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Earth System Dynamics Discussions

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

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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.

We wish to thank the referee for his comments which contributed improving the manuscript. You will find here a point by point response to these comments.

General comments:

1) Q - 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.

A - We agree that even though our original paper (submitted to the Quarterly Journal of the Royal Meteorological Society) is not needed to understand the present paper, it could be useful to make it available. We shall publish a draft version on Arxiv soon.

2) Q - 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.

A - We have added a short introduction to the MEP hypothesis (as well as references), especially in the context of climate studies, at the beginning of section 3.

3) Q - 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.

A - As explained above, this section (3.2) has been considerably improved in the revised version of the paper. We agree that speaking about a "0.5 dimension" can be confusing and as it is not important we have suppressed this expression: we only meant that the vertical dimension was greatly simplified (two layers) while the time dimension, even discretized, could be chosen with an arbitrary small time step, thus remaining a "true" dimension. Regarding the justification of the method, your question is legitimate and we have no formal proof that it should work. Nevertheless we argue that it appears as the "natural" extension of the MEP principle out of steady-state: we just apply Jaynes' general arguments to what we identify as the correct object to take into account the time derivatives of the temperatures: the instantaneous entropy production rate. In the same way the MEP principle selects the steady state for which the "gammas" maximize sigma, here we are looking for the "gammas", which are no longer mere numbers but now functions, which maximize sigma_i. As we discuss in the last paragraph of section 3.2 in the revision, what we show in the paper is that this approach works as "relaxation equations" in this particular example. Of course it would be nice to investigate more closely and more generally the range of validity of this method in the future, but the fact that it gives the correct stability is already very promising, both from a theoretical and from a practical point of view (as soon as one wants to use MEP in a problem with multiple equilibria, our "maximum instantaneous entropy production relaxation" provides a solution to compute the state of the system). In this view, we

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also suggest that another way to use MEP in a time dependent problem is to find the trajectory that maximizes the total (time-integrated) entropy production. In particular, when dealing with periodic phenomena (limit cycles), this approach is easier to implement in practice than the instantaneous EP (contrary to the stability case presented in the paper). We have tested this method with the seasonal cycle and it seems to work well.

Detailed comments:

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

A - This has been taken care of.

2) Q - 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;

A - Indeed, in reality the surface fluxes comprise sensible and latent heat, but in the MEP formulation the partition between the two components is not known. For this reason we prefer to refer to it in the general term "surface heat flux".

3) Q - 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;

A - We admit this notation might be misleading, we shall change this in the revision.

4) Q - 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);

A - We have made it clear in the revision that these equations are valid only at steady-state. However, it is relatively important to us that these equations are written at that

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place because this subsection presents the common material between the remaining of section 2 (the dynamical system approach) and section 3 (the MEP approach). In the dynamical system approach it is not a problem to treat the full equations with the time-derivatives, but this is not the case for the MEP approach and it is precisely the point of section 3.2.

5) Q - 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;

A - Thank you for noticing it, this is now corrected.

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

A - Thanks again for correcting this mistake.

7) Q - 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.

A - We admit that the notation was maybe misleading. Originally the purpose was to distinguish it from the time-integrated entropy production. It has been changed to sigma_i in the revision.

Interactive comment on Earth Syst. Dynam. Discuss., 1, 325, 2010.