Title: Linkage of tropical glaciation to supercontinents: a thermodynamic closure model

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In this paper, the author uses a simplified two-box model of the Earth and investigates how the tropical and polar temperature distribution changes with the change in the area of tropical land. The main finding is that, as the tropical land area increases, solar radiation is reflected effectively, and both tropical and polar temperatures tend to decrease towards the ice-forming temperature, although the land area in the polar region decreases. This tendency may explain tropical glaciations due to the appearance of Precambrian supercontinents, as well as some subsequent glacial epochs triggered by continental drifts. This reviewer finds these results basically interesting. However, the model assumes intricate relationships between radiations and temperatures that are not explained explicitly in this paper. Also, some of the model parameters and assumptions are stated so vaguely that readers of this paper cannot verify the validity of the results obtained. This reviewer therefore recommends major revisions of this paper regarding the comments below.

- *I want to thank you for the highly substantive comments, which point out many shortfalls of the paper. Together with the other reviewer's comments, I plan to thoroughly revamp the paper, as contained in my response given below in the italics.*

- *I will add the differential heat equation and show how MEP would link temperature to the forcing. I will expand discussions of parameters and assumptions as detailed below.*

Major comments:

1. Equation (1) and the sensitivity of s = 0.5 °C/(W/m^2).

This sensitivity (0.5) is used to estimate the dependence of the global-mean temperature on the tropical land area A in Fig. 3b, and therefore plays a significant role. This value, however, is quite large compared to the well-known theoretical value calculated from the Stefan-Boltzmann law: $F = \sigma T^4$, from which one can derive $dT/dF = 1/(4 \sigma T^3) \approx 0.2$ for $T \approx 280$ K. Although the used value is more than twice as large as the well-known theoretical value, the rationale for this is not clearly explained in the text or the cited reference, making the estimate somewhat doubtful. Please provide a valid reason for using this value.

- *"Global" sensitivity is the change of the global surface temperature given that of the global forcing, the latter being the absorbed SW flux or the outgoing LW flux as you have intended. This flux however is quite smaller than the blackbody radiance because of the greenhouse effect. If one uses a LW window of 0.3, it would augment your sensitivity estimate three-fold to 0.6, which is still near the low end of its calculated range [0.5,1.5] (Manabe and Wetherald 1967, their Table 5; Bjordal et al. 2020), so to conform to these calculations, I plan to set the global sensitivity to unity. I will add above discussion in the revision.*

2. Equation (2) and the air-sea transfer coefficient of α = 15 (W/m^2/°C).

This coefficient implies the reciprocal of the temperature sensitivity, and is used to estimate the tropical and polar temperatures from the deviation of solar radiation from the average. The corresponding sensitivity, $1/\alpha \approx 0.07$, then plays a crucial role in determining the temperatures of both regions. This sensitivity is extremely low compared to the global sensitivity of 0.5, resulting in a somewhat strange result of a decreasing polar temperature with increasing area A, even with a slight increase in the net solar radiation in the polar region (q_2 in Fig. 3a). Thus, these two parameters (1/α and s) determine the general temperature dependence shown in Fig. 3b. However, there is no rational explanation for this low sensitivity value in this paper or the cited reference (Ou, 2018, Appendix B). Also, I suspect this transfer coefficient applies to air-sea interaction, not air-land interaction. The author invokes an MEP principle for justification, but then the basic logic for the estimation from that principle should be explained.

- *The linkage of differential temperature to differential forcing (I shall call it "local" sensitivity) bears no relation to the "global" sensitivity since latitudinal variation of the LW flux is dominated by that of the convective flux and in fact neglected in our model (see Section 2). This is the reason that the local sensitivity depends only on the air/sea exchange coefficient, which in turn has no import on the global sensitivity. I will add this discussion in the revision.*

- *I will provide an explicit expression of the air/sea exchange coefficient in the revision, which entails Bowen ratio and turbulent wind. Although both vary latitudinally, their effects cancel somewhat (Bowen ratio is smaller in the tropics where wind is also weaker), so we are justified to use their global-means in our minimal model. Setting the global-mean Bowen ratio at 0.25 (Peixoto and Oort 1992, their Fig.14.1) and turbulent wind at 3 m/s (Hartmann 2015, his Fig. 4.7) yields $\alpha = 15\ Wm^{-2}\ {}^{0}C^{-1}$. This estimate is supported (in fact informed) by the observed differential temperature range of 20 ^{0}C given the equator-to-pole forcing range of 300 Wm^{-2}. As a further test, we note that the MEP-deduced MOC (meridional overturning circulation) depends only on α, and the above estimate yields a transport of 17 Sv for the North Atlantic, not unlike the observed one. I will add this discussion in the revised paper.*

Minor comments:

1. L104-105: "clouds would self-adjust to stabilize the temperature constrained by intrinsic water properties".

Here, the surface temperature seems to be related to the cloud amount. If so, it would be easier to understand if the cloud amount was also shown in Fig. 1.

- *I will remove this figure since without full closure as discussed in Ou (2001), it raises more questions than it answers, as you have rightfully pointed out. On the other hand, intrinsic water constraint on the surface temperature can be rationalized by simpler statements, such as (to be refined): "surface temperature is bounded below by the greenhouse warming and above by the evaporative cooling due to accelerating rise of the saturation vapor pressure with temperature on account of the Clausius–Clapeyron equation".*

2. Figure 2.

I cannot understand what are the lines shown on the top-right side of this figure. Also, there is a protruding structure in the tropical region. It would be better to explain what these mean.

- *They are the tropopause and polar front dividing the tropical/polar airmasses. The protruding structure is the highland, which is critical in seeding the sea-level ice sheet even when the surface is above the freezing point. I will amend the caption and add this discussion in the text.*

3. L171-172: Equations (3) and (4).

In these equations, the reflectance of the sea surface appears to be assumed to be zero. If so, this assumption should be stated in the text as the sea surface reflectance is known to be about 0.1, which is larger than zero.

- *I shall add a statement about the neglect of the ocean albedo (common practice) in comparison with the land albedo, particularly before the advent of land plants.*

4. L182-183: "the land reflectance is that of a desert set to r = 0.5, ... for a total land area set to 0.3".

Here, the land reflectance (r) seems to be changed for a desert (0.5) and total land (0.3). However, it is unclear how the reflectance is changed in the model calculations shown in Fig. 3. Please resolve this ambiguity.

- *I am sorry about the confusion, which will be clarified in the revision. The land reflectance has a single fixed value 0.5 and the total land (0.3) refers to its area as a fraction of the global surface. The x-axis of Fig. 3 is the tropical land area, which increases from 0 to 0.3.*

5. L217-218: "not ... the ice-albedo feedback".

In this model, the land reflectance is fixed to a value (0.3 or 0.5, see the above comment 4). So this model does not include the ice-albedo effect. If so, one should not deny the ice-albedo feedback by the results obtained from a model that contains no ice-albedo effect.

- *Land reflectance is fixed at one value 0.5, see the preceding response. I will write down the differential heat equation to show that perennial sea ice is nil so long as there is differential SST to institute the OHT (ocean heat transport) maximized by MEP. This is a robust outcome of MEP, which nonetheless may explain why subpolar water remains open during LGM (last glacial maximum) even though it is at the freezing point. Over paleohistory, it is also noted by Eyles (2008) that "no sedimentary evidence of sea ice for all glacio-epochs, contrary to models".*

- *I will expand the discussion of ice-albedo feedback by way of bifurcation diagrams to show its progression from iceball earth to tropical waterbelt to divergent land-ice line and MEP. Since the land-ice line is marked by above-freezing temperature of the co-zonal ocean and sea-ice line has retreated to the pole by MEP, there no runaway ice-albedo feedback until the global ocean is cooled to the freezing point, a precondition of iceball earth.*

- *In addition, I shall argue that the iceball earth is untenable by the global-mean balance since positive greenhouse feedback would warm it to above the freezing point (see my response to Comment 1). It is seen therefore that MEP resolves the faint-young-sun paradox, precludes the iceball earth, and allows tropical glaciation abutting an open ocean.*

6. Figure 3b: the profile of T1.

There is a bend in the temperature profile (T1) in this figure, and I cannot understand why. Could you explain the reason?

- *That is because T_2 has already reached the freezing point (the x-axis) and yet its average with T_1 equals the global mean (thick solid line). I shall add this statement and elaborate on the differential temperature in discussing the bifurcation diagram.*

7. Equation (9).

I cannot understand the meaning of this equation. Please explain this equation in more detail.

- *This equation is wrong and will be removed.*

References:

Bjordal, J., Storelvmo, T., Alterskjær, K. et al. Equilibrium climate sensitivity above 5 °C plausible due to state-dependent cloud feedback. Nat. Geosci. 13, 718–721 (2020). https://doi.org/10.1038/s41561-020-00649-1

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Ou HW (2001) Possible bounds on the earth's surface temperature: from the perspective of a conceptual global-mean model. J Clim 14:2976–88 https://doi.org/10.1175/1520-0442(2001)014<2976:pbotes>2.0.co;2

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