Response to referee n.1

We thank the referee for his comments which led to improve the manuscript.

General comments

R- *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 ?............on the contrary, they give the impression of discussing the general problem of the validity of MEP.*

A- The main objectives of the paper are to 1) study a four-box model in order to address the vertical/horizontal entropy production issue (as raised in [2]) and understand how the horizontal/vertical components of the material entropy production are involved in the MEP problem. The referee is reminded of the fact that the choice of the four-box model is not arbitrary because this is what has been recently [2] suggested as a "minimal" climate box-model for studying MEP; 2) extend to a higher resolution model (still accounting for horizontal and vertical material entropy production) in order to better assess MEP results (by using a state-of-the-art GCM output); 3) discuss the horizontal/vertical splitting with comparison to the novel results by [2].

R- *It seems to me that this problem is tightly linked to the fact that the structure and presentation are rather sloppy....development of the authors' ideas are unclear* A- We have substantially reorganised and reshaped the manuscript, trying to be as clear as possible, by welcoming some of the referee's suggestions. For example we have grouped together the discussions on the vertical and horizontal splitting of the total material entropy production. The new structure of the manuscript is therefore the following: 1. Introduction; 2. Simple box-model for material entropy production: 2.1 The model; 2.2 MEP solution; 3. Increasing the resolution of the simple model: 3.1 Resolution; 3.2 Radiative parametrisaton; 3.3 Radiative heating rates and entropy production; 3.4 The variational problem and MEP solution; 3.4 Discussion of the MEP results; 4. Estimates of the vertical contribution to $\dot{S}_{mat}$; 4.1 By averaging over horizontal dimensions; 4.2 By constructing ad-hoc temperature fields; 5. Conclusions

R- *Another concern of importance to me is the questionable relevance of the experiment presented in section 6 ....one only maximises the contribution associated to turbulent heat fluxes in the "standard" MEP procedure*

A- The experiment in section 6 is supposed to serve as an example of the importance of the correctly definying the boundary conditions when using MEP. However, we agree that the way it was presented in the old manuscript made it look to be of more relevance than was intended. Therefore in the revised manuscript we have changed this part of the paper: on one hand we have removed Section 6 and greatly reduced the content of it (we also have removed Fig. 12, 13, 14); on the other hand we have moved the remaining text (which has also been partially altered) in the new section "Discussion of the MEP results " where we discuss the MEP results also under the MaxEnt's point of view on MEP. In that context we mention the case previously presented in Sect.6 as a non sense result that can be obtained if MEP is naively applied.

Specific Comments

- R- 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)*

  A- We agree with this remark and we have used the word "resolution" in place of "degrees of freedom";

- R- 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.*

  A- We have altered the text at the beginning of the introduction as "In 1975 G.W. Paltridge showed that the Earth's climate structure may be organised in a way to maximise its entropy production due to meridional heat transport ([4])"

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

  A- We have said that equation (9) comes from the general expression $\mathbf{F}\cdot\nabla(1/T)$ (put in a discreet form) for the entropy production associated with a heat flux $\mathbf{F}$ through a temperature gradient ∇T and added a reference for this [1];

- R- 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*

  A- Yes, the referee is right and we have altered this part of the text to reflect this;

- R- Page 400, L2: *"increase the spatial resolution": it should be said, here or even any where else in the paragraph, what this resolution is.*

  A- It means that we increase the number of "zones" in the meridional direction and the number of vertical levels (i.e. more atmospheric boxes). We have thus altered the text to explain this better.

- R- 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.*

  A- See reply to referee n.3 on this point;

- R- 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 ?*

  A- We have removed the previous subsection 4.1 ("Null entropy production") as it was irrelevant for the paper. Therefore there is no longer need to define TNOT at all;

- R- 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;*

  A- Same as in the previous remark;

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

  A- The reason is that it is not at all trivial to obtain such a profile. In fact in order to obtain such a temperature profile $(T_s, T_1, ...T_N)$ in our box-model we should solve the equation (see equation (19) in the manuscript):

  $$Q_{lw,z}[T_s, T_1, ..., T_N] + Q_{sw,z} + \partial M(y, z)/\partial y[T_s, T_1, ...T_N] = 0 \qquad (1)$$

  (the divergence of the vertical flux is null because the vertical flux itself is null) in which the first two terms are the longwave and shortwave heating rate of the atmospheric layer at the vertical level z and $M(y, z)$ the meridional heat flux at latitude y and vertical level z. Therefore we would need a relationship between M (or just $\partial M/\partial y$) and $[T_s, ...T_N]$. Indeed one could be obtained indirectly through eq. (1) -as generally done to work out the meridional heat flux -but this would just lead to a trivial identity. Apart from this, there is no alternative method to do it.

- R- 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 ?*

  A- The local thermodynamical equilibrium (LTE) holds when the intensive variables (e.g. temperature, pressure, density, ...) varys in space, but so smoothly that we can assume , in any small volume element, the thermodynamic equilibrium. Implicitly we always assume it when dealing with fluid dynamics, because without this ansatz the same concept of temperature will break down. Therefore if LTE does not hold, we could not even use the ideal gas law to relate the intensive variables (e.g. temperature, density and pressure) of a fluid(gas). In our case the infrared optical depth is mainly defined as ρk, where k depends on the gas (for example water vapour) and ρ is the density of the gas (although in reality it is much more complex because it also depends, for example, on clouds). Therefore a value of T not consistent with a value of ρ would mean a violation of the ideal gas law and therefore a violation of the local thermodynamical equilibrium;

- R- Page 410, L21: *" 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 optimisation makes sense in the first place.*

  A- See our reply in the General Comments part;

- R- 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.*

  A- We agree with the referee here and we add that this problem has remained essentially the same. We believe that this may be due to the fact that the maximum is not a local maximum but a non trivial one, as discussed in the next point. We have therefore added some more comments in the text to explain that thise behaviour should be taken as an indication rather than an exact solution. Furthermore, the whole previous Section 6 has been significantly reduced and given quite a different interpretation.

- R- Page 411, L13-14: *The fact that τ MEP 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*

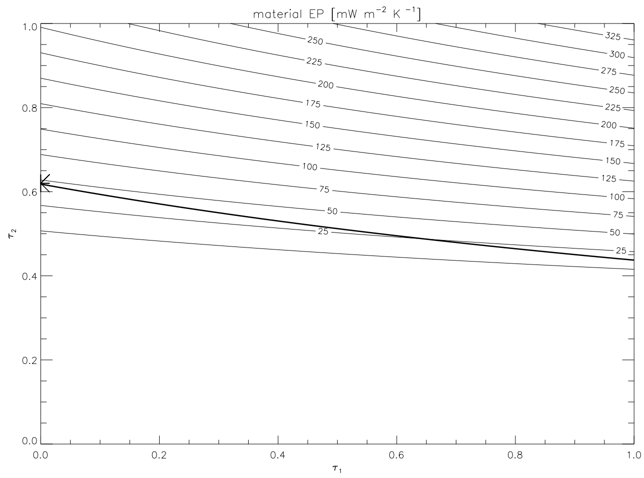

Figure 1: The plot shows lines of constant material entropy production in the (τ_1, τ_2) plane. The thick line denotes the values of (τ_1, τ_2) for which the energy balance holds and hence for which a steady state is defined. The maximum values at the end of such a line.

things more clear.

A- This is a very good comment and we have, to a certain extent, checked it. Rather than the 4 box model (which has only one atmospheric box) we have considered a vertical model (surface+2 atmospheric boxes). This is the minimal model to analyse what the referee is suggesting since we need at least two τ. For the sake of simplicity and in order to visualise the results we have fixed the temperature and allowed τ to vary. The maximum in entropy production is indeed obtained on the edge of the τ-domain (for a sensible choice of the temperatures we obtain $\tau_1 = 0$ and $\tau_2 = 0.65$, see Fig.1). This result is consistent with what we obtained in the previous Sec.6 (see for example Fig. 11(b)).

- R- 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*

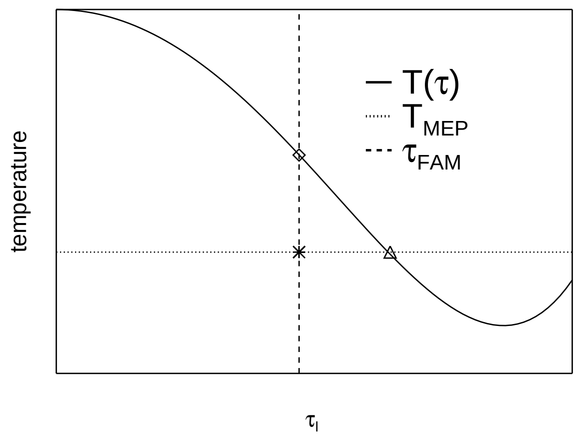

Figure 2: Schematic plot illustrating the relation between the MEP solution and the climatic state. The curve $T(\tau)$ symbolically represents the thermodynamically consistent steady states. The lozenge represents the FAMOUS solution, the asterisk the MEP solution and the triangle the steady state which would be thermodynamically consistent with the MEP temperature distribution.

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.

R- 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.*

R- 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 parameterisation.*

A- We are grateful to the referee for these three comments (since they are very similar we have grouped them together) which have led us to think more carefully about our conclusions. In order to discuss it more thoroughly we have introduced a new subsection, "Discussion of the MEP results ". A schematic view of what happens has been summarised by Fig.2. First we have noted that, with fixed optical properties, the MEP solution is still extraordinarily realistic, particularly in describing the meridional structure of the atmosphere and surface climatology. This circumstance has been explained by the fact that for a τ field not very far from the climatic one the inconsistency should not been too severe (this being the basis of many other studies on MEP, see e.g. [3], [6], [5]). Secondly, we have seen in the discrepancy between the climatic temperature profile and the MEP one, a signature of the fact that the zero-order approximation (fixed τ) is not good enough (not enough physics) to study the vertical structure whereas it is good enough (enough physics) for modelling the meridional one; third, by introducing the MaxEnt point of view on MEP, we discussed a possible way to interpret these agreement/disagreement. So we gave a more critical and controversial interpretation of our results rather than a yes/no response on MEP.

Bibliography

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