

Response to referee n.4

We thank the referee for the many valuable comments which we believe led to a substantial improvement of the clarity of this manuscript.

General comments

R- the paper itself suffers a severe lack of clarity, both in the definition of its goal(s), both in many parts of the actual derivation, discussion and interpretation of the results.....It seems to me that the main reason why the paper results unclear and difficult to follow is that at least three different topics are tackled, without a clear distinction between them.....Therefore I recommend the paper for publication after major revisions, part of which should consist in a substantial restructuring of many parts of the discussion, or even in the removal of some of them in order to enhance the focus (and therefore the clarity) of the paper

A- We agree with the referee about the lack of clarity of the first draft of the paper. As a consequence we have reorganised the structure of the manuscript and removed or altered many parts of the text. In particular we have avoided the mixing of the three topics as noted by the referee. The actual organisation of the text has now the following content structure: 1) study of the four-box model in order to address the vertical/horizontal entropy production issue (as raised in [2]); 2) increase of the resolution in order to better assess MEP results (by using a state-of-the-art GCM output). We remind the referee of the fact that 2) is linked to 1) since also in the higher resolution model we maximise the total material entropy production which does include both the entropy production contributions due to the vertical and horizontal motions; 3) interpretation of the MEP results; 4) discussion of the horizontal/vertical splitting and comparison with the novel results by [2]. Part (4) has been moved at the end of the paper and merged with Section 5.2 of the previous manuscript as required by referee n.1 too. We believe that the structure is now much more coherent. The new sections are now in the following order: 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 parametrisation; 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

Specific comments

- R- Lines 40-42: *specify that both the cited papers estimate the entropy production from model data and not from observational data*

A- We have altered the text in the following way “The total material entropy production of the climate system has been estimated from general circulation models to be about $50 \text{ mW K}^{-1} \text{ m}^{-2}$ ”

- R- Lines 42-43: *a reference for this statement is needed. Moreover, here as well as in the rest of the paper the terms meridional heat transport and horizontal heat transport are used as they were the same. In today’s climate most of the horizontal transport is indeed performed in the meridional direction, but in general the former is just a part of the latter: it would be better to clarify the notation (in particular considering that often, i.e. in line 87, the quantity S_{mer} is said to represent the entropy production due to the horizontal transport, which is not a very clean notation). This remark could seem pedantic, but note for example the discussion of the “orthogonality”, one of the most interesting achievements of this paper, around line 100: does it refer to the vertical and horizontal (in general) components, or to the vertical and meridional (only)? With this notation it is not clear: in the model they are the same thing, in the real world (to which we want to extrapolate the results obtained with the model) they are not. Consider moreover that in different climates (i.e. a Snowball Earth, or on other planets) it is not so obvious that most of the horizontal transport should be in the meridional direction. Personally I would use the term “horizontal heat transport” (and therefore S_{hor}), maybe specifying that most of it*

is normally in the meridional direction

A- Proper references have been included ([1, 3, 2]). We agree on the coherent use of the terms horizontal and meridional asked by the referee and in the new version of the manuscript we use the term “horizontal” only;

- R-Line 53: *“vertical resolution” is a terminology which has sense in the context of numerical modelling. Discussing in general the properties of the real atmosphere, it would be better to refer to the atmospheric vertical inhomogeneity, structure, composition, or similar terms*

A- We replaced “vertical resolution” with “vertical thermal structure”;

- R- Lines 65-66: *something seems to be missing in this sentence*

A- This part of the text has been substantially modified;

- R- Line 73: *it would be better to explain where the values of the τ s come from: now this is done only in table 1, it should be explained (also) in the text*

A- We have briefly explained it in the text as follows: “Their values (see Table 1) have been worked out from a control run obtained with the FAMOUS AOGCM after defining a tropical and extratropical box;

- R- Line 70-75: *the description of the model’s equations it is honestly confusing. In particular it would be important to state clearly which quantities are state variables and which quantities are parameters of the model (this become somehow clear in the following, but I needed to read the formulation of the model a couple of times before being sure to have understood completely. More precision here would surely help the reader)*

A- We have clearly stated which are the parameters of the model and clarified the description of the model;

- R- Line 89-91: *also other processes contribute (even if maybe to a smaller amount) to Sver in the real world: at least the turbulent dissipation of kinetic energy should be mentioned here*

A- We totally agree on this point and we have mentioned also the turbulent dissipation of kinetic energy;

- R- Line 98: *the location of the maximum depends on the values used for the τ s. How much the choice of the τ s affect the results of the model (in particular the existence and uniqueness of the maximum)?*

A- This is a very good point and we thank the referee for raising it. The location and the height of the maximum in the material entropy production depends on the values used for both the optical parameters τ_l and τ_s . For $0.001 < \tau_l < 0.4$ (which corresponds to the physical range of optical depth $0.5 < \exp(-\tau_l) < 7$ investigated by [4]), we note that a unique maximum in \dot{S}_{mat} always exists. The location of M_{mep} is fairly insensitive to τ_l (from 34 W m^{-2} for $\tau_l = 0.001$ to 31 W m^{-2} for $\tau_l = 0.4$) as well as \dot{S}_{hor} (from $7.5 \text{ mW m}^{-2} \text{ K}^{-1}$ to $6.8 \text{ mW m}^{-2} \text{ K}^{-1}$). The location of $(H_{1,mep}, H_{2,mep})$ is instead more sensitive to τ_l and it varies from $(115, 59) \text{ W m}^{-2}$ to $(59, 7) \text{ W m}^{-2}$. \dot{S}_{mat} (and particularly \dot{S}_{ver}) is the highest for $\tau_l = 0.001$ ($30 \text{ mW m}^{-2} \text{ K}^{-1}$) and the lowest for $\tau_l = 0.4$ ($18 \text{ mW m}^{-2} \text{ K}^{-1}$). As far as τ_s is concerned, again a unique maximum in \dot{S}_{mat} exists in a wide range for τ_s . If we take $\tau_s = 1$ (atmosphere completely transparent to solar radiation) the MEP maximum is found for $(M_{mep}, H_{1,mep}, H_{2,mep}) \approx (36, 166, 93) \text{ W m}^{-2}$ whereas for $\tau_s = 0.1$ (case of an atmosphere extremely opaque to solar radiation) the peak in the material entropy production is placed at $(M_{mep}, H_{1,mep}, H_{2,mep}) \approx (31, 13, 5) \text{ W m}^{-2}$;

- R- Line 100: *the model "HadCM3" should be presented (with references). In general any model or software used in producing or analysing results should be presented as soon as it is introduced, considering that the reader is not necessarily informed about the facilities in use in any specific research environment. The same holds in the following for the model "FAMOUS" and the software "IDL" (a description and references to HadCm3 and FAMOUS are indeed given in 140-145, but after their first introduction in the text)*

A- The referee is right, therefore we have moved all the information and

the references about the model in the new section 2.1 when FAMOUS is mentioned for the first time. In particular the part of text used to present the model is:” FAMOUS, (Jones et al. 2005; Smith et al. 2008) is the low resolution version of HadCM3 (Gordon et al. 2000, Pope et al. 2000), which has been widely used to simulate present day and future climate and compares well with current general circulation models and observations (Reichler and Kim, 2008). FAMOUS solution can be considered a relatively good representation of real climatology, as shown in (FAM)”;

- R- Lines 117-119: *honestly I don't understand the meaning of the sentence*

A- The notion of “local maximum” is known from elementary analysis of real functions and we are not going to repeat it in the paper; the rest of the text has been altered and now the meaning should be evident;

- R- Lines 125-126: *two different spellings are used in the title (generalisation) and in the first line of the paragraph (generalisation). Stick to only one convention, double check the text for other cases like this*

A- We agree, and we took care of this;

- R- Line 127: *what do you mean exactly by "increasing the spatial resolution"? Do you increase the number of boxes in the horizontal (meridional) direction, in the vertical, both? How much? In particular, does the model remain 2-D? I mean, do you add boxes also in the zonal direction? It seems not, but it should be specified. Which is the goal of this operation? From now on the paper is honestly difficult to follow, maybe also because the starting point is (in my opinion) not well described*

A- We recognise that we could have been much more clear here. We have altered the text as follows: “ we consider an extension of the spatial resolution of the climate model..... We maintain the same physics but increase the number of boxes in the meridional and vertical direction (so the model remains zonal). In particular we consider eleven

vertical boxes in order to make them coincide with FAMOUS atmospheric vertical layers. The meridional resolution will be defined by 17 boxes (11.25°).” In the revised manuscript we also have explicitly state the goal of this operation ”In order to have a model which is more easily comparable with FAMOUS climatology”;

- R-Line 132: *in which conditions is the reference state from FAMOUS computed (preindustrial, present-day, ice-age, other...)? And how have the FAMOUS fields been coarse-grained in order to fit with the resolution (and dimensionality) of the box model? More details are needed. Moreover there is a bit of confusion about the model which is taken as a reference: the solar input here is taken from FAMOUS, before the values of the taus were taken from HadCm3, the results of the box model are sometimes compared with the FAMOUS climatology, sometimes to HadCm3 climatology. Why not using only one reference GCM (FAMOUS, considering that from now on HadCm3 is not used anymore)?*

A- We have added the details required by the referee. In particular, we consider a FAMOUS control run obtained with pre-industrial CO₂ concentration; FAMOUS has a zonal grid spacing of 5° so its fields have been regridded by area-averaging. We agree with the referee about the confusion about the models (FAMOUS, HadCM3) which has been generated by the fact that the two models are the same but with different resolution; however we have used only FAMOUS data and so we have dropped the term HadCM3 from the manuscript.

- R- Line 156: *again, "radiative scheme" is a terminology which normally refers to a numerical model like a GCM: "parametrisation" or "model" would be preferable in this more general context*

A- We agree, we have used the word “model” in the new version of the manuscript;

- R- Line 161: *define OLR* A- We have written it in full extension;
- R- Line 163 (the line before eq. 10 and 11): *define e . Note that in the following equations e_z and τ_z are used instead of e and τ . Be careful*

with the notation

A- Thanks, we have taken care of this in the new version of the paper;

- R-Lines 160-171, from "We deduce" to the end of the paragraph 3.1: *are you computing these quantities at the FAMOUS model levels? Why? How are then these quantities used for the box model? Are we still using the box-model in this section? Honestly I don't understand much of this part of the paper*

R-Line 173: *again, "grid-box" of what? FAMOUS? The box model? The whole section 3 is really obscure to me, I'm sorry. The only reasonable idea is that you are taking for the box-model the same vertical and meridional resolution as in FAMOUS, and you prescribe the τ s taking the zonal means of the correspondent tau in FAMOUS: whether this is the case or not, it should be explained explicitly*

A- The two previous remarks have been answered at the same time as they are very similar. The referee is right, we are taking for the box-model the same vertical resolution as in FAMOUS and we prescribe τ by taking the zonal means of the correspondent τ in FAMOUS. In the revised version we have stated this very explicitly in order to avoid confusion and misunderstanding;

- R-Line 196: *how long is the period over which the time mean is considered?* A- 30 years;
- R- Line 199: *what does it mean to "estimate the horizontal and vertical components of the material entropy production"? In which conditions? For a realistic present day climate (I guess)? Then why should values coming from such extreme cases be representative of that case? Again, the goals of each section should be more clearly stated at the beginning of the section*

R-Line 204: *as far as I've understood, now we are prescribing the temperature "field" for the box-model taking some extreme case studies, with the optical properties inferred (in a way that, I have to say, it is not clear at all) from a FAMOUS run which shows a completely*

different temperature field. The authors admit that this is a clear inconsistency, and that therefore the results should be taken as an order of magnitude analysis of the quantities of interest. A couple of sentences on why this approach should give reasonable order of magnitude results would be welcomed

R-Lines 227-228: *why do we want to have such a solution? Again, it is not clear enough what the authors intend to do, and why*

R-Line 257-259: *again, why these extreme, ad hoc cases should provide any information about the behaviour of these quantities in a realistic (for certain boundary conditions) case?*

A- We reply to these four points at the same time since they are very similar. The overall goal of this part is to obtain an estimate of the splitting of the material entropy production between contributions due to vertical and horizontal processes. First, to "estimate the horizontal and vertical components of the material entropy production" means to estimate the material entropy production due to vertical processes and the material entropy production due to large scale horizontal processes, as described also, for example in [2] to which the referee can refer to for more details. The conditions are those of a present-day climate. The "ad hoc" cases are just order-of-magnitude estimates and their validity relies on the fact that they are not very apart from the GCM mean state. We have added these comments in the revised manuscript and compared these estimates with those obtained directly from the FAMOUS output by using the averaging technique describe in Sect. 5.2 of the old manuscript.

- R- Lines 207-208: *what does it mean "radiative solution"?* A- The whole subsection "Null entropy production" has been removed from the manuscript;
- R- Line 210: *it is not necessary to write the name of the subroutines used, it would be better to explain what IDL is and to give a reference for it* A- We have given a very brief description of what IDL

is (“an array-orientated data analysis environment widely used in climate sciences”). However we believe that it may be useful to name the optimisation subroutine, which is fully explained on the IDL online help (reported on the manuscript);

- R- Lines 217-225: *at the beginning of section 4 it was said that the goal of the section is to estimate the horizontal and vertical components of the material entropy production. Which is the relevance of paragraph 4.1 in this sense?*

A- We totally agree with the referee, in fact we have removed it;

- R- Line 232: *what does it mean exactly “model level means”?*

A- FAMOUS has 11 vertical levels and for each of those levels we have a surface over which the various fields are defined; so for example the temperature is a field which is defined on 11 surfaces at each vertical level. “Model level means” thus means the array of area-averaged means over each of those surfaces.

- R- Lines 235-236: *I guess that you mean equal in each point (the global means are always equal on a climate mean, apart from spurious biases), better to specify it*

A- Yes, we meant that. We have specified it in order to avoid confusion;

- R- Line 237: *specify the nature of this adjustment (why it is necessary, how much, applied on which fields, etc.)*

R- Line 251: *again, specify more precisely which kind of mean and how the adjustment is applied*

A- We thank the referee for these two points. In the revised manuscript we have now given much more detail about how the “ad hoc” temperature fields are obtained. I have quoted some of it “ First, let us consider \dot{S}_{ver} . If we consider a temperature field with no meridional temperature gradients (we call NOHT, i.e. NO Horizontal Temperature gradient) then $\dot{S}_{hor} = 0$ by construction and $\dot{S}_{mat} = \dot{S}_{ver}$. Such a field is obtained by replacing the temperature field from FAMOUS

climatology with one which is horizontally uniform over each vertical level. The uniform value of the temperature over each model vertical level is obtained by taking its surface mean over the surface at that level. Second, we consider a temperature field T_{NOHH} which has the characteristic of producing a TOA longwave radiation equal in each point to the net incoming shortwave one. This is obtained as $T_{NOHH} = \alpha^{1/4} T_{CLIM}$, where $\alpha = SW/LW$ is the ratio between the magnitude of the net shortwave and longwave fluxes at the top of the atmosphere from the FAMOUS climatology. In fact if we assume heuristically $LW_{NOHH} \sim \sigma T_{NOHH}^4$, then $LW_{NOHH} \sim SW$ and also “ Second, let us consider a temperature field T_{NOVT} which is vertically homogeneous (NOVT, NO Vertical Temperature gradient): the temperature is constant throughout each vertical column ($\partial_z T = 0$, including the surface as well) but with a meridional gradient. Such a field is defined as $T_{NOVT} = \int \rho dz T_{CLIM} / \int \rho dz - 24$ (where the -24 degree term is needed to satisfy the energy balance”

- R- Lines 290-291: *this trade-off justifies the existence of a maximum, but it tells nothing about the real climate trying or not to stay close to this state;*

A- We agree, this remark is useless within the context of the discussion and we have removed it;

- R- Line 293: T_{MEP} and T_{MEP} A- Corrected
- R- Lines 314-316: *I don't understand how do you come to this conclusion*

A- The referee's comment clearly stems from the fact that our discussion at lines 314-316 was very short and cursory. Therefore we are happy about this comment (see also Referee n.1) because it has given us the possibility of expanding this part and analysing it in a much more detailed way. In particular we have added a new subsection (Discussion of the MEP results, see in General comments' reply) in which we discuss how to interpret the MEP solutions and what they can tell

us about the real climate (for more details see the response to referee n.1's remark -Page408,L13).

- R- Line 317: *something seems to be wrong in the title of paragraph 5.2*
A- The title has been changed anyway;
- R- Line 324: *the same symbol is used in line 196 to identify the time mean;* A- We use now a different symbol, namely $\langle \rangle$
- R- Lines 361-367: *didn't we have the same problem in principle in the extreme cases of section 4? The temperature fields were inconsistent with the optical properties also there. Let us consider that the problem here is different because the inconsistency can be dramatic. The MEP hypothesis refers to steady states of a non-equilibrium system, in which the local thermodynamic equilibrium has to be fulfilled: it seems to me that the application of MEP to cases in which the local thermodynamic equilibrium does not hold is against the very definition of MEP. As the authors suggest in the conclusions, the local thermodynamic equilibrium should be regarded as a condition of applicability of MEP just as the energy balance requirement, since they are both needed in order to have a steady state, and MEP (to my knowledge) is just a criterion to identify the most probable steady state among many (and therefore, indirectly, to tune the parameterization of unresolved or "unknown" processes by taking the maximizing values of the parameters). So, from my point of view, it is trivial that no local thermodynamic equilibrium $\dot{}$ no (physical) steady state $\dot{}$ MEP not applicable (meaning that you can still find maxima of course, but they do not represent preferential physical states of the system). I understand that this is also the position of the authors, and that section 6 is probably meant to give a practical example of this inconsistency, but, again, this is not stated clearly enough in the paper, confusing the reader on what the authors are actually doing and why. It seems to me also that this would be the argument in the discussion about the interpretation of MEP as an inference algorithm (MaxEnt): part of the discussion present in the conclusions should be moved also here, in order to make clear to*

the reader in which conceptual context (radically different from what has been done in the rest of the paper) the experiment of section 6 is performed;

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 of the MEP results), in which we now discuss the MEP results for the box-model and, more generally, how to formulate and interpret them. Furthermore, Fig. 12, 13 and 14 have also been removed as these were needed for a detailed analysis of this unrealistic solutions which we now deal with more shortly.

Bibliography

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- [4] 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.