Earth Syst. Dynam. Discuss.,
https://doi.org/10.5194/esd-2018-69-AC1, 2019

Earth System Dynamics Discussions
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Interactive comment on "A Radiative Convective Model based on constrained Maximum Entropy Production" *by* Vincent Labarre et al.

Vincent Labarre et al.

vincent.labarre@lsce.ipsl.fr

Received and published: 19 January 2019

Overview: The authors build on the model developed by Herbert et al (2013) (I note that the second and third authors have contributed to the earlier paper) to use the MEP conjecture to develop profiles for convective fluxes and temperatures. They add a further constraint, in explicitly expressing the energy transferred by convection as an upward and downward mass-flux times a quantity resembling moist static energy or some of its components. The atmospheric profiles that they obtain are compared with observations for various regions and sensitivities to atmospheric composition are obtained. The paper is an interesting addition to work already published on MEP, and I would recommend publication subject to the comments below.

Authors response 1:

We would like to thank the referee for reading our manuscript. We have read his remarks with great interest.

The paper suffers from poor presentation. It would benefit greatly from extensive copy-editing by someone with a good level of scientific English, paying particular attention to words that have a different meaning in French and in English, and to the use of the definite article. (In some places I wonder if Google Translate deserves credit as a co-author!).

Authors response 2:

Both referees have noted numerous grammatical errors. We want to apologize for that. We will do our best to correct the manuscript.

There are also occasional inconsistent uses of the decimal comma. There are also rather odd spaces before commas in citations, parentheses for citations in a context where only the date of publication should be in parentheses and on occasion no spaces either side of a full stop separating sentences. It is now unusual to see SI units separated by a full stop, but I presume that the journal has a house style which will state whether this is permitted or not. Likewise, atmospheric pressure is expressed here in terms of mB rather than the more usual hPa.

Authors response 3:

We will also perform a thorough check of the units used in the manuscript and reread the manuscript preparation guidelines to correct such errors.

I only explicitly mention a handful of corrections to the language used below, where I particularly wish to bring a point to the authors' attention.
Minor remarks
Page 2
L. 7 The word "ensemble" in climate science generally refers to a set of perturbed climate models which seek to establish the reliability of a forecast and is confusing here.

Authors response 4:

We will replace the word "ensemble" by "set" to avoid confusion.

L. 16 Not just geometric - some models may use varying albedo. L.19 It is not just the opacity of the surface, but the relative transparency of the atmosphere that is relevant.

Authors response 5:

We thank the referee for mentioning it, we will add these precisions in the next version of the manuscript.

L. 34 I would not regard "the absence of dynamics and/or the validity of MEP" as "one" reason to criticise MEP models.

Authors response 6:

We agree with the reviewer. Still, this is often the main critic arising from the "Fluid dynamics" community, and it is difficult to ignore it altogether. We will write "have been criticized" instead of "can be criticized".

Pages 8, 9, 12, 14
The captions to Figures 3, 4, 5 and 6 describe the plots in a different order to that presented, which is mildly irritating.

Authors response 7:

We will present the captions of the figures in the same order as the figures themselves.

Page 9
L. 13 - multiple energy profiles are presented; different constraints result in different characteristics. The authors should be more specific as to what they are describing.

Authors response 8:

This concerns page7 L.13. The represented energy profiles are the energy corresponding to the constraint. For C_pT we represent the profile $e = C_pT$, for $C_pT + gz$ we represent the profile $e = C_pT + gz$, and for for $C_pT + gz + Lq_s$ we represent the profile $e = C_pT + gz + Lq_s$. For $e = C_pT$, the energy per unit mass is trivially more important for hot regions. For $e = C_pT + gz$, the geopotential adds energy to upper layers. For $e = C_pT + gz + Lq_s$, the latent energy term adds energy to more humid layers. In all cases, the constraint imposes the direction of the flux (opposed to the energy gradient). If an energy flux in this direction is not favorable in term of entropy production, it vanishes and we have stratification. We will add these precisions in the next version of the manuscript.

L. 20. Clearly some thermal capacity is taken into account somwhere as C_p is not zero!

Authors response 9:

This concerns page10 L.20. The reviewer is right. We are here confusing thermal inertia (the term with time variations of temperature $C_p \frac{\partial T}{\partial t}$ which is equal to 0 in the stationary state) and thermal capacity. We will erase this point because it is not relevant to the comparison with reference profiles.

Page 13
L. 2 If the discretisation effect is important, have the authors satisfied themselves that N=20 is sufficient for their purposes?

Authors response 10:

We have been a little bit to fast on this point, but we have verified that N=20 is sufficient for our purposes. We will add the figure below that shows the temperature profiles for different values of N (for tropics) in the article. We observe that the method converges after $N \simeq 15$.

Page 16
L. 14 I am concerned about the linearisation assumption. Radiation emitted by a layer will have a quartic dependence on temperature, so the linearisation will only be valid

for a small perturbation. Is it possible to solve for T as a function of R other than by inverting a linear matrix?

Authors response 11:

We are not linearising the radiative budget. We are sorry for this misunderstanding. We are just using an iterative procedure to solve the full non-linear problem:
1) We linearize around a reference temperature profile.
2) Then we solve the linear constrained optimization problem.
3) Finally, we reiterate 1) by linearizing around the solution computed in 2) until convergence.
This is a rather standard procedure for optimization though there is no guarantee of finding the global optimum in case of multiple local maxima. In practise, the algorithm may fail for large N ($N \simeq 50$) but is robust for $N \leqslant 40$. We will explain this point more clearly in the future version of the manuscript.

Interactive comment on Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2018-69, 2018.

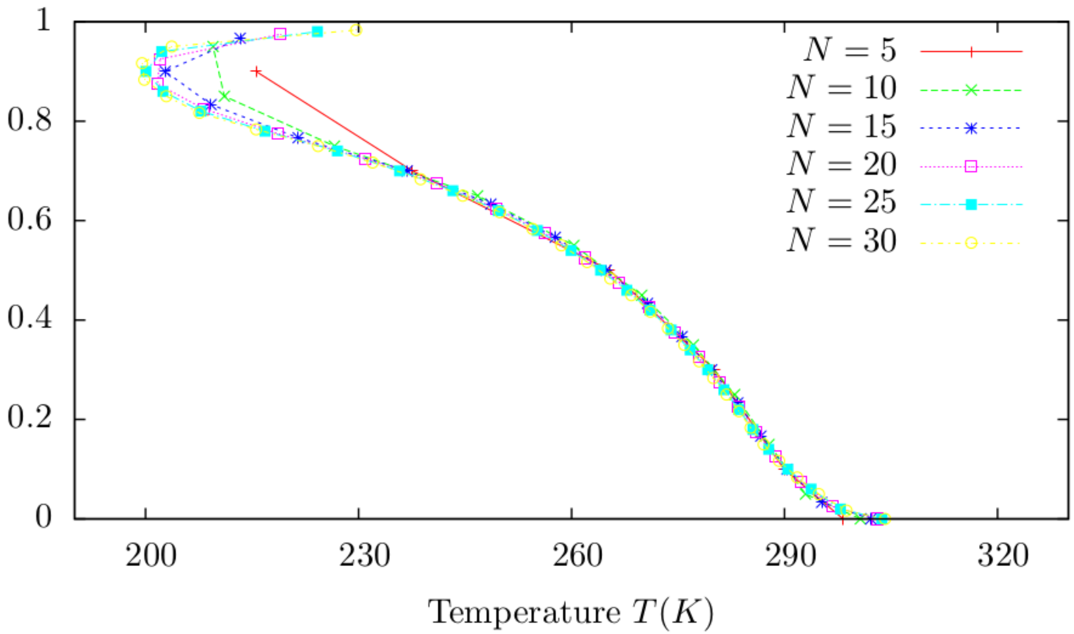

Fig. 1. Temperature vs dimensionless elevation for different numbers of layers N.

Interactive comment

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Discussion paper