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Climate of the Past Discussions

Interactive comment on "Sensitivity of the Eocene Climate to CO_2 and Orbital Variability" *by* John S. Keery et al.

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This paper reports on an ensemble of 50 Eocene climate-model simulations, each of which characterized by a different combination of eccentricity, obliquity, precession and atmospheric CO2 concentration. The climate model is the PLASIM-GENIE model, a new model of intermediate complexity, recently introduced by Holden et al. (2016). The study aims to summarize the ensemble of paleoclimate simulations by looking at what-they-call *"simple metrics"*, principal component analysis and an emulator approach.

This study provides a couple of interesting results. The first is the existence of a sea-ice-related threshold mechanism in the northern hemispheric high latitudes. From Figure 2 and 3, it seems that when a certain threshold in the extent of DJF-sea-ice is

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Discussion paper

exceeded, temperatures (both sea-surface and maritime air temperatures) drop significantly. It would be interesting to read the author's opinion how this compares to the recent findings of modeling work by Zeebe et al. (2017), who found that *"High-latitude mechanisms are unlikely drivers of orbitally paced changes in the late Paleocene-early Eocene"*. The interesting role of (seasonal) sea-ice in the climate system of the early Eocene aspect remains, however, rather underdeveloped in the present version of the paper. The second interesting aspect is the distinct response to precession of monsoonal precipitation and temperature in the different monsoonal systems (e.g. Figure 6). The description and discussion of these Eocene paleoclimate simulations is useful and perfectly fits the scope of the journal. The current version of the manuscript is, however, unsatisfactory for publication in Climate of the Past for the reasons listed below.

Major Comments

1. One of the major conclusions in the current version of the manuscript, is that 95The emulator approach adopted in this study allows for estimating the response of different aspects of the climate system (e.g. wet-season monsoonal precipitation) over the full input space. It would -for example- be interesting to see the response of precipitation and temperatures in the different monsoonal systems to astronomical forcing for specific pCO2 levels. This could be an elegant way to circumvent the disparity in time-scales between CO2 and orbital variability.

2. The authors do not provide their 50-simulation experimental design. It is essential to have an overview of the parameter settings for each simulation that was run in the framework of this study. The details on the settings of the 50 simulations could be given either in the form of a Table, or in the form of a figure, or in both forms. For good examples, please check Figure 2 and Table 1 in Araya-Melo et al. (2015, cp-11-45-2015), Figure 2 and Table 2 in Lord et al. (2017, cp-2017-57), and Figure 1 in Bounceur et al. (2015, esd-6-205-2015).

3. From Figure 6, it is very clear that precession has an important influence on the Asian Monsoon intensity, with higher rainfall when the index is minimum (i.e. Earth in perihelion during JJA, maximum northern hemisphere summer insolation). However, if I interpret PC2 in JJA temperature and PC2 in JJA precipitation correctly (Table 5 and Figures 7 and 8), it seems that a precession-driven increase in monsoonal rainfall coincides with a decrease in JJA temperature in the Asian Monsoon region. Such a decrease in temperature is remarkable, given that it occurs when northern hemisphere JJA insolation is maximum. This observation can either be explained by the consumption of incoming solar radiation as latent heat, or by a negative influence of the increased cloud cover on the radiation balance. Indeed, the reflective character of clouds contributes to the planetary albedo. In the revised version of the manuscript, I would like to read more discussion of paleoclimate mechanisms like this one.

4. Page 7, lines 23-25 and Figure 6: When I was first interpreting Figure 6, I was confused by the fact that the Asian Monsoon and the African monsoon seemed to respond to precession in the same way, despite the fact that they are located on opposite sides of the equator. It took me quite a while to realize that both monsoonal systems are responding to precession in the expected way: with intensified wet-season precipitation in the Asian Monsoon system when the Earth reaches perihelion in JJA (negative precession index), and intensified wet-season precipitation in the African Monsoon system when the Earth reaches perihelion in DFF (positive precession index). I only understood this after reading lines 23-25 (page 7) several times. Indeed, the authors define their monsoon-related "simple scalar metric" by the difference in rainfall in DJF and JJA, regardless of whether DJF is the wet or the dry season in the monsoonal system considered. This also explains why the panel of Figure 6 that is related to the African Monsoon shows negative values, whereas the panel that is related to the Asian Monsoon exhibits positive values. I would strongly advise the authors to think about ways to illustrate the monsoonal response to precession in a more intuitive way. Maybe the paper by Tuenter et al (2003) could provide some inspiration as to how to best present the response of a summer monsoon to precessional (and obliquity?) forcing. Also, why

is the South American monsoon system missing from Figure 6?

Additional comments and recommendations

Abstract line 5 and p. 2 lines 1-3: I would recommend being a little bit more conservative on the possible analogy between the PETM and the ongoing anthropogenic disturbance of the global carbon cycle. Also cite Zeebe et al. (2016, Nature Geoscience) here.

Abstract: The abstract reads too technical and vague. I find the following sentence particularly vague: “Two dimensional model output fields are reduced to scalar values through simple summarizing algorithms and by singular value decomposition.” The reader gets very little information from this sentence. I would recommend rewriting the abstract, making it more results-oriented.

Page 2, line 30: suggestion: “The Earth resided in a greenhouse state”

Page 3, line 4: What do you mean with “high levels of radiative forcing”? Only eccentricity influences the total amount of solar energy received by the Earth. . . but the amplitude of that variability is only 0.15

Page 2, line 9: Either you provide the reader with information on which kind of evidence exists. Or you rewrite like: “During the PETM, the emission of organic carbon was initially in the form of methane, which later oxidized to CO2”.

Page 2, line 23: “broadly similar” is quite a subjective, interpretative qualification. I find the Eocene paleogeography quite different from todays, given that the Tethys Ocean was still open. If you want to point to the similarity with the present-day, you could state that the majority of the continents were located in the northern hemisphere.

Page 4, line 10 and many other occurrences: “dominant periods of 100 kyr and 405 kyr”. In an eccentricity power spectrum there are 4 peaks around 100 kyr, but only a single one at 405 kyr. Therefore, I would suggest the above notation.

Page 4, line 16: Jacques Laskar does not calculate time scales. He calculates astronomical solutions.

Page 5: Why is Section 3 not a subsection of Section 4 “Methods”?

Page 5, line 3: What is “T21”?

Page 6, lines 9-11: An injection of carbon into the atmosphere is measured in tons of C, whereas the concentration of CO2 in the atmosphere is measured in ppm. These are thus two different things, with two different units. You have to rephrase this sentence to correct for that.

Page 6, lines 13-16: It’s not immediately clear to me how knowledge on the phase relationship between carbon isotope excursions and the astronomical parameters would influence the experimental design of your study. If you would know these phase relationships, would you then have designed your experiments differently?

Page 6, line 26: What do you mean with “quasi-steady state”?

Page 7, line 7-8: The atmospheric circulation patterns during the Eocene were most definitely different from those in the modern world. I think you can remove the “are likely to”.

Page 7 line 27: Spell out SVD

Page 8 line 9: Please provide the appropriate references where these criteria are defined.

Page 8 lines 23-24: The Figure 3 that you are referring to, only contains global annual mean SST’s, not the Arctic winter SST’s you are discussing.

Page 9, line 1: It is unclear to me what exactly you mean with “parametric uncertainty”

Page 10, line 17: JJA instead of JJF.

Page 10, line 15: Shouldn’t this be Table 4?

The paper contains a few important shortcomings when it comes to appropriately referencing pre-existing work.

For example, the authors do not refer to the Deep-time Model Intercomparison Project (Deep-MIP, Lunt et al., 2017, gmd-10-889-2017). The authors do not frame their study within that project, nor do they differentiate their study from that project. A statement on this topic is indiscernible, given that both this study and the Deep-MIP project explicitly focus on simulating (early) Eocene warm climates and that both are using the same paleogeographic configuration from Herold et al. (2014).

The authors refer to Bounceur et al. (2015), who applied a “similar emulator approach” (p. 8 line 13). First of all, I am unsure whether that statement is technically correct. Secondly, this reference is missing from the reference list.

On page 4, line 28, the authors give credit to Ruddiman (2006, cp-2-43-2006) for noting “a relationship between obliquity and the extent of northern ice sheets”. First of all, this is a Pleistocene-focused paper, of which I don’t really see the relevance when discussing orbital configurations during the Eocene and possible influence on climate. Moreover, the relationship between obliquity-induced minima in NH summer insolation and ice age cycles was already suggested by Milutin Milankovitch in 1941.

References

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Herold, N., Buzan, J., Seton, M., Goldner, A., Green, J. A. M., Müller, R. D., Markwick, P., and Huber, M.: A suite of early Eocene (âĹij 55 Ma) climate model boundary conditions, Geosci. Model Dev., 7, 2077–2090, doi:10.5194/gmd-7-2077-2014, 2014.

Tuenter, Erik, et al. "The response of the African summer monsoon to remote and local forcing due to precession and obliquity." Global and Planetary Change 36.4 (2003):

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