Response to referee n.3

General comments

R- *The conclusion of this paper is then obscure since we cannot judge whether the proposed conjecture is invalidated or whether some important physical processes/constraints are simply missing.*

A- We partially disagree about this conclusion, first of all because the aim of the paper is not testing MEP to a high degree of precision, rather studying a situation which takes into account both horizontal and vertical processes altogether trying to understand how vertical/horizontal material entropy production is involved in MEP. This is the element of novelty of this manuscript. In this respect the paper shows that the prediction of the vertical structure of the atmosphere needs different physical constraints than horizontal processes. The issue of how many physical constraints and which one to include in a MEP problem is not known a priori: the more we include, the more we constraint the problem thus reducing the possible steady states necessary for MEP to operate. It is therefore of interest to understand when "just enough physics" has been included. We admit however that the previous title might have been misleading and therefore we have changed it to "Vertical and horizontal processes and the Maximum Entropy Production conjecture".

R- *As far as this referee know, the temperature-opacity feedback was investigated by Pujol (2003), who assumed a fixed profile of relative humidity for the atmosphere and sought a MEP state in a radiative-convective model where the longwave opacity is a function of the temperature. His result shows the existence of a unique MEP state that is in close agreement with observations. The authors may be able to implement the same line of research in this respect. Or, at least, appropriate explanations about the temperature-opacity feedback and its consequences should be included in the discussion of this paper*

A- We are grateful to the referee for pointing out the paper by [3] about the role of the temperature-opacity feedback in the context of the MEP solution. Following the referee's suggestion we have used it in order to discuss our results in which temperature-opacity feedback is not implemented. In particular we have noticed and discussed the similarity between our solution and that found in [3] without such a feedback (e.g. near-surface convective instability) and the limitations of our approach. Furthermore such elements have been put in a more general context (new subsection 3.5) about the interpretational issue of MEP (following referee n.2's suggestions)

Specific comments

R- Page 400: *"The interior of the ocean is neglected since the material entropy production due to the small-scale eddy turbulence (1 $mW\ m^{-2}\ K^{-1}$) is negligible when compared to the material entropy production of the whole climate system.... " According to Paltridge (1978), the oceanic meridional entropy production is of the same order of magnitude as the atmospheric meridional entropy production (see Fig. 2 of Paltridge, 1978). Thus, even though small-scale eddy turbulence entropy production may be negligible, the overall contribution to entropy production due to the oceanic meridional heat transport cannot be omitted. Most probably, this omission results in an enhancement of the atmospheric meridional heat transport, which tends to reduce the surface temperature gradient to a realistic one. The situation should be explained in the text.*

A- The authors thank the referee for giving them the opportunity to clarify this point. It is true that in [1] the material entropy production due to ocean internal processes is estimated to be of the same order of magnitude as that of the atmospherie. However we have to point out that such estimate is clearly not realistic, since calculations performed with more refined models ([4, 2] and with direct methods (i.e. by estimating the entropy production directly from the diabatic heatings due to irreversible processes) show that this contribution is about 1 mW m^{-2} K^{-1}. This difference has, in our opinion, a very well defined reason: the fact that the ocean is much more "adiabatic" than the atmosphere and its motions are, to a very good approximation, adiabatic. Therefore in the ocean we have the situation in which most of its meridional heat transport takes place adiabatically (advection). In Paltridge's model the ocean meridional transport is modelled as a diffusive heat flux (not the case in reality) and this is why it has such an inappropriately high material entropy production. However it has to be said that the meridional heat transport due to the ocean is indeed comparable to the atmospheric one and surely it has a role in the regulation of the surface temperature. Therefore we have briefly explained the circumstance mentioned by the referee;

R- Equation (15): *material entropy production in terms of the radiative heating rates. This equation may deserve a further explanation. The actual meaning of this equation is the net entropy export rate by radiative processes (heating and cooling). If radiative heating (cooling) takes place, this rate is negative (positive). For a radiatively driven system, heating leads to an increase in the heating place temperature whereas cooling leads to a decrease in the cooling place temperature. The supplied energy will be transported by material processes (e.g. turbulence) in the system. In a steady state, the radiative heating and cooling rates should be balanced by the material energy transport rate (say, q), and the radiative entropy export is balanced by the material entropy production:* $\int Q/T dV = q/T_h + q/T_c = q(T_h - T_c)/(T_h T_c)$. *Thus, the material entropy production rate can be expressed by the radiative entropy export rate provided that the system is in a steady state. It would be good to add some explanations about the meaning and the limitation of this equation so that the reader can clarify the relation of Eq. (5) with other explicit expressions Eqs. (9) and (21).*

A- We agree with the referee on the fact that more explanation is needed for Equation (15), which holds only if the system is in a steady state. Therefore we have added some text to explain this important detail and clarify the relationship between equation (15) and the direct formula (e.g equation (5)) to express the material entropy production. Specifically the text added is: “ The validity of the inverse formula (based on radiative fields only) for expressing the material entropy production can be easily understood if we consider the *direct* formula (9). For a steady state the terms $M - H_1$, $H_2 - M$, H_1 and H_2 representing the material “diabatic” heating rates for each box of the model shown in Fig.1 can alternatively be expressed in terms of the local radiative heating through the equations (1)-(4). If the system is not in a steady state the material entropy production is still expressed by equation (9) but no longer by the inverse formula"

R- Page 407: *"The value of the material entropy production for MEP is* $S_{mat} 70\ mW\ m^{-2}\ K^{-1}$*... MEP2 is instead associated to an entropy production* $\approx 57\ mW\ m^{-2}\ K^{-1}$*. The reason of the large discrepancy in* S_{mat} *as well as those in temperature, heat flux and entropy production (Figs. 9b and 10c, d) is not clear. The difference seems to result from a slight difference in the prescribed emissivity profiles* $\epsilon(z)$ *between MEP and MEP2. If so, I would suggest checking the difference in* $\epsilon(z)$ *between MEP and MEP2. Also, since the distributions of* $\epsilon(z)$ *and* $\tau(z)$ *are prescribed from a GCM steady state (i.e. the temperature and humidity of a FAMOUS steady state), the validity of this assumption is limited to cases where the predicted temperature distributions are not very apart from the GCM mean state. The situation should be explained in the text in addition to the temperature-opacity feedback problem pointed out in the general comments.*

A- The only difference in MEP and MEP2 is, as noted by the referee, in the definition of the infrared emissivity. There is no other difference. Therefore we can safely say that the reason for the discrepancy in S_{mat} must be entirely due to differences in ϵ. By comparing ϵ from MEP2 (say $\epsilon_2(z)$) with the one from MEP ($\epsilon(z)$) we notice that ϵ_2 tends to be larger than ϵ in the middle-lower atmosphere (differences ~ 0.01) and smaller in the middle-upper atmosphere (differences $\sim\ 0.02$). Therefore the atmosphere in MEP2 is less "emissive" (i.e. less cooling) in the upper atmosphere and more emissive (more cooling) in the lower atmosphere. However it is not easy to explain in a simple way why in terms of these differences the MEP2 and MEP solution develops the differences in climate we have shown in the manuscript. In practice the differences are due to the differences in the radiative heating rates and temperatures shown by the two solutions. Let us note however how the meridional surface temperature and the meridional transport of heat is not affected by these differences, which almost entirely affects the vertical structure of the atmosphere.

Coming to the second point, we totally agree with the referee on the validity of our assumption (ϵ and τ fixed and taken from a GCM state) is limited to cases where the predicted temperature distributions are not very different from the GCM mean state. Therefore in the manuscript now we have explained this assumption both in Section 3.1 ("Experiment setup and material entropy production ") and in Section 4.2 (" Alternative order-of-magnitude estimates of $\dot{S}_{ver}$ and $\dot{S}_{hor}$").

Bibliography

[1] Paltridge, G. W. (1978). The steady state format of global climate. *Quarterly Journal of Royal Meteorological Society*, **104**, 927–945.

[2] Pascale, S., Gregory, J., Ambaum, M., and Tailleux, R. (2011). Climate entropy budget of the HadCM3 atmosphere-ocean general circulation model and FAMOUS, its low-resolution version. *Climate Dynamics*, **36**(5-6), 1189–1206.

[3] Pujol, T. (2003). Eddy heat diffusivity at maximum dissipation in a radiative-convective one-dimensional climate model. *Journal of the Meteorological Society of Japan*, **81**(2), 305–315.

[4] Shimokawa, S. and Ozawa, H. (2001). On the thermodynamics of the oceanic general circulation: entropy increase rate of an open dissipative system and its surroundings. *Tellus*, **53A**, 266–277.