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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.

Anonymous Referee #2

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General Comments:

The first two paragraphs of the comment by Referee #1 neatly summarize the paper. Therefore I will not repeat them here.

My first observation is that the objective of the paper is not clearly stated, which makes the paper's significance difficult to assess. From the abstract and Introduction it seems that the main purpose of the study is to test the validity of the MEP conjecture using a combined vertical and meridional representation of the climate system – the novelty being that previous "box-model MEP proofs" (p395, line 25) are incomplete (e.g. no vertical resolution) or are perceived by the authors to be unsatisfactory in other ways (e.g. "very sensitive to the parameterisation of humidity").

But at the same time, the scope of the paper seems wider than this. The abstract (lines 20-22) promises "[a] critical discussion about how to interpret MEP and how to apply it in a physically correct way". And the Introduction (p396, lines 23-25) promises a discussion of the authors' findings in relation to the view (Dewar 2009; Dyke & Kleidon 2010) that MEP is not a physical principle but an inference algorithm (i.e. MaxEnt). So I was also expecting the authors to address the wider (and still open) interpretational issue of MEP as physical principle vs MEP as MaxEnt.

Unfortunately this wider issue is hardly confronted at all, the promised "discussion" being both inadequate and confusing.

First the inadequacy. For the most part, the authors appear to view MEP exclusively as a conjectured physical principle, whose validity is therefore to be assessed by comparison with observational data (or, perhaps less convincingly, with other models considered representative of real climate). This is evident from phrases such as "whether ... climate can be explained by the [MEP] conjecture" (p394, lines 24-25); "[s]uch a hypothesis ... has been mainly tested" (p394/p395); "[w]hether the climate really is in a MEP state" (p395, lines 12-13); "box-model MEP proofs" (p395, line 25); "MEP validity" (p408, lines 11-12); "if the actual [FAMOUS] model solution is one of [MEP]" (p408, line 14); "[disagreement between MEP and FAMOUS] implies . . . that the real world is not in an MEP state" (p413, lines 10-11).

Alternatively, if MEP is MaxEnt then a quite different interpretation of the results is possible: when MEP predictions disagree with observations (or with a realistic model), it is the imposed physical constraints (e.g. boundary conditions, essential physics, model assumptions) that are invalidated, not MEP. However, when it comes to the "discussion" about the interpretation of MEP, this alternative view is given only a cursory mention (p414, lines 2-5).

The confusing aspect is that, a few lines previously (p413, lines 20-24: "importance of the boundary conditions and the model formulation", "proper physical ingredients"), the authors effectively come to the same conclusion as that implied by the MaxEnt interpretation of MEP – success of MEP predictions signals a realistic constraint set – although their subsequent “discussion” of MEP as MaxEnt fails to even recognise that connection.

The discussion of the results in relation to the interpretation of MEP (physical principle vs MaxEnt) therefore needs to be substantially enlarged and clarified. For example, in terms of MEP as MaxEnt, the unrealistic vertical structure obtained from MEP using energy balance constraints alone (no dynamics) implies that some relevant dynamical constraints are missing, rather than “the real world is not in an MEP state.” And the reference to “unconstrained and constrained MEP” (p414, line 7) simply misses the point: MEP is always a constrained optimisation problem; there is no such things as “unconstrained MEP”. Indeed, in terms of MEP as MaxEnt, the focus shifts entirely onto identifying the relevant constraints, with MEP playing a passive role. These points need to be brought into the discussion.

MEP as MaxEnt also has implications for the comparison between MEP and more detailed dynamical climate models (e.g. FAMOUS, HadCM3). Agreement between the two approaches would imply that some details of the dynamics (the ones not included in the MEP constraint set) are actually irrelevant to the features of the climate system under consideration. Conversely, disagreement signals that the MEP constraint set is incomplete. Agreement or disagreement will depend on the features being predicted, e.g. the present study suggests that the prediction of vertical convection requires a different constraint set (i.e. different essential physical ingredients) than horizontal transport. Again, this discussion is missing from the present paper.

Specific Comments:

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 inconsis-

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tent: 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. Although the issue remains unresolved, MEP as MaxEnt suggests that the relevant entropy production depends on the degrees of freedom resolved by the model to which it is applied, i.e. there is no universal entropy production that fits all applications of MEP (which MEP as physical principle might suggest).

P398, line 18: as far as I know, the criterion that the EPs associated with meridional, ver1 and ver2 transport are individually positive has no rigorous basis. 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 (e.g. a refridgerator). The authors should at least provide a reference to the origin of this criterion.

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 sub-system 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 applied to the whole system. So in practice there is no advantage in applying MEP to a sub-system, because one has to solve the full MEP problem anyway to get the correct fixed boundary conditions. Maybe I am missing something.

P404/405, section 4: although the quantification of the entropy budget (vertical vs horizontal) is of some background interest, I am not sure that it is central to the objective of the paper (partly because I am not sure what that objective is!). As noted above, the argument for including vertical EP based on its numerical domination over horizontal EP is in my view dubious (cf. radiative EP). Perhaps this part could be eliminated or at least relegated in importance.

P408/409, section 5.2: could the technique of horizontal and vertical averaging not also

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be applied to section 4.2 instead of using fictitious temperature distributions (T_NOHT etc)?

P410/411, section 6: The constraints don't make sense here, as referee #1 and the authors themselves note. Therefore it is not surprising that the results are unrealistic. This is just what we would expect from MEP as MaxEnt (i.e. rubbish in, rubbish out), but the authors fail to bring this point out .

P414, line 10: importance of planetary rotation rate. Jupp & Cox should be cited here. Again, the discussion here is cursory. Jupp & Cox showed that for regions of parameter space, inclusion of the additional dynamical constraints associated with rotation rate do not change the result. Another missed opportunity to discuss MEP as MaxEnt vs MEP as physical principle.

Technical Comments:

Title, abstract etc: I suggest 'simple' is better than 'minimal'.

P394, line 1. Insert 'The' before 'Maximum'.

P395, line 3: Herbert et al. 2010 is wrongly dated in the reference list.

P395, line 20: please cite references to 'extremal principles known in [f]luid [d]ynamics'.

P403, eqn (16): define symbol CS on the integral (presumably, climate system), although it would be better to use V (total volume of climate system) rather than the acronym CS. Whatever the notation adopted, it should also be applied to eqn (15).

P406, line 9: 'chapter' is unclear.

P407, line 6: should be 'associated with'.

P407, line 17: 'justify' should be 'characterise' or 'explain the existence of'.

P407, line 19: missing subscript on T_MEP.

P418, Table 1 legend: define Box 1 and Box 2 as the tropical and extra-tropical regions.

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Interactive comment on Earth Syst. Dynam. Discuss., 2, 393, 2011.