do not appear in figure 3a.

AR: Indeed, annually averaged minimum temperature is only <0 over Siberia in the 38Ma 2xPIC case. This will be corrected here.

RC: L387: The authors should indicate where the effects of orographic lift can be observed.

AR: A few examples will be added here; e.g. North/South American middle latitudes.

RC: L406-409: These two sentences seem to be redundant.

AR: The second part of this sentence can indeed be removed.

RC: L465: The paleogeography of Douglas et al. (2014) is different. The difference in latitude between Tasmania and the tip of Antarctica peninsula is about 5degC in Douglas' work but reaches 15degC in this study. Can it explain the difference of temperature?

AR: The main difference is that the palaeogeography used here includes the effect of true polar wander, shifting some of these gateways north or south by as much as 5deg. This can indeed explain to a large extent be explained by shifts in latitude, but are also partly the result of induced circulation changes. Some discussion can be added here.

RC: L467-472: How can the authors explain the absence of strong sub-polar gyre in the Ross Sea? Is it due to the paleogeography (Antarctica) or the depth of Tasmanian Gateway? The bathymetry, overturning regime and latitude of the Ross Sea all add to the gyre strength.

AR: The possible discrepancy with proxy indications is mentioned here, but not considered further as it is yet mostly unclear on how to interpret these proxies in terms of ocean circulation. A short discussion will be added here.

RC: L488: The authors should indicate in table S2 and S3 where the SST proxies are located (Gulf of Mexico, Blake Nose . . .).

AR: Not adding the site locations to the tables was a specific choice made to keep these reasonably compact. In addition, we will provide the original (MS Excel) files with a more complete overview of proxy site information.

RC: Minor comments: L45: reference missing : Toumoulin et al., 2020, Quantifying the effect of the Drake Passage opening on the Eocene Ocean, https://doi.org/10.1029/2020PA003889 L65: reference missing : Tardif et al., 2020, Clim. Past, https://doi.org/10.5194/cp-16- 847-2020

AR: We will add these references.

RC: Figure 1 caption: typo error (needleleaf) L347: typo error ('deg' is missing) L432 : typo error (Indo-Pacific)

AR: These will be corrected.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2020-29, 2020.