Responses to the comments of Stan Schymanski

January 19, 2019

I have read the paper with great interest and tried to follow and verify the authors' line of arguments. Below, I try to give an untainted and unbiased view of the manuscript, but of course it represents my personal perspective, subject to my own limitations.

Authors response 1: We thank the reviewer for his interest for our manuscript.

1) Summary of key results

The manuscript entitled "A Radiative Convective Model based on constrained Maximum Entropy Production" by Labarre, Paillard and Dubrulle follows on previous articles investigating the usefulness of the Maximum Entropy Production (MEP) principle for climate modelling [e.g. Herbert et al., 2011a,b, 2013, Herbert and Paillard, 2014], with two of the co-authors overlapping between articles. As in previous papers, the authors use the MEP principle as a closure scheme to compute energy fluxes between vertical air layers in a simple climate model (SCM). If I understood correctly, they add consideration of a gravitational field and latent heat flux to the approach by Herbert et al. [2013] and show that the resulting vertical temperature profiles are closer to observations compared with the previous approach for tropical conditions (e.g. Fig. 3c), but not for sub-arctic conditions (e.g. Fig. 4c). The authors also investigate the effect of ozone and different CO2 concentrations on the resulting vertical profiles of temperature and energy fluxes, but I was neither able to verify how these effects were implemented nor assess in how far they resemble observed effects. Table 1 suggests that the climate sensitivities to CO2 in all simulations was quite different to those presented in another paper, based on an Earth System Model. The authors explain some of the discrepancies between simulation results and observations by various simplifying assumptions in their model.

Authors response 2: We will discuss the computation of the radiative budget below and we focus on the sensitivities to CO_2 computed in table 1. The goal of the paper is to verify if a more detailed account of the energy content of the atmosphere and of the energy fluxes helps to improve MEP based model of convection. Sensitivities computed for fixed absolute moisture are far away from those computed with an Earth System Model (ESM). However, the results show that for fixed relative moisture, and for tropical latitudes (the most pertinent for our model for reasons mentioned in the manuscript), the sensitivity of our model is closer to the ESM than the previous MEP based model of [Herbert et al., 2013] (fourth column of the table). In fact, sensitivities of the ESM correspond to a global response. So the reported values in the sixth column are presented only for qualitative comparison.

2 General assessement and recommendation

The linguistic shortcomings, already pointed out by Referee 1, were a bit distracting and so plentiful that I gave up highlighting them, in order to focus more on the content. If this manuscript was going to be accepted for publication, it would need a serious revision for language and typography.

Authors response 3: We want to apologize for linguistic and typographical errors (see comment to the first referee). We will give special attention to this point during the correction of the manuscript.

The promise of the MEP principle to reduce the need for empirical parametrization and/or model calibration is huge and hence I generally welcome attempts to build MEP-based models and evaluate them rigorously. However, the work presented here left me puzzled in many places about its thermodynamic basis. As in previous work, the authors formulate entropy production as an energy flux between two layers divided by the temperature of one of the layers. However, here, the energy flux comprises not only sensible heat, but also water vapour and potential energy. One of the basic textbook expressions of entropy production is that it relates to the rate by which a given thermodynamic potential gradient is depleted, e.g. temperature, pressure, or chemical potential, in some treatments also gravitational potential [e.g. Kondepudi and Prigogine, 1998, Eq. 10.1.9]. This is expressed as a product of a thermodynamic force and a thermodynamic flow, e.g. by a reaction rate multiplied by the affinity of the reaction divided by temperature or a heat flux divided by the temperature difference of two systems. In the present paper, mass flux is multiplied by the sum of thermal, gravitational and latent energy (Eqs. 1, 2 and 5), but I cannot draw the connection of these equations to the relevant thermodynamic formulations. For example, *I do not see how the usual TS term relates to C_pT_i , as $S \neq C_p$, or how Lq_i is related to the classical μN . I am also missing the PV term in the formulation of total energy, and the consideration of volume work. Clearly, the authors should refer to established literature to help the reader reconcile their equations with thermodynamic theory.*

Authors response 4: The entropy production is here the product of affinity (gradient of inverse temperature which is the thermodynamical force that drives the flux) and energy flux $F = m\nabla e = m\nabla(C_pT + gz + Lq)$. More explicitly, the local entropy production is $\sigma = F\nabla(1/T)$. And it can be easily shown that the total entropy production (sum other all layers) can be rewritten as in equation 5. We consider only this term in the entropy production because we use the hydrostatic hypothesis (so there is no work of pressure force PV), and we don't consider the term due to chemical potential μN. This is usually the only term retained in MEP based model [Kleidon, 2010]. The terms gz and Lq are not affinities-fluxes product terms of the entropy production (μN) but nonthermal contributions to the energy flux. We will describe these standard atmospheric assumptions in more details in the revised manuscript.

I am also very confused about the combination of energy balance (Eq. 3) and the radiative budget (Eq. 4), as it suggests that all shortwave radiation reaching a given layer is absorbed and converted to either longwave radiation or one of the other energy terms. I suppose that R_i should be the absorbed shortwave radiation, but then the equation still misses the absorbed longwave radiation, which is the main reason for the greenhouse effect.

Authors response 5: We are sorry for this misunderstanding that may originate from our notations, $\mathcal{R}_i$ represents the net radiative energy input in layer i taking into account several effects: shortwave, longwave, reflexion, and reabsorption. More explicitly:

$$\mathcal{R}_i = SW_i + LW_i = SW_{i\downarrow} - SW_{i\uparrow} + LW_{i\downarrow} - LW_{i\uparrow}$$

where
$SW_{i\downarrow}$ is the downward radiative energy flux for shortwaves;
$SW_{i\uparrow}$ is the upward radiative energy flux for shortwaves;
$LW_{i\downarrow}$ is the downward radiative energy flux for longwaves;
$LW_{i\uparrow}$ is the upward radiative energy flux for longwaves.

This precise definition of $\mathcal{R}_i$ will be given earlier in the text. We will also give equation 4 before equation 2. If we consider that radiative heating is the only forcing of convection, and given the definition of $\mathcal{R}_i$, equation 3 is correct.

The paper does not specify at all how SW_i and LW_i are calculated.

Authors response 6: The radiative code was developed by [Herbert et al., 2013]. The detailed description is long so we referred to this original paper, and its supplementary material, for details. But your remarks push us to give more details about the radiative code in the future version of the manuscript. We will add the following precisions:
As specified by [Herbert et al., 2013]: "The purpose of this model is to reach a balance with a realistic description of the absorption properties of the major radiatively active constituents of the terrestrial atmosphere while keeping a relatively smooth dependence of the radiative flux with respect to the temperature profile. This last requirement is necessary to use the model in the framework of a variational problem.";
Computation of LW_i: In the longwave domain, the code decomposes the spectrum into 22 narrow bands, and in each band, it accounts for absorption by water vapor and carbon dioxide only. The absorption coefficient is computed using the statistical model of [Goody, 1952] with the data from [Rodgers and Walshaw, 1966]. For the spatial integration, the diffusive approximation is performed with the standard diffusion factor $\mu = 1/1.66$. Apart from the absorption data, given once and for all, the inputs of the model are the water vapor density and temperature profiles and carbon dioxide concentration. One may either fix absolute or relative humidity;
Computation of SW_i: In the shortwave domain, absorption by water vapor and ozone is accounted for by adapting the parameterization from [Lacis and Hansen, 1974]. The input parameters for the model are the water vapor density and ozone density profiles, as well as surface albedo and solar constant.
The radiative budget of the atmospheric layer i, $\mathcal{R}_i$, is given by summing over all terms involving the layer in question. In particular, $\mathcal{R}_i$ is a function of all temperatures $\{T_j\}_{j=0,\ldots,N}$ in the profile

I find even more puzzling the assumption that $m_i \geq 0$ (P6L1), termed as the "mechanical constraint". On P4L4, the authors define m i as either "the upward mass flux leaving the layer $i - 1$" or "the downward mass flux coming to the layer $i - 1$". This does not make any sense to me, as according to this wording, both would refer to the flux across the upper boundary of Layer $i - 1$ but would have opposite signs. Whatever the correct *definition of m_i , I do not see how setting it to a positive value represents a mechanical constraint and how this is reconcilable with the statement on P11L14 about a "downward convective energy flux" in the case of light absorption by ozone at the top of the atmosphere.*

Authors response 7: In our model, the definition of the mass fluxes comes from graph theory: for an oriented graph, there can be a link in both directions for edges. m_i^+ and m_i^- are positive. $m_i^+ - m_i^-$ represents the net mass flux, while the minimum represents a mixing of mass. In the stationary state, the net mass transport must be 0 ($m_i^+ = m_i^- \equiv m_i$), but not necessarily the mixing. m_i can be viewed as a diffusion coefficient. We need to impose this coefficient to be positive (i.e $m_i > 0$). Consequently, the energy flux is oriented from hot layer to cold layer. We have used the term mass flux for m_i, but it represents a mixing of mass. We will use the term mixing in the next version of the manuscript which is less confusing. We will also remove the unnecessary definitions of m_i^+ and m_i^-.

Last but not least, the article lacks a data availability statement. See www.earth-system-dynamics.net/about/data policy.html. Without the code and data used to generate the results and given the above-mentioned inclarities I am afraid that the manuscript is of very limited use.

Authors response 8: The code used to compute profiles is a small part of a much larger code. It is likely to be of very limited value without documentation. The implementation of the code is not really important to describe the results. In fact, one can use any algorithm that solves the optimization problem defined in equation 8.

2.1 Recommendation

While my above comments seem rather negative, I still believe that the paper could be a valuable contribution to the scientific literature, if the authors drew clear connections between their formulations and classical thermodynamic formulations, explaining all simplifying assumptions and if they provided all the information needed to reproduce their results. More rigorous comparison of simulations with observations would also be helpful. Since this, along with the necessary clean-up for language and typography, would require considerable effort, I would recommend a major revision or a recommendation for re-submission.

Authors response 9: Our model does not aim at simulating precisely an atmospheric column since our assumptions are far too crude for that. The comparison with observations can only be qualitative.

3 Specific comments

Eq. 4: What about incoming longwave, reflection and re-absorption? Why does SW depend on T? What about LW emissivity?

Authors response 10: These effects are taken into account in the radiative model (see authors responses 5 and 6). However, *SW* does not depend on temperature. We will correct this error in equation 4. We thank the referee for noting this mistake.

P5L8: What does "non local dependence" mean?

Authors response 11: The net radiative budget of the atmospheric layer i, $\mathcal{R}_i$, naturally depends on every temperatures $\{T_j\}_{j=0,\dots,N}$ and greenhouse gases concentrations because of reabsorption and reemission and not of characteristics of layer i alone. We hope that a more detailed description of how the radiative budget is computed will help the reader (see authors response 6).

P5L21: This does not make sense, as mass flux can go in both directions.

Authors response 12: m correspond to a mixing coefficient that should be positive and not to mass flux. We will change the definition in the next version of the manuscript as explained above (see authors response 7).

P6L14: What does "absolute ratio moisture at saturation" mean? What are the units of q_s?

Authors response 13: q is the mixing ratio (the ratio between the mass of water vapor and the total mass of the air for a given volume) which is a dimensionless quantity. And q_s is the value of q for the saturated air. We will replace "absolute ratio moisture" with "mixing ratio", and give the definitions of q and q_s in the text.

P6L15: How is pressure prescribed? How does it vary with height?

Authors response 14: For atmospherical modeling, it is common to work with pressure as vertical coordinates. Then, the elevation z depends on the temperature profile. The computation of geopotential gz is detailed in Appendix B. We will state explicitly that pressure levels are prescribed in the legend of figure 1.

P7L10: What is the thermodynamic force driving the flux? Please explain why flux can be in the opposite direction to the energy gradient.

Authors response 15: As said above, the thermodynamic force driving the flux is the usual gradient of inverse temperature. In previous MEP based model like [Herbert et al., 2013], nothing forbid the flux to be opposed to the energy gradient at some point if it allows to attain a larger entropy production state. In our model, the constraint (positivity of m_i) impose the direction of the flux everywhere (see authors response 7).

Fig. 3: Is this for the tropical latitudes? Please specify what the data in each figure corresponds to and how it was generated.

Authors response 16: This figure is the results of the model for tropical latitudes. All the results come from the numerical resolution of the optimization problem described in AppendixA. We will be more explicit on this point in the figure legend.

P10L26: Why would insolation vary more at high latitudes? It would vary more at the seasonal scale, but less at the diurnal scale. Which variation is more important for the profiles simulated here?

Authors response 17: Since we use measurements [A. McClatchey et al., 1971] that depend on season and latitude, we are talking about the seasonal variations here. We will be more explicit in the next version of the manuscript.

P11L1-5: I do not understand the first sentence at all and the whole paragraph appears rather hand-waving to me. Why does the current version give worse results for high latitudes than Herbert et al. [2013]? More analysis and explanation would be necessary to generate any definitive insights here.

Authors response 18: In this paragraph, we try to explain why our model is not realistic for arctic regions. We don't compare results to [Herbert et al., 2013]. We just want to insist on the point that a single column model without lateral fluxes is not very appropriate for polar conditions. We will try to reformulate better this paragraph.

Table 1: Where the literature results based on the same albedo values? How were these values chosen?

Authors response 19: We have chosen representative (typical) values of albedo. They don't come from a precise article. Again, the comparison with given reference profiles is for illustrative purpose, not for a precise evaluation of the model. We will describe these albedo values in the text as "typical".

P13L1-2: This sentence is not clear. Was it a discretization effect or not?

Authors response 20: We have used the model of [Herbert et al., 2013] to compute the sensitivities of the fourth column of table 1. However, these values are different from those of the original article. We have verified that it was only due to the fact that [Herbert et al., 2013] have used $N = 9$ layers but we choose $N = 20$. We will erase the citation in the table and give more precise explanation in order to avoid any ambiguity.

P15L29: Observation-based temperature profiles were presented, but what are realistic "energy content, and energy fluxes profiles"?

Authors response 21: Realistic energy and energy fluxes profiles referred to the global structure that described a stratification (which are not obtained by previous MEP based model, up to our knowledge). Measurements energy fluxes and energy content are not given in [A. McClatchey et al., 1971]. We will replace "realistic" with "physically relevant".

References: Why are the references not sorted by author name?

Authors response 22: We will use bibtex correctly in the revised manuscript.

References

[A. McClatchey et al., 1971] A. McClatchey, R., W. Fenn, R., E. Volz, F., and S. Garing, J. (1971). Optical properties of the atmosphere (revised). *Environ. Res. Pap.*, 411:100.

[Goody, 1952] Goody, R. M. (1952). A statistical model for water-vapour absorption. *Quarterly Journal of the Royal Meteorological Society*, 78(336):165–169.

[Herbert et al., 2013] Herbert, C., Paillard, D., and Dubrulle, B. (2013). Vertical temperature profiles at maximum entropy production with a net exchange radiative formulation. *Journal of Climate*, 26(21):8545–8555.

[Kleidon, 2010] Kleidon, A. (2010). A basic introduction to the thermodynamics of the earth system far from equilibrium and maximum entropy production. *Philosophical Transactions of the Royal Society B: Biological Sciences*, 365(1545):1303–1315.

[Lacis and Hansen, 1974] Lacis, A. A. and Hansen, J. (1974). A parameterization for the absorption of solar radiation in the earth's atmosphere. *Journal of the Atmospheric Sciences*, 31(1):118–133.

[Rodgers and Walshaw, 1966] Rodgers, C. D. and Walshaw, C. D. (1966). The computation of infra-red cooling rate in planetary atmospheres. *Quarterly Journal of the Royal Meteorological Society*, 92(391):67–92.