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

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General comments:

The authors wish to thank Referee #1 for his/her thorough review of the manuscript and many insightful comments. All three authors are mathematicians by training and are climate science neophytes. The referee's patience in explaining more subtle climate issues is very much appreciated.

The EBM of this paper is indeed very simple and does not include the effects of precipitation, atmospheric circulation, geography or vegetation. The authors' intention was to explore the role that mathematical bifurcation theory might play in climate change, starting with the simplest possible model in order to keep the mathematics easy. This

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Discussion paper

paper is intended to be only the first in a sequence of more sophisticated models that will be closer to reality. This point will be clarified in the paper.

The results we have obtained in this paper are qualitative, not quantitative, since the model we are using does not include many contributing features such as precipitation, etc. However, we believe the model contains the most important features and the point we wish to emphasize is that the model predicts there must be a bifurcation where a warm climate state disappears leaving only a cold stable state and that this is is an important insight to understanding both the Pliocene paradox and the glaciation of Antarctica.

The authors will rewrite the text to be shorter and more focussed, avoiding repetition, and the figures will be clarified. In particular, claims made regarding paleoclimate and geological changes will be substantiated or changed. The recent publications referenced by the referee will be added. Comparisons with GCM results will be rewritten.

Specific comments:

P2 L9; L17; L20. These concerns will be addressed explicitly in the revised paper.

P3 L4: The referee is technically correct. In mathematical bifurcation theory, there is a proof that bifurcations are "structurally stable", that is, they persist in a wide class of generalizations of the model, however; it is not clear that this proof applies to something as complex as climate. L27: Albedo is treated later.

P4 L12: L14: Typos will be fixed. L15: Citation will be added.

P5 The referee has uncovered an inconsistency in Table 1. As the paper evolved, the values in Table 1 also evolved, originally reflecting Arctic values only, but later reflecting global values. Table 1 will be rewritten to be a table of Arctic values, with later sections showing corresponding Antarctic and Tropical values where appropriate.

P6 The symmetry of the tanh function is incidental here; we don't think it is important. We agree with the referee that the addition of a smooth transition between cold and warm states is important.

P7 The referee makes a number of good points here; all will be included in the paper.

P8 L5: The authors do not understand the referee's comment that the temperature scaling is "preliminarily ill-posed". This scaling used here brings the temperature variable to neighbourhood of 1 and brings all parameters closer to 1; the most well-posed of numbers.

P9 L11: Yes; that was our idea. CH4 behaves similarly to CO2 and can be included In CO2 as a greenhouse gas. We will make this explicit; i.e. \mu represents CO2 + CH4.

P10 L18: Yes, this is redundant.

P11 F3: The caption of Figure 3 a) gives the values of CO2 on the blue curves, decreasing from top to bottom. This seems clear to us. Placing that information inside the figure might clutter the figure. Also, distinguishing each of the 5 blue curves by a different colour or line type could cause clutter. Our goal was to distinguish between the surface equation (red) and each of a family of atmosphere equations (blue). L8: We will make this less pedantic.

P12 L3: OK, separate sentence. L10: We will give a range for \delta and say "varies less" instead of "more nearly constant".

P14 F4: Referee is correct. Figure will be truncated.

P15 L13: The explanation of positive feedback will be abbreviated. The main point here is that there are two independent mechanisms of positive feedback in the EBM. L25: Right. We intended that this apply only to the early Paleogene, not to the entire period. We will rewrite this sentence. L30: Removed "the"

P16 L2: "Development" instead of "creation" is fine. The "extremely simplified view" described here is, we think, appropriate for our simplified model. There was indeed "a complex geological and oceanic evolution that took place over ∼20Ma", but most of this is outside the scope of our model. L5: The lower value of 30 W/mˆ2 is just a guess, frankly, just as the upper value of 100 W/mˆ2 was a guess in the 1980's (by Barron). The authors had great difficulty finding reliable estimates of Ocean Heat Transport (OHT) to both poles, in different time periods. Surely, the combined effects of the ACC and the continent of Antarctica have reduced the OHT to the South Pole essentially to zero, today. If it started at 100 W/mˆ2 [Barron], then by the EOT it would have decreased significantly to, say, 30 W/mˆ2. This is plenty good enough for our topological model. L6: Ma instead of MYa. We will add references here regarding EOT / glaciation. L7: CO2 concentration is assumed the same worldwide. F5: Yes, A stable frozen solution exists in the EBM at 1500 ppm CO2, but it disappears slightly above 1500 ppm. Because this is a crude model, exact numerical values are not significant.. The important point is that the model gives a mechanism that explains the disappearance of the frozen solution, via a saddlenode bifurcation.

P17 L17: Agreed. CO2 definitely did not decrease linearly. But this is irrelevant for the bifurcation theory. All we need to know is that CO2 decreased. Then it must have crossed the bifurcation value somewhere. It is amazing good luck that it happened in the model at a time that corresponds so well to the EOT boundary. (With a little help from the authors, of course.) The MECO is an anomaly and is irrelevant to our analysis, as is the PrOM event. The Post-EOT recovery and Antarctic deglaciation ∼25 Ma, and re-glaciation at ∼13 Ma are fascinating, by are not explained by our analysis (yet). L29: Agreed. The referee was paying attention to notice this. But it is very satisfying for the modellers that they were able to hit the EOT boundary so accurately. We will add a disclaimer. The good numerical agreement here should not be taken too seriously.

P18 L7: References added. L9: It is true that "the crossing of a CO2 threshold with/without the effect of Southern Ocean Gateways has been suggested many times before". The relative importance of these two mechanisms is openly debated in the literature. We think our paper is the first to indicate that both mechanisms are necessary, to cross the glaciation threshold, and that the suddenness of the transition may be explained by a bifurcation.

P19 L16: Okay, we will remove speculation about pre-Cretaceous climates. L18: Okay, we will change “the Arctic was ice-free” to “the Arctic was largely land ice free”. L19: Okay, we will remove the comparison to the relative humidity of today’s climate. L25: Okay, we will remove the implication that the Arctic was warmer, and also reduce the influence of ocean heat transport. L29: Typo fixed, thanks. L31: The EOT marked an abrupt initiation of glaciation in the Antarctic. However, the accumulation of ice/snow in the Antarctic was gradual, due to the low relative humidity. The depth of ice/snow in Antarctica went from meters to kilometres over a period of millions of years and sea levels dropped inversely. L34: We will note the existence of cold fresh waters flowing out of the Arctic, into the North Atlantic. L35: Point taken. The North Atlantic existed, but was only narrower and not always connected to the Arctic Ocean.

P20 L1: Typo fixed. L1: We will add a ref. for the closing of the Turgai Strait, date its closing more accurately. L3: We will clarify that the “cooler Pacific” reference is only local to this region. L7: This reasoning at first seemed far fetched to us too, but the papers of Haug et al. (2004) and Bartoli et al. (2005) convinced us. It is true that our simple model does not include the effect of enhanced precipitation directly. However, the work of Haug et al. (2004) and Bartoli et al. (2005) makes a strong argument that the net effect of the mechanisms described here was to reduce the ocean heat transport from the North Atlantic to the Arctic. This we can use in our EBM, and thus indirectly include the effects of enhanced precipitation. It is true that qualitatively the same problem is being solved for the Antarctic and Arctic glaciations. Both involve the same saddlenode bifurcation in our EBM. But, the important question remains: Why did these two glaciations occur at times separated by 30 million years, when for most purposes the Earth has a North-South symmetry? Our model gives a plausible answer to this question, by including both global CO2 concentration and ocean heat transport to each pole, in the calculation. L15: This discussion will be abbreviated. L30: Pagani et al. show continued decrease in CO2 levels during the Oligocene, but nearly constant

CO2 in the Miocene. This suggests that the decrease in ocean heat transport occurring at this time, as described in the paragraphs above, though small, played an important role in the final Pliocene transition.

P21 F7: Yes, in Fig. 7a), only the frozen solution exists for CO2 less than about 400ppm. However, this is true only for the particular choices of F_O and F_A indicated in the figure. Since there is great uncertainty in the values of F_O and F_A, the quantitative predictions of the EBM can not be taken literally. What is important here is that, as CO2 decreases (blue curve moving downward) and F_O decreases (red curve moving upward), a saddlenode bifurcation is inevitable, and this bifurcation corresponds to an abrupt glaciation of the Arctic. This is a qualitative, not a quantitative result. Also, the differences in conditions in the Arctic and Antarctic account for the multi-million year gap in the timing of the two glaciations. The interglacial oscillations appear to have been driven by Milankovitch cycles, a phenomenon well outside the scope of this paper.

P22 L2: As noted in the paper, there are several "Pliocene Paradoxes" to be considered here. We believe that our EBM sheds some light on the understanding of all 4 of the "paradoxes" listed here. Still, this simple model is not the final word on any of them. L5: This is a matter of degree. The early Pliocene was not as equable as the early Eocene, but was more equable than the late Pliocene. L14: Yes. The underlying cause of the asymmetry was simple geography, as the Referee points out. Exactly how this asymmetry in geography manifests itself in the asymmetry of the polar glaciations is investigated in this paper. L23: The authors are not experts on GCM spin-ups, and they thank the referee for this insight. The text will be corrected. L29: Fedorov et al (2006,2010) announced that there was a permanent El Nino condition in the early Pliocene. This is not inconsistent with ENSO conditions in the Eocene. The authors do not take responsibility for claims of a permanent El Nino in the early Pliocene,

P23 L1: This sentence refers to the equator to pole temperature gradient, post-glaciation, which clearly increased. The remainder of this paragraph, concerning

“Hadley cell feedback”, is speculation at this time.

P25 L2: Yes, a drop on temperature in the Tropics of 30C, in the 100 million years from mid-Cretaceous to Pre-industrial, is excessive. However, this simple EBM is qualitative, not quantitative, and this number should not be taken too seriously. The authors had difficulty finding estimates of heat transports (F_O and F_A) away from the Tropics, so the values used here are only estimates. The important take-away from this Tropics model analysis is that (a) the average Tropical temperature did decrease, and (b) there were no bifurcations. L16: Thank you for this information.

P26 L11: Yes, the predicted equable pole-to equator temperature difference is excessive, but again the discrepancy is quantitative, not qualitative. L14: Corrected. L15: Thank you. This will be corrected.

P27 L6: We will expunge the word “failed” and its synonyms, in all reference to GCMs in this paper. Thank you for pointing us to recent progress. Section 3.5: The authors agree with the Referee that there is nothing new, either mathematically or physically, in this Section. The authors chose to separate the “warm equable mid-Cretaceous climate problem” and the “warm equable early Eocene climate problem” into different sections, because they have been presented in the literature as two independent problems. (Perhaps this is because the proxy records are separated by almost 50 million years.) From our point of view, they are the same problem. Would the Referee approve a rewriting that combined Sections 3.4 and 3.5 into one section, “The mid-Cretaceous and early Eocene warm equable climate problems”? This would remove some redundancies.

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