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Earth System Dynamics Discussions

Interactive comment on "MEP solution for a minimal climate model: success and limitation of a variational problem" *by* S. Pascale et al.

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First I want to thank the Anonymous Referee #2 for his insightful comments. His technical and specific remarks were very valuable and I will take them into account in the revised version of the paper in order to make it more coherent and well-structured. However I would like to discuss some points which may be more controversial and for which more interactive comments may be helpful before revising the paper.

In general the referee stresses very much the difference between MEP as a conjectured physical principle and MEP as MaxEnt. However I do not believe that this dichotomy should be stressed to much as if the two were two completely different things. In Equilibrium thermodynamics in fact what is called the second principle of Thermodynamics (so a physical principle) has its final and deep justification in the statistical

interpretation given by Jaynes. So the two things coincide and you can equally consider the law of growth of entropy for isolated system either as a physical principle or a statistical consequence. In non-equilibrium Thermodynamics this reconciliation has not been done yet, although some previous claims (Dewar, 2003; Dewar, 2005).

However I welcome the referee's suggestions to deal with these ideas more "explicitly" in terms of the possible ways of interpreting MEP (principle vs. MaxEnt) although the paper's main aim is not meant to be the open interpretational issue of MEP but rather "to test the validity of the MEP conjecture using a combined vertical and meridional representation of the climate system". The parts of the paper which have generated confusion (as the abstract) have clearly to be altered and improved.

More specifically:

R- "P395, lines 25-26: the authors reason that "box-model MEP proofs" which consider only horizontal transport are incomplete because material entropy production due to vertical transport is numerically much larger. This reasoning is dubious and inconsistent: it has been applied previously to argue that MEP models which consider only material entropy production are incomplete because radiative entropy production is numerically much larger. The authors therefore appear to be inconsistent in their argument for including EP due to vertical transport when they do not also include radiative EP"

A- I partially disagree. In fact our interest is still confined to the "material" entropy production, not the radiative one, which is different in nature and has nothing to do with the climatic fluid. For three-dimensional system as well as in our models the total material entropy production (vertical+horizontal) is a well-defined quantity and in our opinion it is natural to consider it as far as MEP is concerned. However we agree that the total material entropy production may not be significant. For example it has been shown that the dissipation of kinetic energy (or generation of APE) may be a more useful quantity as far as MEP is concerned (Pascale et al, Climate Dynamics

DOI: 10.1007/s00382-011-0996-2) for the general circulation of the atmosphere. On the other hand the vertical component of the material entropy production is indeed a big component of the material EP, and therefore it seems natural to us to explore its role.

R- "P398, line 18: ...Only the positivity of the total EP is governed by the 2nd law. EP can be locally negative provided it is compensated by positive EP elsewhere. The authors should at least provide a reference to the origin of this criterion"

A- If we take "Non-equilibrium Thermodynamics" by de Groot&Mazur, chapter 3 equations (10)-(11) we can see that the local (material) entropy production for a continuum system associated with the entropy produced inside the system (eq. 2, same chapter) has to be positive. Then equation (21) of the same chapter gives an explicit formula for the local (material) entropy production, which includes: 1) diffusion of heat through a temperature gradient; 2) diffusion of chemical species across a chemical potential gradient; 3) dissipation of kinetic energy and 4) irreversible chemical reactions. In our four-box model the only process considered is the first one, so we only have to deal with heat fluxes and temperature gradients. The physical assumption when we require Sver1, Sver2 and Smer to be positive is that the heat always flows locally from a hot to a cold place if no external mechanical work is put into the system (which is not the case of the climate system).

R- "P399, last para: The significance of the "orthogonality" between horizontal and vertical EP is not clear. It seems mathematically trivial that when MEP is applied to a subsystem with the external fluxes fixed at the values obtained from MEP applied to the whole system, then the predicted sub-system fluxes are the same as those obtained from MEP to the whole system. So in practice there is no advantage in applying MEP to a subsystem, because one has to solve the full MEP problem anyway to get the correct fixed boundary conditions."

A- First, I note that the "orthogonality" itself is not a trivial feature and thus it is of

interest by itself. Second, this characteristic may turn to be useful in the case in which, for example, we know the boundary fluxes (for example because we have measured them), and we want to apply MEP to the remaining flux structure of the subsystem. Because of the orthogonality property we are sure that we can apply MEP to that subsystem.

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