Review on the manuscript (cp-2018-56) entitled "An Energy Balance Model for Paleoclimate Transitions" by Brady Dortmans et al.

This paper aims to revisit some of the major climate transitions of Mesozoic and Cenozoic using a new EBM model and to study bifurcations. Specifically the authors apply the EBM for mid-Cretaceous, early Eocene, Eocene/Oligocene (E/O) transition and Pliocene.

In a first part the authors introduce the "paradoxes" they intend to solve using new EBM. Then, in a second part, they describe the novelty of this new model and finally, in a third section, they apply this tool to different climatic transitions.

The paper is well written and tackles an interesting topic but, from my point of view, suffers from many caveats that I will describe below.

Major comments

1. Presentation of the climate paradoxes

Concerning the use of a large hierarchy of models to investigate different climatic transitions, I fully agree that this approach is necessary, mainly because sophisticated AOGCM can only investigate a few numbers of trajectories whereas EBM and EMIC can provide a large number of experiments and investigate a larger range of possible trajectories. This has been illustrated for instance by Roche el al., Nature, 2004 and Claussen et al., GRL, 1999.

The authors continue a long lasting history with pioneering studies of Budyko, Sellers and more recently Paillard et al., Nature, 1998, with more conceptual models, Ganopolski et al., Nature, 2001 using more complex EMIC and Stap et al., Climate of the Past, 2017 with an EBM, for instance.

My major criticism concerns the real added value of this study and the authors should more clearly pinpoint which "paradoxes" they solve with respect to the abundant literature already published on this key transition. Moreover, the bibliography concerning each period has to be updated. Since Crowley and Barron pioneering studies, in the nineties, much progress has been done with the large hierarchy of models which doesn't appear in this paper. Indeed, the transitions that the authors tackled in this paper and depicted as paradoxes or problems have been investigated by major publications which solved many problems. The main caveat with this paper is that the authors did not present these studies and gave a dated view of these questions.

In section 3, I give more details for each period including some references which are not exhaustive and the authors should really provide a better and updated list of appropriate references to clarify first if the paradox they consider is still real and what problem they solve exactly.

2. Model description, validation and sensitivity

A second caveat is that the paper is referring continuously to a recent publication (Dortmans et al., 2017) which makes the paper sometime difficult to read. Moreover, despite a detailed description on the different processes included in this model it is not always clear to understand what could be the climatic consequences of this improvement compared to previous EBMs studies.

What is crucially missing is the validation and sensitivity of the model. A rapid validation of the model for present day climate and cryosphere and sensitivity to doubling CO2 scenarios or glacial-interglacial oscillations would bring some credit to the model capability before testing it for deeper time periods.

In their conclusion, surprisingly, the authors claim that after solving difficult problems for deeper time that are associated with changes in topography, CO2, vegetation…they will study future climate. But in fact, we would be very interested to know what is the sensitivity of the EBM to doubling CO2 in present day configuration. Indeed, there are a large bunch of model results on this transition. It would be therefore useful to compare the result of this EBM to other model results.

The fact this EBM is dividing the Earth in 3 latitudinal boxes (Arctic, Antarctica and tropics) with different properties for each box is puzzling because the climate at the end is global and it is not really possible to separate and optimize independently each box. For instance, for mid-Cretaceous (O'Brien et al., 2017, ESR and Ladant & Donnadieu., 2016, Nature Com) or even for early and mid-Pliocene, the tropical response is not completely clarified and the data model comparison for these periods are not completely robust and therefore the ocean meridional circulation from the Tropics to higher latitudes is also an open question (Z. Zhang et al., 2013, Clim Past). The authors should discuss in more details the consistency of their results over the 3 boxes and their interaction between these boxes.

For periods corresponding to ice sheet build-up (Eocene, Oligocene, Pliocene, Pleistocene) the authors should discuss the limitation of EBM to assess the correct computation of ablation, accumulation to the lack of representation of hydrologic cycle.

3. Application to different paradoxes

This part is the weakest for me due to the fact that many important references are missing and it is not always clear what in which extent this new EBM solves or clarifies these issues.

Concerning Eocene/Oligicene transition: the first paradox raised by the authors has been deeply explored and the bibliography the authors depicted is rather short. For instance specifically on the evolution of climate and ice sheet at the E/O transition a model study Lear et al, Geology, 2008; Scher et al., Geology, 2011; Ladant et al., Paleoceanography. 2014 see reference herein pointed out and explain many features of this glaciation for pCO2 values that are in rather good agreement with reconstructions of the literature even if there are still some uncertainties.

Antarctica tectonically achieved a polar position already circa 90 Ma ago and the ice cap is triggered only 34 Ma ago (see recent publications of Ladant, De Conto and Pollard)

I don't get the feeling the approach of the authors with their new EBM brings new insight when compared to recent publications. Moreover the exact occurrence of the transition seems more like a tuning result rather than a prediction.

Concerning the "Pliocene" paradox:

The conclusion summarizes (page 19) the four different paradoxes for Pliocene the authors tackle in this section. To my opinion, some are not really paradoxes and other may be irrelevant.

Paradox 1, with long lasting climate simulation at 410 ppmv for present day configurations, similar to Pliocene pCO2 reconstructions used in PLIOMIP1 and PLIOMIP2 for instance (see Haywood et al. for boundary condition description), the equilibrium achieved in several centuries would be more similar to Pliocene climate. Therefore with similar pCO2, future climate would be close to Pliocene. Therefore, this transition is more relevant of a radiative threshold on pCO2 (see recent publications Wiley et al., Tan et al. for modeming and Martinez-Botti and Seki for pCO2)

Concerning the second paradox, many studies, especially Lunt in Nature 2008, have demonstrated, using different forcing factors, that the major cause of Pliocene transition is pCO2 decrease. Such behavior has also been depicted by Deconto and Pollard.

Therefore, the others should clarify exactly what is their own contribution to solving these two paradoxes.

The decrease of pCO2 from Eocene to Pliocene will lead to the onset of Antarctic 34 Ma ago associated with tectonic and seaways but it will be necessary to wait much longer that pCO2 reach around 300 ppm for Greenland inception due to much unfavorable conditions. See Tan et al., EPSL, 2017 and also all the papers on cryosphere and climate evolution published by De Conto and Pollard. I don't really understand what is new in this third paradox

The fourth paradox is just a misunderstanding. GCM simulations did not proceed as written by the authors ". The EBM suggests that these GCM simulations, starting with today's climate and moving backward in time, would have remained on the stable frozen
25 climate state of the bifurcation diagram in Figure 7 b), and thus failed to "see" the coexisting warm state.". First, GCM do not move backward in time, and second, they are deterministic and of course cannot capture two different equilibrium modes. these models prescribed boundary conditions and a starting state. I believe that the authors mean that because some of these models start their simulations from a present day they are biased by cold conditions and only reach a cold solution and miss another equilibrium. But this point of view is over simplified. Long transient simulations during Cenozoic of De Conto and Pollard are able to reproduce these glaciations onsets.

Concerning mid-Cretaceous climate paradox:

Once again, since Barron's pioneering studies, many recent studies revisited the issue of cretaceous climates and pointed out a large control of paleogeography on climate evolution. Indeed, a major reason for small temperature changes in the Tropics is the polar amplification. Moreover when dealing with deep times the continental distribution is crucial. When neither land nor ice cap is present at the pole, the equator to pole thermal gradient is indeed much flatter. Therefore, the paleogeographic configuration plays an important role for instance during mid-Cretaceous (cenomanian) with high sea-level and smaller continent areas. The authors mostly cited the pioneering studies of Barron and one more recent study (Cromin, 2010) but there are plenty of simulations which depict a more accurate view on this topic for instance Donnadieu et al. ,EPSL, 2006 and Ladant & Donnadieu, Nature Com, 2016 and references herein.

In fig. 9, the EBM simulation shows a similar result for the Arctic and Antarctic boxes which is not surprising because of symmetric forcing factors. It would be interesting to show the result for the tropical box and also to investigate an asymmetric forcing.

Minor comments

- In the section 2.4 "Positive Feedback Mechanisms", the authors should clarify what is really new here. As far as I know, many EBM models, as for instance those currently used by Stap et al., Climate of the Past, 2017 or Weaver et al., Journal Atmosphere-Ocean, 2001, already included these feedbacks. Maybe the computation of GHG is different but then the comparison with other EBMs should be done. Moreover, if there is some added value that doesn't exist in any EBM yet, the authors should describe its effects on climate simulation.
- In the section 3.2.1 "Permanent El Niño and Hadley cell feedback" : Indeed papers as Heather et al., GRL, 2015 suggested permanent El Niño at early Pliocene, but this is not yet a consensus view. Moreover, Lunt et al., Nature, 2008; and many other papers demonstrate that these warmer tropics do not explain the shift to perennial ice sheet over Greenland. Therefore, the authors should discuss in more details, in this section, the potential role of permanent El Niño. Concerning changes in Hadley cell during Pliocene, there are also studies comparing tropical circulation from Pliocene to present day that could be useful to the authors (Sun et al., Climate Past, 2013 and Sun et al., Climate Dynamics, 2018).

- The authors write: *"It complements, rather than replaces, more detailed General Circulation Models (GCM)."* I suggest to write instead for instance: "in complement to GCM, very sophisticated models, including a lot of 3D processes are only able to run some climate trajectories; EBM and EMIC may explore more possibilities and investigate climate transitions (tipping points) but with major simplifications.

- The authors should be more careful concerning the ocean dynamics changes both for early Pliocene (Lawrence et al., Paleoceanography and Paleoclimatology, 2009 and De Schepper et al., Nature Com 2015) and Oligocene/Eocene (Miller et al., The Geological Society of America, 2009).

Conclusion:

In summary, looking to all the applications for which the authors apply this new EBM, I feel very uncomfortable because the presentation of the issues lacks of knowledge on recent studies but also does not bring new insight on these issues.

Finally, I suggest at this stage, to reject this manuscript. I believe there is room for large improvement in two major directions:

1. In demonstrating the validation of the EBM for present day and for simple sensitivity experiments as doubling CO2 and glacial/interglacial transition to test the sensitivity of EBM before going to deeper time climate transitions.
2. Depicting a better and updated insight in the recent bibliography concerning each paradox to show more clearly what is the added value of these new simulations in understanding major transitions.