Second review on the manuscript entitled "An Energy Balance Model for Paleoclimate Transitions" by Brady Dortmans, William F. Langford, and Allan R. Willms.

In the first round I raised major comments on this paper which was promising but suffered from two major caveats. One concerned the model calibration and the other one referred to the lack of clarity on original findings of this manuscript compared with important literature already published and sometimes not even cited.

In this new manuscript the authors seriously answered my major comments.

1 they added appendices concerning comparison of model results and climatology for present day and also investigated the sensitivity to a CO2 doubling

2 They largely increased the number of references cited which is now very important but, since they are using their EBM model in very different paleoclimatic contexts, this justifies such a huge number of references.

Moreover, they clarify the originality but also the limits of their study (page 4).

Please find below my comments to the authors responses and to the new manuscript

1 General introduction: The papers I cited concerning illustration of quaternary bifurcations are to my point of view very useful and relevant to convince Climate of the Past readers of the interest of simple models and bifurcations in a much better constrained context with many climatic proxies than model of intermediate complexity as EBM. Indeed, for instance Paillard and Ganowpolski papers demonstrated that bifurcations may explain abrupt transitions.

Conversely, in Stap 2017, paleogeography is poorly constrained through time and even if the periods are more relevant, the demonstration is, for me, less convincing. Indeed between 38 and 5 Ma, which is the period under consideration in Stap 2017, many forcing factors have changed, as for example paleography, including Tethys shrinkage, Tibetan plateau uplifts, opening and closing of seaways that are not accounted for in this paper.

I have a comment concerning the author statement " However, they would be very interested in exploring with GCM experts the possibility of multiple stable equilibrium states in a paleoclimate GCM".
A good illustration concerning the comparison of EMIC and GCM and the existence of bifurcation is provided in the context of mid Holocene green Sahara. Whereas bifurcation is depicted by Clausen (Claussen et al. GRL 1999, Simulation of an abrupt change in Saharan vegetation in the Mid-Holocene) using an EMIC, according to Liu et al (Z. Liu et al. Science 2009 Transient Simulation of Last Deglaciation with a New Mechanism for Bølling-Allerød Warming) , such a bifurcation is not necessary when using a transient GCM simulation.

I made this comment because I think the authors should discuss more the competition between EMIC showing bifurcation and transient GCM simulations that include non linear processes and not necessarily showing bifurcations.

Page 4, the authors present carefully the limits and the possibility of their new EBM, but once again, considering the progress made on coupling OAGCM coupled with ISM and describing the triggering of Antarctica (Ladant 2015) and Greenland (recently published Tan et al 2018), we can wonder if the concept of bifurcation is necessary. The authors should justify the interest of the EBM in these 2 different contexts.

My opinion as already mentioned in the previous review is that a hierarchy of models is necessary. GCM explored only some trajectories while EMIC may explore a much larger range of parameters.

2 Model presentation and publication strategy

The authors accounted for my previous claim concerning the necessary validation of their model and its sensitivity. Nevertheless, I am still puzzled that the authors provide a first manuscript devoted to the use of the new model for large climate change periods and claim that they will, in a second paper, describe and validate the model and use it for more constrained sensitivity experiments for present day and future climates. I would have preferred in a first paper a solid description of the strong and weak points of the new EBM and in a second paper, applications to key periods of deep time climate changes. Moreover the detailed description of the EBM characteristics provided till page 19 is still difficult to follow. How far EBM parameterizations favor bifurcations ? Moreover such a detailed description of the tool would require some insights on validation of mean present day climate and its sensitivity.

Nevertheless the minimum knowledge has now been provided in the appendices.

Concerning the first appendix, it is devoted on modeling tuning to reproduce present day climate. Still an important figure is missing showing for instance the annual present day temperatures simulated by the model compared to a climatology data set.

3 Paradoxes

The authors have deeply modified this section. Nevertheless, new papers have been published that, at least for Pliocene paradox should be included and discussed. Indeed, papers depicting the transition from a Northern Hemisphere free of ice caps to Greenland inception using OAGCM asynchronously coupled with sophisticated ice sheet models as for example Tan et al Nat. Com 2018 and Willeit QSR 2015 partly solved the “Pliocene paradox”. Therefore, this concept should be revisited.

More generally, Baron series of numerical experiments did not account for many processes that are nowadays better simulated by coupling AGCM with ocean, vegetation and ice sheets. Therefore, also the mid-Cretaceous paradox is nowadays not so clear. But still the issue of reproducing flat equator to pole thermal gradient is a challenge.

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