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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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The paper "Entropy production and multiple equilibria: the case of the ice-albedo feedback" by Herbert et al. is a box-model study in which the authors make a comparison between a classical "dynamical system" approach and one based on the Maximum Entropy Production conjecture (MEP). It is well written and highlights some intriguing issues related to the relationship between stability and MEP showing the potential of MEP for studying glaciation. Moreover it originally extends the list of "box-model" MEP studies in order to account for the ice-albedo feedback and proposes a new, although questionable, application of MEP as an integration scheme.

We wish to thank the referee for his comments which helped improving the paper. A point by point response to these comments follows.

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General comments:

1) Q - The paper tries to build up a link between MEP states and equilibrium positions. In particular Sect. 3.2 ("Stability of the MEP states") reinforces the idea that the system is driven towards stable states that are also states of maximum entropy production, as already proposed by Shimokawa and Ozawa (2002). However there are some papers- Nicolis, QJRM (2003), 129, pp.3501-3504; Nicolis & Nicolis, QJRMS (2010), 136 pp.1161-1169- in which such an idea has been proved to be of limited generality and which the authors could consider in their discussion in order to give a fairer and more complete view on this subject;

A - Shimokawa and Ozawa (2001,2002,2007) suggest that when several steady-states are available, the system has a tendency to move to the one with the highest entropy production. Our conclusions are slightly different in at least two ways: when the entropy production function has several local maxima, (i) they all seem to represent steady-states of the system (whereas steady-states in Shimokawa and Ozawa are not necessarily local maxima of the entropy production rate) (ii) they are not all dynamically stable, but stability is not selected by the mere numerical value of the entropy production at the maximum but by the criterion we introduce in section 3.2. Point (ii) allows us to find several stable steady-state, in accordance with classical reasoning based on dynamical-systems theory. Note that the role played by the relative importance of the numerical value of the entropy production rate at local maxima does not seem to have an obvious role in this context. As a result our findings on this point meet with the conclusions of Nicolis (2003) and Nicolis and Nicolis (2010).

2) Q - Results from MEP and from the stabiltiy analysis of the dynamical system are different although qualitatively similar. This could be relevant when MEP is applied to paleoclimates, as noted by the authors in Sect. 3.3 where they discuss the surface fluxes but it could also be a severe limit for obtaining reliable estimates. Therefore some more discussion would be need.

ESDD

1, C138–C143, 2011

A - As you note, the quantitative differences between the MEP and the dynamical system approaches may constitute important difficulties when it comes to numerical results, especially in the framework of paleoclimate studies. The spirit of the present paper remains largely theoretical, and the aim is mainly to point at some new issues that might arise when using MEP in practical situations (here due to the presence of multiple equilibria). We suggest a method to overcome this difficulty in section 3.2, which seem to behave reasonably well (qualitatively speaking) in comparison with the dynamical system approach. Yet, due to the simplicity of the model, it would not be relevant to evaluate quantitatively the results. To that purpose, we are currently working on a more realistic version of the vertical model to continue the discussion of section 3.3 on a quantitative basis.

3) Q - The interpretation given of the MEP based scheme according to which time derivative can be treated as a flux and the time dimension can be considered as a geometric dimension is, frankly, confusing and, in the way it is explained, irrelevant. The material entropy production of the system is defined anyway even if the system is not in a steady state by the second term in eqn.(29) which, in the case of a system that is not in a steady state, depends on the time derivative of the local temperature, from which the integration scheme. If a deeper understanding is associated with such an interpretation, it should be stated more clearly;

A - The section devoted to the description of the technique that enables us to investigate the dynamic stability of the local maxima of entropy production found in section 3.1 is greatly improved in the revised version of the paper. In particular, after a short introduction to explain the need for this technique, we start by introducing the instantaneous material entropy production before going into the less familiar part. Still, we believe that it is very important to see that we treat the time derivative as if it were a “known“ flux (ie one that can be computed from the dynamic variables), exactly as radiative fluxes, in a bigger space (even if in the end the space upon which the maximization is done is not bigger since the extra variables necessary to compute the time derivatives are

ESDD

1, C138–C143, 2011

Interactive Comment

just the initial conditions). This interpretation makes the justification of the procedure more natural, as it appears closer to the familiar use of the MEP principle. To avoid confusion, we now present this interpretation in a slightly more detailed way at the end of the section.

4) Q - It should be said more clearly what is the original contribution of the first part of the paper (Sect.2).

A - Section 2 does not comprise fundamentally new results properly speaking; it is mainly a reformulation of the now widely admitted dynamical system viewpoint on the ice-albedo feedback. The only difference is that usually, this analysis is presented with a one box model (with no surface flux); here we introduce the surface flux and the bulk formula so that this model and the MEP model have the same structure. This also enables us to discuss the differences for the surface heat flux between the two model, which we believe to be of high relevance for paleoclimate studies.

Detailed comments:

a) Q - pp 328 line 5. Schulman (1977) has not investigated the vertical entropy production but the strength of the atmospheric energy cycle (rate of generation of available potential energy,G). Vertical fluxes of sensible heat are included, but to account for their contribution to the local diabatic heating (needed to work out G) and not their entropy production;

A - Yes, this was a mistake, thank you for pointing it out.

b) Q - it may be helpful for the reader to include a plot of the function representing the temperature dependent albedo (eq. 18);

A - We totally agree, we will add the graph in the revision.

c) Q - how is it possible that in Table 1 Cpa and Cpg are of the same magnitude if at page 331 line 20 it is claimed that Cpa Âń Cpg and thus Cpa=0?

A - Indeed, the value in the table for cpg was very low: it corresponded to a 1m thick land surface layer. Anyway, we make use of the approximation cpa « cpg only for the clarity of the presentation, but it has little influence on the steady-state results (only on the speed at which we reach steady-state). Actually it makes more sense to compute cpg for a 50m ocean layer as usually done for present climate. In that case it is clear that cpa « cpg. Since this is not a major point, we have modified the value of cpg in the table to avoid confusion and we explain in the caption that in reality, the value depends on the nature of the surface but it does not really matter for the results presented here.

d) Q - it may be helpful for the reader if the physical idea underlying the "net exchange formulation" could be briefly recalled in the paper;

A - This have been taken care of in the revision, we have added a brief reminder about the difference between the traditional flux formulation and the NEF.

e) Q - why to introduce in the paper the meridional heat transport (pp 329) whether this does not enter at all the following discussion based on a vertical model?

A - Since the reader is referred to our QJRMS paper for the details of the model, we mention the horizontal transfer here in the general presentation of the model to stay consistent with that paper. It seems important to us to make the link between the two papers so that it is clear that we are using the same model, just horizontally reduced to a single box here. We have added a sentence to explain clearly that here, we shall only consider the 0D version of the model, hence with the horizontal transports equal to zero.

f) Q - Observations at pp 328 lines 7-13 are not clear enough and a bit obscure, although they seem of great relevance.

A - The purpose of these remarks is just to mention that since the ice-albedo feedback is fundamentally a radiative process, there is no need to have horizontal dimensions in the model to study its basic effects. To keep the presentation simple, we thus choose

ESDD
1, C138–C143, 2011

Interactive Comment

the simplest possible case: the zero-dimensional model (as in mentioned references). The basic advantage is that we have a finite dimensional dynamical system (here the dimension is 2), with eigenvalues and eigenvectors from classical linear algebra. As soon as a continuous dependance on one coordinate (for instance latitude as in Ghil 1976) is introduced, we are dealing with an operator on an infinite-dimensional space and we need the Sturm-Liouville theory to discuss the properties of the (countably many) eigenvalues. In our opinion it is not necessary to introduce this mathematical complexity here since it is not necessary to the understanding of the physics of the problem.

g) Q - why is the material entropy production in eq. (29) written as a difference or increment (delta S)?

A - This notation was to make clear that we were not considering the integrated entropy production over the whole trajectory, but we agree that it might be confusing and we have changed the notation in the revision.

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

ESDD

1, C138–C143, 2011

Interactive Comment