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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.

Some scientific issues which I would like to point out:

1) 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 sys-

tem 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;

2) Results from MEP and from the stabilty 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.

3) 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 dimesion 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 cleaarly;

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

Technical and specific remarks

a) 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;

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

c) 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 « Cpg and thus Cpa=0?

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

e) 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?

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

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

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