Response to referee n.2

We are very grateful to the referee for his comments which have helped us to considerably clarify the manuscript.

General comments

A- The two main messages from the referee's comments on this manuscript are: 1) the fact that the objectives of the paper are not clearly stated; 2) the fact that the two possible way of interpreting MEP (principle vs. MaxEnt) are not clearly separated and dealt with in the paper, thus generating confusion/inadequacy. In the revised version of the manuscript we have therefore reshaped many parts of it in order to enhance the aims and avoid any confusion.

The main objectives of the paper is to 1) study a four-box model in order to address the vertical/horizontal entropy production issue (as raised in [3]). The referee is reminded of the fact that the choice of the four-box model is not arbitrary as this is what has been recently [3] suggested as a "minimal" climate box-model for studying MEP; 2) extension 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) Discussion of the horizontal/vertical splitting and comparison with the novel results by [3].

Coming to the second point raised by the referee, we are aware of the interpretational issue of MEP as stated recently by [1]. Although there is still no rigorous proof of it (as there is instead for equilibrium statistical mechanics, for which actually this dichotomy does not exist and the second law or principle of Thermodynamics has indeed a statistical foundation), we found that the interpretation of the MEP results in the paper can give good example of MEP as MaxEnt. Therefore we have: 1) discussed the MEP interpretational issue in the Introduction in order to make it clear to the reader which are, at the moment, the two ways MEP is interpreted (physical principle vs. MaxEnt); 2) introduced a new subsection in which the suggestions proposed by the referee (see in the responses to Specific

Comments) are brought into the discussion of the MEP results.

Specific comments

- R- P395, lines 25-26: *the authors reason that "box-model MEP proofs" which consider only horizontal transport are incomplete because material entropy production due to vertical transport is numerically much larger. This reasoning is dubious and inconsistent: it has been applied previously to argue that MEP models which consider only material entropy production are incomplete because radiative entropy production is numerically much larger. The authors therefore appear to be inconsistent in their argument for including EP due to vertical transport when they do not also include radiative EP. Although the issue remains unresolved, MEP as MaxEnt suggests that the relevant entropy production depends on the degrees of freedom resolved by the model to which it is applied, i.e. there is no universal entropy production that fits all applications of MEP (which MEP as physical principle might suggest).*

  A- We partially disagree with the referee here. We accept the fact, as discussed first by [2] and then by [4] and [5] that the radiative entropy production, which is due to the degradation of photons exergy by thermalisation, has little to to with the dynamics of the climatic fluid. However we believe that it makes absolutely sense to consider the whole material entropy production, since this is due to turbulent processes related to the motions of the climatic fluid. That is why we believe that the total material entropy production is a natural relevant quantity to consider in the context of MEP for our models. Therefore we do not think we are inconsistent in our argument, which is also raised by another paper published recently ([3]). Furthermore, we believe that it is of great scientific interest to understand the properties of the different terms of the material entropy production, as this will certailny help to understand better how to formulate a MEP problem. For example [6] shows that the generation of APE may be a better dissipative function to formulate MEP in GCM context whereas Paltridge only considered the entropy production due to meridional heat transport. Now we agree with the referee on the fact that MEP as MaxEnt may explain these situations (e.g. different constraints like the momentum equations may lead to a different dissipative functions to be maximised as the generation of APE) but while there is no rigorous proof of it, we think that it is sensible to mantain an empirical approach and investigate the different situations.

- R- P398, line 18: *as far as I know, the criterion that the EPs associated with meridional, ver1 and ver2 transport are individually positive has no rigorous basis. Only the positivity of the total EP is governed by the 2nd law. EP can be locally negative provided it is compensated by positive EP elsewhere (e.g. a refridgerator). The authors should at least provide a reference to the origin of this criterion.*

  A- Since this point was completely irrelevant to this paper, we have removed it;

- R- P399, last para: *The significance of the "orthogonality" between horizontal and vertical EP is not clear. It seems mathematically trivial that when MEP is applied to a subsystem with the external fluxes fixed at the values obtained from MEP applied to the whole system, then the predicted sub-system fluxes are the same as those obtained from MEP applied to the whole system. So in practice there is no advantage in applying MEP to a sub-system, because one has to solve the full MEP problem anyway to get the correct fixed boundary conditions. Maybe I am missing something.*

  A- We have clarified this point in the new draft. Here we did not mean that MEP has to be applied to a subsystem with the external fluxes fixed at the values obtained from MEP applied to the whole system. We meant instead that even though the whole system is not in MEP state, and therefore the fluxes external to the subsystem are not fixed at the values obtained from MEP applied to the whole system (consider, for example, Fig. 3(b) of the new draft), MEP could still be applied to the subsystem in order to predict its internal structure in terms of fluxes and temperatures. Two examples will explain it better. First, consider the atmosphere only in the four-box model. M is an internal flux (associated with its internal dynamics), H_1 and H_2 are the surface fluxes. Regardelss of the value of H_1 and H_2 (in fact we could also not know their value), MEP can be applied to the atmospheric submodel to predict M 30 $\mathrm{W\,m^{-2}}$ (Fig. 3(b) on the new draft). Second, let us consider a vertical subsystem, say the tropical one (labelled 1 in the manuscript). It has got one internal flux H_1 whereas M is a boundary flux. Again MEP can still be used for predicting the internal flux H_1 120 $\mathrm{W\,m^{-2}}$ because $\dot{S}_{1,ver}$ is quasi-independent of M (Fig.4(a) of the new draft).

- R- P404/405, section 4: *although the quantification of the entropy budget (vertical vs horizontal) is of some background interest, I am not sure that it is central to the objective of the paper (partly because I am not sure what that objective is!). As noted above, the argument for including vertical EP based on its numerical domination over horizontal EP is in my view dubious (cf. radiative EP). Perhaps this part could be eliminated or at least relegated in importance.*

  A- In the reorganisation of the paper we have merged this part on the quantification of the vertical vs horizontal entropy budget with Sect. 5.2. (in which we use a different technique to achieve the splitting) which deals with a similar topic. Furthermore, it has been considerably shortened, as asked by the referee.

- R- P408/409, section 5.2: *could the technique of horizontal and vertical averaging not also be applied to section 4.2 instead of using fictitious temperature distributions (T_{NOHT} etc)?*

  A- Yes, this has been done already (as could be seen in Table 3), although we did not mentioned it in the paper. We have done it now and the results are of the same order of magnitude as those obtained with the ad hoc temperature distributions.

- R- P410/411, section 6: *The constraints don't make sense here, as referee 1 and the authors themselves note. Therefore it is not surprising that the results are unrealistic. This is just what we would expect from MEP as MaxEnt (i.e. rubbish in, rubbish out), but the authors fail to bring this point out.*

  A- We totally agree with the referee and in order to address his requirements we have radically modified this part. First, we have completely removed section 6 (" Varying temperature and τ simultaneously ") from the manuscript; second, we have significantly reduced the content of Section 6 and moved the remaining text in the new Section 3.4 (Discussion), in which now we discuss the MEP results for the box-model and, more generally, how to formulate and interpret them. In this section therefore we discuss Dewars's view on MEP (MaxEnt, [1]) and we use the experiment previously described in Section6 (T and τ freely variable) as an example which may support MaxEnt.

- R- P414, line 10: *importance of planetary rotation rate. Jupp/Cox should be cited here. Again, the discussion here is cursory. Jupp/Cox showed that for regions of parameter space, inclusion of the additional dynamical constraints associated with rotation rate do not change the result. Another missed oppor tunity to discuss MEP as MaxEnt vs MEP as physical principle.*

  A- This part has been removed because it is unnecessary for the rest of the paper

Technical Comments

Title, abstract etc: I suggest "simple" is better than "minimal".

P394, line 1. *Insert "The" before "Maximum".*

P395, line 3: *Herbert et al. 2010 is wrongly dated in the reference list.*

P395, line 20: *please cite references to "extremal principles known in [f]luid [d]ynamics".*

P403, eqn (16): *define symbol CS on the integral (presumably, climate system), although it would be better to use V (total volume of climate system)* *rather than the acronym CS. Whatever the notation adopted, it should also be applied to eqn (15).*

P406, line 9: *"chapter" is unclear.*

P407, line 6: *should be "associated with".*

P407, line 17: *"justify" should be "characterise" or "explain the existence of".*

P407, line 19: *missing subscript on T_{MEP}.*

P418, Table 1 legend: *define Box 1 and Box 2 as the tropical and extra-tropical regions.*

A- we have made the necessary corrections to address the referee's technical comments.

Bibliography

[1] Dewar, R. C. (2009). Maximum entropy production as an inference algorithm that translates physical assumption into macroscopic predictions: don't shoot the messenger. *Entropy*, **11**, 931–944.

[2] Goody, R. (2000). Sources and sinks of climate entropy. *Quarterly Journal of the Royal Meteorological Society*, **126**, 1953–1970.

[3] Lucarini, V., Fraedrich, K., and F.Ragone (2011). Thermodynamical properties of planetary fluid envelopes. Journal of Atmospheric Sciences, in press, doi: 10.1175/2011JAS3713.1.

[4] Ozawa, H., Ohmura, A., Lorenz, R., and Pujol, T. (2003). The second law of thermodynamics and the global climate system: a review of the maximum entropy production principle. *Reviews of Geophysics*, **41**(4), doi:10.1029/2002RG000113.

[5] Paltridge, G. W. (2001). A physical basis for a maximum of thermodynamic dissipation of the climate system. *Quarterly Journal of the Royal Meteorological Society*, **127**, 305–313.

[6] Pascale, S., Gregory, J., Ambaum, M., and Tailleux, R. (2011). A parametric sensitivity study of entropy production and kinetic energy dissipation using the FAMOUS AOGCM. *Climate Dynamics*, pages doi 10.1007/s00382–011–0996–2.