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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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The paper "MEP solution for a minimal climate model: success and limitation of a variational problem" by Pascale et al. considers the properties of the solutions of a 2-D box model of the climate system under different boundary conditions in the context of the validation/critical discussion of the Maximum Entropy Production (MEP) hypothesis.

The topic of the paper is interesting, scientifically relevant, but the paper itself suffers a severe lack of clarity, both in the definition of its goal(s), both in many parts of the actual derivation, discussion and interpretation of the results. The previous reviewers have already tackled most of the relevant scientific issues, therefore I will just add a couple of general comments (and a list of specific comments which I find important, in particular concerning the problem of the lack of clarity).

It seems to me that the main reason why the paper results unclear and difficult to follow is that at least three different topics are tackled, without a clear distinction between them. This leads to a substantial degree of confusion, in particular because the three different topic are of course related to each other.

One point is the importance of considering both the vertical and horizontal fluxes in a minimal box-model of the climate system. And therefore the discussion of the properties of such a model (with the specific formulation given in this paper as an example), in general and in the context of MEP, in particular the "orthogonality" property concerning the maximization of the entropy production.

A second point is the use of this model (in the 4-boxes or high-resolution version) in order to mimic a GCM climate using MEP with the boundary conditions for the box-model coming from the GCM.

The third point is the discussion related to the interpretation of MEP as an inference principle, with the use of this model in order to give examples of the inconsistency towards which a "religious" application of MEP can lead.

The results and the discussions related to these topics are mixed up in the paper in a way that makes very difficult for the reader to understand what the paper is about. The organization of sections and paragraph doesn't help either. I don't want to sound offensive (I have quite enjoyed what I've grasped of the intentions of the authors), but reading the paper (several times) I had the feeling of following the flow of thoughts of the authors on the general topic of MEP and the (numerous) experiments that they have done in this context, rather than a well structured exposition of scientific results and ideas.

Therefore I recommend the paper for publication after major revisions, part of which should consist in a substantial restructuring of many parts of the discussion, or even in the removal of some of them in order to enhance the focus (and therefore the clarity) of the paper. I entirely let to the authors how to proceed in this operation.

Specific comments:

Lines 40-42: specify that both the cited papers estimate the entropy production from model data and not from observational data;

Lines 42-43: a reference for this statement is needed. Moreover, here as well as in the rest of the paper the terms "meridional heat transport" and "horizontal heat transport" are used as they were the same. In today's climate most of the horizontal transport is indeed performed in the meridional direction, but in general the former is just a part of the latter: it would be better to clarify the notation (in particular considering that often, i.e. in line 87, the quantity S_{mer} is said to represent the entropy production due to the *horizontal* transport, which is not a very clean notation). This remark could seem pedantic, but note for example the discussion of the "orthogonality", one of the most interesting achievements of this paper, around line 100: does it refer to the vertical and horizontal (in general) components, or to the vertical and meridional (only)? With this notation it is not clear: in the model they are the same thing, in the real world (to which we want to extrapolate the results obtained with the model) they are not. Consider moreover that in different climates (i.e. a Snowball Earth, or on other planets) it is not so obvious that most of the horizontal transport should be in the meridional direction. Personally I would use the term "horizontal heat transport" (and therefore S_{hor}), maybe specifying that most of it is normally in the meridional direction;

Line 53: "vertical resolution" is a terminology which has sense in the context of numer-

ical modeling. Discussing in general the properties of the real atmosphere, it would be better to refer to the atmospheric vertical inhomogeneity, structure, composition, or similar terms;

Lines 65-66: something seems to be missing in this sentence;

Line 73: it would be better to explain where the values of the τs come from: now this is done only in table 1, it should be explained (also) in the text;

Line 70-75: the description of the model's equations it is honestly confusing. In particular it would be important to state clearly which quantities are state variables and which quantities are parameters of the model (this become somehow clear in the following, but I needed to read the formulation of the model a couple of times before being sure to have understood completely. More precision here would surely help the reader);

Line 89-91: also other processes contribute (even if maybe to a smaller amount) to S_{ver} in the real world: at least the turbulent dissipation of kinetic energy should be mentioned here;

Line 98: the location of the maximum depends on the values used for the τs. How much the choice of the τs affect the results of the model (in particular the existence and uniqueness of the maximum)?

Line 100: the model "HadCm3" should be presented (with references). In general any model or software used in producing or analysing results should be presented as soon as it is introduced, considering that the reader is not necessarily informed about the facilities in use in any specific research environment. The same holds in the following for the model "FAMOUS" and the software "IDL" (a description and references to HadCm3 and FAMOUS are indeed given in 140-145, but after their first introduction in the text);

Lines 117-119: honestly I don't understand the meaning of the sentence;

Lines 125-126: two different spellings are used in the title (generalization) and in the first line of the paragraph (generalisation). Stick to only one convention, double check the text for other cases like this;

Line 127: what do you mean exactly by "increasing the spatial resolution"? Do you increase the number of boxes in the horizontal (meridional) direction, in the vertical, both? How much? In particular, does the model remain 2-D? I mean, do you add boxes also in the zonal direction? It seems not, but it should be specified. Which is the goal of this operation? From now on the paper is honestly difficult to follow, maybe also because the starting point is (in my opinion) not well described;

Line 132: in which conditions is the reference state from FAMOUS computed (pre-industrial, present-day, ice-age, other...)? And how have the FAMOUS fields been coarse-grained in order to fit with the resolution (and dimensionality) of the box model? More details are needed. Moreover there is a bit of confusion about the model which is taken as a reference: the solar input here is taken from FAMOUS, before the values of the τs were taken from HadCm3, the results of the box model are sometimes compared with the FAMOUS climatology, sometimes to HadCm3 climatology. Why not using only one reference GCM (FAMOUS, considering that from now on HadCm3 is not used anymore)?

Line 156: again, "radiative scheme" is a terminology which normally refers to a numerical model like a GCM: "parametrization" or "model" would be preferable in this more general context;

Line 161: define OLR;

Line 163 (the line before eq. 10 and 11): define e. Note that in the following equations e_z and τ_z are used instead of e and τ. Be careful with the notation;

Lines 160-171, from "We deduce" to the end of the paragraph 3.1: are you computing these quantities at the FAMOUS model levels? Why? How are then these quantities used for the box model? Are we still using the box-model in this section? Honestly I

don't understand much of this part of the paper;

Line 173: again, "grid-box" of what? FAMOUS? The box model? The whole section 3 is really obscure to me, I'm sorry. The only reasonable idea is that you are taking for the box-model the same vertical and meridional resolution as in FAMOUS, and you prescribe the τs taking the zonal means of the correspondent fields in FAMOUS: whether this is the case or not, it should be explained explicitly;

Line 196: how long is the period over which the time-mean is considered?

Line 199: what does it mean to "estimate the horizontal and vertical components of the material entropy production"? In which conditions? For a realistic present day climate (I guess)? Then why should values coming from such extreme cases be representative of that case? Again, the goals of each section should be more clearly stated at the beginning of the section;

Line 204: as far as I've understood, now we are prescribing the temperature "field" for the box-model taking some extreme case studies, with the optical properties inferred (in a way that, I have to say, it is not clear at all) from a FAMOUS run which shows a completely different temperature field. The authors admit that this is a clear inconsistency, and that therefore the results should be taken as an order of magnitude analysis of the quantities of interest. A couple of sentences on why this approach should give reasonable order of magnitude results would be welcomed;

Lines 207-208: what does it mean "radiative solution"?

Line 210: it is not necessary to write the name of the subroutines used, it would be better to explain what IDL is and to give a reference for it;

Lines 217-225: at the beginning of section 4 it was said that the goal of the section is to estimate the horizontal and vertical components of the material entropy production. Which is the relevance of paragraph 4.1 in this sense?

Lines 227-228: why do we want to have such a solution? Again, it is not clear enough what the authors intend to do, and why;

Line 232: what does it mean exactly "model level means"?

Lines 235-236: I guess that you mean equal in each point (the global means are always equal on a climate mean, a part from spurious biases), better to specify it;

Line 237: specify the nature of this adjustment (why it is necessary, how much, applied on which fields, etc.);

Line 251: again, specify more precisely which kind of mean and how the adjustment is applied;

Line 257-259: again, why these extreme, ad hoc cases should provide any information about the behavior of these quantities in a realistic (for certain boundary conditions) case?

Lines 290-291: this trade-off justifies the existence of a maximum, but it tells nothing about the real climate trying or not to stay close to this state;

Line 293: T_{MEP} and T_{MEP};

Lines 314-316: I don't understand how do you come to this conclusion;

Line 317: something seems to be wrong in the title of paragraph 5.2

Line 324: the same symbol is used in line 196 to identify the time mean;

Lines 361-367: didn't we have the same problem in principle in the extreme cases of section 4? The temperature fields were inconsistent with the optical properties also there. Let us consider that the problem here is different because the inconsistency can be dramatic. The MEP hypothesis refers to steady states of a non-equilibrium system, in which the local thermodynamic equilibrium has to be fulfilled: it seems to me that the application of MEP to cases in which the local thermodynamic equilibrium does not hold is against the very definition of MEP. As the authors suggest in the conclusions,

the local thermodynamic equilibrium should be regarded as a condition of applicability of MEP just as the energy balance requirement, since they are both needed in order to have a steady state, and MEP (to my knowledge) is just a criterion to identify the most probable steady state among many (and therefore, indirectly, to tune the parameterization of unresolved or "unknown" processes by taking the maximizing values of the parameters). So, from my point of view, it is trivial that no local thermodynamic equilibrium $->$ no (physical) steady state $->$ MEP not applicable (meaning that you can still find maxima of course, but they do not represent preferential physical states of the system). I understand that this is also the position of the authors, and that section 6 is probably meant to give a practical example of this inconsistency, but, again, this is not stated clearly enough in the paper, confusing the reader on what the authors are actually doing and why. It seems to me also that this would be the argument in the discussion about the interpretation of MEP as an inference algorithm (MaxEnt): part of the discussion present in the conclusions should be moved also here, in order to make clear to the reader in which conceptual context (radically different from what has been done in the rest of the paper) the experiment of section 6 is performed;

Lines 423-424: something seems to be wrong in the sentence;

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