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Interactive comment on "An Energy Balance Model for Paleoclimate Transitions" *by* Brady Dortmans et al.

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The authors wish to thank Referee#2 for insightful comments on the manuscript. We are grateful for the time and effort taken by both Referees to write such detailed reviews, and also for the updated references to the paleoclimate literature. The point of view given by the references in the original manuscript is indeed dated, and this will be corrected with more current references as suggested by the Referees.

Our comments in this Reply follow the same order as the Referee#2 comments.

Major comments

1. Presentation of the climate paradoxes

The authors agree with the Referee that a large hierarchy of models to investigate different climate transitions is necessary. The papers of Roche et al. (2004), Claussen et al. (1999), Paillard et al. (1998) and Ganopolski et al. (2001) illustrate this nicely. The authors found them to be informative reading. However these four papers deal with more recent climate changes than considered in the manuscript, and therefore have not been added to the bibliography. The glacial-interglacial cycles of the Pleistocene are believed to be driven primarily by variations in Earth's orbital parameters, which are not present in the EBM of the manuscript, and thus it can not address these phenomena.

However, the authors found the paper of Stap et al. (2017) extremely interesting. Those authors found hysteresis in the relationship between CO2 and temperature, over the past 38 million years, in a simple one-dimensional ice sheet model. Here "hysteresis" means the co-existence of two different mathematically-possible stable climate states. The Earth climate system can be in only one state at a given time, and the selection of which state is occupied depends on the past history of the state of the system. Stap et al. (2017) found both a warm equable state and a cold state, depending on the initial conditions chosen for their run. This result is in qualitative agreement with the predictions of our EBM. The authors have long wondered if any of the GCM studies of paleoclimates have found multiple stable climate states, for the same values of forcings. We would be very grateful if the Referee could point us to any other publications that show hysteresis or bistability in a climate model.

As for the "real added value of this study", the authors feel that their main contribution is a demonstration that a simple EBM based on sound physical principles, which are intrinsically nonlinear, can exhibit hysteresis (or bistability). That is, mathematically the EBM may have two different stable equilibrium states; one warm and equable and the other non-equable with frozen poles. The actual climate state of the Earth, or of a GCM experiment, can exist in only one of these two states. In reading the classic papers of Barron and others on the "warm equable climate problem", the authors were led to conjecture that a solution to Barron's problem may be that the Cretaceous Earth's climate state and the solution state of Barron's early GCM experiments were simply exhibiting two different climate states, both of which were mathematically correct. The authors can not prove, from their simple EBM, that they have "solved" Barron's problem. This is only a suggestion. However, they would be very interested in exploring with GCM experts the possibility of multiple stable equilibrium states in a paleoclimate GCM.

Similarly for the "paradoxes" discussed in the manuscript, the authors can not claim to have solved them with a simple EBM calculation. Any such claims will be modified in revising the manuscript. Rather, we hope that this paper will lead to further investigations with more sophisticated models that will confirm (or refute) our conjectures. In addition to suggesting bistability, our EBM suggests that mathematical bifurcations may account for abrupt climate changes, such as occurred at the Eocene-Oligocene Transition at the South Pole, and in the Pliocene Transition at the North Pole. It has been recognized for some time that these abrupt transitions were some form of "tipping points". To a mathematician at least, it is very satisfying to think that these tipping points can be explained as mathematical bifurcations, which are unavoidable in the EBM.

A great deal of progress has been made on these problems and paradoxes in the climate science literature, since Barron's pioneering work. Thanks to the Referee's suggestions, the authors can and will cite this exciting current work, and clarify how the present work relates to current work in climate science.

2. Model description, validation and sensitivity

The Conference Proceedings publication (Dortmans et al. 2017) presents an earlier version of the EBM and a preliminary analysis of the Pliocene paradox. We will make the current manuscript more self-contained and independent of that proceedings paper.

The authors will clarify the climatic consequences of this improved EBM compared to previous EBM studies.

The authors have performed an Equilibrium Sensitivity Analysis (ESA) of the EBM under modern conditions, and plan to publish our ESA results and comparisons with other ESA results in a follow-up paper. ESA has become useful for comparisons in the IPCC reports. The main focus of this second paper will be anthropogenic climate change. We will not study glacial/interglacial oscillations because the mathematical analysis is quite different for orbital forcings. Journals in climate science tend to focus either on paleoclimates or on modern-day climate change, so we have split our work accordingly. We chose to apply our model first to paleoclimate changes, because today much is known about the paleoclimate changes addressed in this manuscript; before, during, and after the changes took place. This enabled us to "calibrate" our EBM before applying it, with greater confidence, to future climate changes which are still speculative.

It is true that changes in climate are influenced by many factors, including changes in topography, vegetation, etc. The authors' goal in this study was to find the simplest climate model that could address some well-known paleoclimate paradoxes. This model includes the effects of both CO2 and H2O as greenhouse gases, in a more physically realistic way that was the case for previous EBM studies, and includes ocean and atmospheric meridional heat transport, but ignores other forcing factors. We feel that our EBM has been remarkably successful in achieving this goal.

Regarding the separation of the Earth's climate into three latitudinal models, for Arctic, Antarctic and Tropical climates, respectively, this was done to improve resolution over traditional EBM's which only consider a globally averaged energy balance. With a globally averaged EBM, one can not study Arctic amplification, for example. In the manuscript, the polar models are coupled to the tropical model indirectly, through the ocean and atmosphere transport terms, which are adjusted by hand. The authors had difficulty in finding values for these meridional transport terms; in this regard the Referee's references are useful. The paper of O'Brien et al. (2017) provides valuable detailed information about Cretaceous climates. The consistency of our results over the three boxes will be discussed in more detail. In future work, these separate models will be coupled directly in pairs, and then later combined into a continuous meridional model.

Regarding ice-sheet build-up and ablation, these can only be estimated in this simple model. The important consequence for the EBM is the change in surface albedo; high albedo below the freezing temperature and low albedo above freezing.

3. Application to different paradoxes

Referee#2 found this the weakest part of the manuscript. Clearly it needs clarification.

Concerning the Eocene/Oligocene Transition (EOT): The authors thank Referee#2 for bringing to our attention important recent studies of the EOT. We stand in awe of paleoclimate "detectives", such as Lear et al. (2008) and Scher et al. (2011), who deduce details of climate changes that occurred 34 Ma, from seemingly insignificant (to us) benthic foraminifera. These studies confirm that an abrupt climate change, including glaciation of Antarctica, occurred 34 Ma (perhaps in two steps). The modelling paper of Ladant et al. (2014) combines an atmospheric GCM with an ice-sheet model and obtains generally good agreement with proxy data for the EOT. Ladant et al. (2014) state "The reasons for this greenhouse-icehouse transition have long been debated, mainly between the "tectonic- oceanic" hypothesis and the CO2 hypothesis". Our simple EBM weighs in on this important debate, suggesting that both of these forcing factors are necessary to account for the transition. We also support the viewpoint that it was not the polar position of Antarctica but rather the onset of the ACC that led to Antarctic glaciation. The new insight that our EBM brings to the EOT debate is a simple mathematical explanation for the suddenness of the transition. Typically, geological changes occur slowly, over long periods of time. The glaciation of Antarctica was very abrupt on a geological time-scale. Our model suggests a simple mechanism for this sudden transition. Referee#2 is correct in thinking that the timing of the transition in the EBM is in part fortuitous. What is important in our view, is not the prediction of the timing of

transition, but rather the mathematical existence of a sudden transition in the EBM. In our EBM, a sudden transition must occur at some point during the Eocene-Oligocene.

Concerning the "Pliocene paradox": The term "Pliocene paradox" was not introduced by us, but rather by Fedorov et al., Science (2006), who stated "During the early Pliocene, globally averaged temperatures were substantially higher than they are today, even though the external factors that determine climate were essential the same". The manuscript authors took this statement as evidence of bistability; that is, two distinct stable equilibrium climate states coexist, with the same forcing factors. In our simple EBM, mathematical analysis confirms the co-existence of two distinct climate states with the same Pliocene forcing factors; one state matches the early Pliocene warm climate and the other is similar to today's climate with a frozen Arctic. Although we can not prove that the much more complex climate of Earth behaves like our simple EBM, we feel that the EBM result gives a new perspective that can be explored further, and has the potential to "solve" the Pliocene paradox stated by Fedorov et al. (2006).

The second "paradox" considered in the manuscript is the abruptness of the climate transition, from equable to frozen, in the Arctic during the Pliocene. The authors agree with Referee#2 and other authors cited that the major cause of the Pliocene transition is pCO2 decrease. However this does not explain why the climate changed drastically, after a long period of gradual change, even though the pCO2 continued to decrease slowly. The EBM provides a mechanism for this sudden change, namely a saddlenode bifurcation, which characteristically causes a "jump" to a new state with only a small change in parameters.

The third paradox concerns the broken North-South symmetry of the Earth's climate, that persisted for 30 Ma, from the EOT to the Pliocene. To paleoclimate scientists, this is not a paradox because it is well understood. To others, it is a surprise to learn that the Antarctic was ice-covered while the Arctic was ice-free for most of this 30 million year time frame. The North-South symmetry was broken by plate tectonics, geography and ocean currents that were very different around the two poles. Knowing this, it is no surprise that the polar climates were different. Tan et al. (2017) shed light on Arctic glaciation and the closure of the Central American Seaway in the Pliocene. Referee#2 is correct to say that there is nothing fundamentally new in this third paradox of the manuscript. The EBM simply confirms what paleoclimate scientists already know. Therefore, this will be removed from the list of paradoxes in the manuscript and replaced with an observation in the main text.

The fourth paradox is indeed a misunderstanding. The authors were not well-informed on the methodology and efficacy of modern GCM simulations. Our thinking was guided mainly by the pioneering work of Barron, Earth Sci. Rev. (1983), as described in Section 3.2. The Referee is correct to surmise that "the authors mean that . . . some of these models . . . only reach a cold solution and miss another equilibrium." The so-called fourth paradox will be removed from this section and discussed later in the context of the warm equable Cretaceous climate problem. Modern GCM simulations have been successful in closing the gap with Pliocene paleoclimate proxies, and these will be referenced.

Concerning the mid-Cretaceous climate paradox: Barron (Earth Sci. Rev. 1983) stated that proxy data showed mid-Cretaceous climate to be warm and equable, while his GCM simulations gave a climate more like today's climate. He called this discrepancy the warm equable Cretaceous climate problem. In this manuscript, we suggest that both may be correct, if the Earth's climate system (like our EBM) has multiple stable equilibrium states. The actual Cretaceous climate was in a warm equable state, while Barron's GCM computed (correctly) the colder state. Given that the GCM was originally designed to model today's climate, it would not be surprising for it to find the Cretaceous climate state that is more like our own climate. Modern, more successful simulations also will be referenced. The authors agree that the paleogeographic configuration plays an important role. The papers of Donnadieu et al. (2006) and Ladant & Donnadieu (2016) shed light on this issue. There is no "geography" in our EBM; however, the effects of continent areas and sea levels are included indirectly in the ocean

heat transport parameter of this EBM.

Minor comments

In section 2.4 "Positive Feedback Mechanisms", neither ice-albedo feedback nor water vapour feedback is intrinsically new; these have been understood for many years and are included in more sophisticated models. In simple EBM studies, however, such as Payne et al. (2015), ice-albedo feedback is usually modelled as a discontinuous step function and water vapour feedback is usually just a parameter tuned by hand, instead of determined by the Clausius-Clapeyron equation. These changes make the EBM amenable to bifurcation analysis, which had not been done before.

In section 3.2.1, "Permanent El Nino and Hadley cell feedback", the idea that a permanent El Nino in the early Pliocene explains the Pliocene paradox is not supported by us, but has been proposed by others. The idea that Hadley cell feedback contributed to Arctic amplification also is controversial and is not well justified. The papers of Sun et al. Climate Past (2017) and Sun et al. Climate Dynamics (2018) shed light on this Hadley circulation. It is not very relevant to this manuscript and probably is best omitted.

In the Introduction, the authors thank Referee#2 for the revision of the statement beginning "It complements, rather that replaces more detailed . . .".

Ocean dynamics changes are not part of this EBM, except in so far as they change ocean heat transport to the poles. The references for the Pliocene and EOT will be added. Lawrence et al. (2009) and De Schemer (2015) provide important information about SST and ocean dynamics in the Pliocene and Miller et al. (2008) for the EOT.

Conclusion:

Equilibrium Sensitivity Analysis (ESA) for modern day doubling of CO2 will be presented in a follow-up paper on anthropogenic climate change. This analysis will follow the approach of the IPCC.

Glacial/interglacial transitions were driven in large part by orbital forcings that are not included in this EBM. Addition of these issues to the manuscript would significantly lengthen what is already a rather long paper.

Thanks to the suggestions of Referee#2, better and updated insights into the recent bibliography will be added.

The added value of the new EBM simulations in understanding major transitions will be made clear.

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