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Interactive Comment

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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In this manuscript, the authors consider Maximum Entropy Production (MEP) solutions of a simplified 2D (latitude-height) model with two different resolutions: a four-box model (2x2) and an increased resolution (which is not given in an unambiguous way, the reader assuming that it is the resolution of the zonally averaged FAMOUS model, i.e. 12x37) version. In both cases, they examine how realistic the MEP solutions are and discuss the entropy budget in MEP state.

The MEP principle is a very controversial hypothesis, with shadowy theoretical support. Its formulation and conditions of applications themselves are not yet well-posed. However, I do agree that a sensible approach may be to investigate empirically some specific cases, to sort out in what situations the MEP hypothesis seems to apply or not, either for practical use only or as a way to obtain theoretical insight.

My major criticism to this paper is that it is difficult to know how it fits in this general framework. What is the main contribution of the paper to the MEP debate, as compared to the existing literature ? The model in itself is very similar to those used in previous studies and models of this type have already been compared to observations and more complex models in the past. Hence the novelty of this study must lie somewhere else. There are some very interesting remarks, like the one concerning "orthogonality" between horizontal and vertical entropy production, or the detailed entropy budget (especially on the vertical dimension). It is also interesting to compare the MEP state to some limit cases for the temperature distribution (No temperature gradient, no heat fluxes,etc). To the best of my knowledge, these elements have not been put forward before in the MEP literature. Yet, it is not made clear by the authors if these are precisely the results they are presenting; on the contrary, they give the impression of discussing the general problem of the validity of MEP.

It seems to me that this problem is tightly linked to the fact that the structure and presentation are rather sloppy. For instance in section 4 and 5, it would certainly be beneficial for the reader to group the discussions of, on the one hand, how the different temperature fields TNOT, TNOH, TMEP,... are obtained and on the other hand, the analysis of the entropy budget. To put it differently, in the present form, methods and results are mixed in a way that confuses the reader. There is also much room available for language improvements: it is not only a matter of style, since some sentences seemingly important in the development of the authors' ideas are unclear.

Another concern of importance to me is the questionable relevance of the experiment presented in section 6. As far as I understand the MEP conjecture, it provides a way to compute without empirical parameterization some quantities (fluxes in most cases) associated to degrees of freedom whose macroscopic equations of motion are "unknown". Therefore I am not convinced that it makes sense to apply MEP optimization to quantities pertaining to radiative transfer, since the laws of radiative transfer are in fact "known". If it was to make sense in any way, I would expect one would have to include a certain number of “known” constraints, extracted from the “known” laws of radiative transfer. Otherwise this endeavour seems bound to fail. To illustrate this point, recall that albeit the major contribution to the global entropy production of the Earth is due to radiative processes, one only maximizes the contribution associated to turbulent heat fluxes in the “standard” MEP procedure.

For the reasons stated above, I think that a major revision will be necessary before the paper can be considered suitable for publication. Nevertheless, I am confident that all the issues raised above can be solved.

Specific Comments:

Page 394, L9: I do not like so much the word “degrees of freedom”: “resolution” would maybe me more down-to-earth and thus easier to understand (although I agree that in the context of the variational problem, we are indeed speaking of degrees of freedom)

Page 394, L24: Certainly it would be beneficial for the reader to be briefly reminded what the MEP conjecture is and how Paltridge applied it.

Page 398, Eq 9: Maybe it would be useful to explain briefly why this is the expression for the material entropy production rate.

Page 398, L12: If I am correct, there is no distinction between latent heat and sensible heat in the model and thus the vertical entropy production is due to the sum of the two.

Page 400, L2: “increase the spatial resolution”: it should be said, here or even anywhere else in the paragraph, what this resolution is.

Page 400, L2: Although I agree that the ocean interior be neglected out of modelling necessities, I am not confident that reasonings based on the numerical value of the entropy production associated with this process hold.

Page 403, L23: How do you obtain the value of TNOT ? Is it arbitrarily chosen or do you solve the global steady-state condition for a uniform temperature field ?

ESDD
2, C194–C198, 2011

Interactive Comment

Page 404, L7: Since you are computing a radiative equilibrium, all the vertical columns are independent and your numerical problem amounts to solving a set of only (N being the number of vertical levels) N+1 equations with N+1 degrees of freedom, and N=12 (if I am correct). Thus it is not clear to me why the latitudinal resolution is really a problem for the numerical procedure.

Page 405, L20: Is there a good reason not to consider also a profile with no vertical heat flux similarly to the horizontal case ?

Page 408, L13: “Nevertheless, we may say that if the actual model solution is one of maximum entropy production,...” I disagree with this sentence: if the longwave transmissivity varies with T, the MEP state for this model has no reason to coincide with the one obtained with prescribed longwave transmissivities. The only a priori statement one can make is that you expect the MEP state in the first case (longwave transmissivity varying with T) to be more realistic than for prescribed transmissivity.

Page 410, L19: “most of the states (..) will violate the local thermodynamic equilibrium,...” It would be necessary to develop on that point: how is the local thermodynamic equilibrium violated ?

Page 410, L21: “MEP leads to nonsense results..” As explained in the general comments, I am not convinced that varying the longwave transmissivity as an unconstrained parameter for MEP optimization makes sense in the first place.

Page 411, L8: If the entropy production rate differ so much for different initial conditions, it must be that the algorithm does not converge. Yet, it should not be a difficult task to patch up this problem, since the entropy production surface seems to be steep enough.

Page 411, L13-14: The fact that τMEP is either 0 or 1 (which are the bounds for this variable) seem to indicate that there is in fact no nontrivial maximum. Checking that with contour plots in a low-resolution model (for instance your 4 box model) would certainly make things more clear.

ESDD

2, C194–C198, 2011

Interactive Comment

Page 413, L8: “We note that MEP does not give us a temperature field...” Due to the fact that the optical properties of the atmosphere are fixed, one cannot honestly expect that the temperature field be consistent with these values, independently of the MEP conjecture. In fact the temperature field corresponding to radiative equilibrium is not consistent with these longwave transmissivities either. Neither would be any other model than FAMOUS with fixed transmissivities (obtained through FAMOUS) but different physics. Hence I do not believe you can draw conclusions as to the eventual validity of the MEP conjecture based on such grounds.

Page 413, L20: “In fact it is unrealistic to think of the longwave transmissivity as a variable independent from temperature” I fully agree with this. As a consequence, my opinion is that the only relevant experiment one could do is to look for a MEP solution in a model with longwave transmissivity depending on temperature, even in a crude parameterization.

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