**Earth System
Dynamics
Discussions**

Interactive comment on "Entropy production and multiple equilibria: the case of the ice-albedo feedback" *by* C. Herbert et al.

Anonymous Referee #2

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This paper reconsiders the classical Snowball Earth problem comparing a classical dynamical systems approach and MEP-based approach, introducing also a new method to analyze the stability of the multiple equilibria based on the MEP hypothesis. The paper is interesting, generally well written, and is recommended for publication after minor revisions.

My principal remarks are:

1) The authors refer to Herbert et al. 2010 for the description of their 'net exchange formulation' box-model. Unfortunately, the paper seems to be not published yet, nor present on public access services as Arxiv (or at least, I have not found it). Even if knowing the details of the derivation of the model is not necessary for a general understanding of the paper, the original source should be made available in some way.

2) The main point of this work is to give a connection between the dynamical systems approach and a MEP-based approach in finding the possible steady states of a system and in assessing their stability. Nevertheless, basically no description is provided of what the MEP hypothesis is and its range of validity as discussed in the literature. The authors refer again to Herbert et al. 2010, but I think that some (short) introduction on the topic should be provided also in this paper.

3) The description of the method to integrate the trajectory of the system using MEP is in my opinion not fully satisfactory. To my knowledge, the MEP principle has been so far used in the literature only as a variational principle to identify the most probable steady state among others. Its use in a non-stationary context is therefore questionable, and a careful motivation for that is needed. As far as I understand, you consider that at each instant the system can be considered as if it were in a steady state, with 'frozen' variables and an additional energy flux given by what would be the instantaneous variation of the (now frozen) variables due to the actual non-stationarity. This reinterpreting the time dimension as an additional geometric dimension (by the way, why the vertical dimension should be 0.5? it has not a fractional dimension, it′s just that the description of the vertical dimension has been discretized, while the description of the time dimension has been kept continuous). MEP is then used to find the state of the system at each instant. Why do you think this should work? Do you propose that it′s always possible to determine the time-dependent state of a system in this way? Or is it only valid in a neighborhood of a steady state? (actually this would be enough for a stability analysis, I guess). In any case, is it valid for any system or only for the one you are considering here? Of course, the fact that in this way you are able to obtain the correct properties of stability of the steady states of the system is promising, nevertheless I think that a better description of the physical or mathematical basis of the method, also addressing these questions, would definitely improve the paper. The idea is very interesting and the results promising, I think it should be adequately discussed.

Some minor remarks (in addition to what already noted by the first reviewer):

1) page 328, lines 20-23: I would not use the abbreviation 'resp.' (check for similar truncations also in the rest of the paper);

2) page 329, lines 8-9: the latent heat flux at the surface is missing in the description of the energy exchanges in the Earth system;

3) page 329, eq. (6) and (7): personally I don't think that the notation 'da' and 'dg' is appropriate, they look too much like differentials;

4) pages 329-330: Eq. (8) and (9) represent energy balance on time-scales over which the system is in a steady state, while the dynamical model (13) is derived from the full, instantaneously valid energy balance equations. I think that the use of the steady state version in (8) and (9) could be misleading for the reader (and moreover it's never used in the rest of the paper);

5) page 329, eq (12): the parameters of the bulk formula are not defined (even if their meaning is obvious). Take care in general that all the variables and parameters which are used in the formula are defined in the text;

6) page 335, line 16: use Kelvin units as in the rest of the paper ;

7) page 337, eq(29): why the entropy production is denoted as an increment? that is exactly the entropy production of the system at the instant t+dt. Am I missing something in the comprehension of the method? Again an improved description of it would be helpful, also in explaining why you are using this notation.

Finally, even if I'm not a native English-speaker, I would suggest a revision of the paper from the point of view of the English language. Some small errors are present (as many are present also in this review, I would bet on it :)), like page 336, line 18: it should be 'thermodynamic quantities', not 'thermodynamics quantities'. Moreover some parts of the paper, even if it is in general readable and clear, could be improved in fluidity. I think that a screening from a native English-speaker (if possible) would be helpful.

C127