Review of Manuscript # esd-2018-69 by Labarre et al.

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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.

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.

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.

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.

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

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.

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.

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.

3 Specific comments

- Eq. 4: What about incoming longwave, reflection and re-absorption? Why does SW depend on T? What about LW emissivity?
- P5L8: What does 'non local dependence' mean?
- P5L21: This does not make sense, as mass flux can go in both directions.
- P6L14: What does 'absolute ratio moisture at saturation' mean? What are the units of q_s?
- P6L15: How is pressure prescribed? How does it vary with height?
- P7L10: What is the thermodynamic force driving the flux? Please explain why flux can be in the opposite direction to the energy gradient.
- Fig. 3: Is this for the tropical latitudes? Please specify what the data in each figure corresponds to and how it was generated.
- 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?
- 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.
- Table 1: Where the literature results based on the same albedo values? How were these values chosen?
- P13L1-2: This sentence is not clear. Was it a discretization effect or not?
- P15L29: Observation-based temperature profiles were presented, but what are realistic 'energy content, and energy fluxes profiles'?
- References: Why are the references not sorted by author name?

References

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