

Interactive comment on “A Radiative Convective Model based on constrained Maximum Entropy Production” by Vincent Labarre et al.

Anonymous Referee #1

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Overview

The authors build on the model developed by Herbert et al (2013) (I note that the second and third authors have contributed to the earlier paper) to use the MEP conjecture to develop profiles for convective fluxes and temperatures. They add a further constraint, in explicitly expressing the energy transferred by convection as an upward and downward mass-flux times a quantity resembling moist static energy or some of its components. The atmospheric profiles that they obtain are compared with observations for various regions and sensitivities to atmospheric composition are obtained.

The paper is an interesting addition to work already published on MEP, and I would recommend publication subject to the comments below.

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Discussion paper



The paper suffers from poor presentation. It would benefit greatly from extensive copy-editing by someone with a good level of scientific English, paying particular attention to words that have a different meaning in French and in English, and to the use of the definite article. (In some places I wonder if Google Translate deserves credit as a co-author!). There are also occasional inconsistent uses of the decimal comma. There are also rather odd spaces before commas in citations, parentheses for citations in a context where only the date of publication should be in parentheses and on occasion no spaces either side of a full stop separating sentences.

It is now unusual to see SI units separated by a full stop, but I presume that the journal has a house style which will state whether this is permitted or not. Likewise, atmospheric pressure is expressed here in terms of mB rather than the more usual hPa.

I only explicitly mention a handful of corrections to the language used below, where I particularly wish to bring a point to the authors' attention.

Minor remarks

Page 2

L. 7 The word “ensemble” in climate science generally refers to a set of perturbed climate models which seek to establish the reliability of a forecast and is confusing here.

L. 16 Not just geometric – some models may use varying albedo.

L.19 It is not just the opacity of the surface, but the relative transparency of the atmosphere that is relevant.

L. 34 I would not regard “the absence of dynamics and/or the validity of MEP” as “one” reason to criticise MEP models.

Pages 8, 9, 12, 14

The captions to Figures 3, 4, 5 and 6 describe the plots in a different order to that

presented, which is mildly irritating.

Page 9

L. 13 – multiple energy profiles are presented; different constraints result in different characteristics. The authors should be more specific as to what they are describing.

L. 20. Clearly some thermal capacity is taken into account somewhere as C_p is not zero!

Page 13

L. 2 If the discretisation effect is important, have the authors satisfied themselves that $N=20$ is sufficient for their purposes?

Page 16

L. 14 I am concerned about the linearisation assumption. Radiation emitted by a layer will have a quartic dependence on temperature, so the linearisation will only be valid for a small perturbation. Is it possible to solve for T as a function of R other than by inverting a linear matrix?

Interactive comment on Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2018-69>, 2018.

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