Interactive comment on “Statistical optimization for passive scalar transport: maximum entropy production vs. maximum Kolmogorov–Sinay entropy” by M. Mihelich et al.

Anonymous Referee #1

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The Maximum Entropy Production (MEP) conjecture is a much debated scientific issue and has attracted lots of interest and criticism over the last thirty years. The main problem with it is that, in spite of some empirical evidence built up mostly in climate science, a rigorous, general demonstration does not exist yet. This piece of work by Mihelich et al. adds a useful contribution to this debate as it shows that, for a simple statistical model of diffusion (a Markov model of the passive scalar diffusion) between two reservoirs, the entropy production is linked to the Kolmogorov-Sinai entropy, a well know quantity in information theory. Results by Mihelich et al. are not general as they hold only for this specific model – and for another similar case (Mihelich et al. (2013))– but may open new avenues of research.

The paper is, overall, interesting and gives a useful contribution to the scientific discussion on the Maximum Entropy Production conjecture. However some more work is required to have the manuscript in its final, publishable form. Therefore no recommendation for publication can be made until the comments and suggestions, listed below, are addressed.

Major points and general remarks

1) Results could be displayed in a more convincing and complete way. The authors should make a more systematic exploration of resolutions and far-fromequilibrium setups. I suggest the author to plot the difference (or percentual difference) between f_{MPE} and f_{MKS} as a function of N and s, that is a 2D contour plot, with s going from 0 to a value typical of far-from-equilibrium conditions and N from O(1) to , e.g., O(1000). This would summarize very effectively the main findings of this study and show clear patterns in the (N; s) space in a wide range of N and s;

This remark is entirely justified. Thus I add a new 2D contour plot representing the difference between f_{max_{ep}} and f_{max_{ks}} in the (N,s) space.

2) page 1695. Here the definition of _S
is not correct and the notation used for the
fluxes between contiguous boxes confusing. Paltridge used the divergence of the meridional heat flux in a certain latitudinal box divided by the box temperature, not the flux itself divided by the temperature, i.e.
R
(r _ F)=T , not
R
R F=T. Then
(r _ F)=T =
R
F _ r(1=T) because F = 0 at the boundaries (poles). Moreover,
the notation f_{ij} is confusing because it looks like there can be a heat exchange

between any i and j, so also noncontiguous boxes, which is not the case.

Indeed, I have corrected the error by writing: $\dot S=\sum_i f_{i(i+1)}(\frac{1}{T_{i+1}}-\frac{1}{T_{i}})$

3) English. There are several typos and minor English mistakes. I'll list a few ones in the following, but this is not an exhaustive list. Therefore the manuscript should be carefully edited to correct minor grammatical errors;

4) References. The scientific literature cited in this study is very, very limited indeed.

In some cases, references cited by the authors are old and more updated studies

could be instead cited. For example, when introducing the "macroscopic" entropy production _ the authors cite Balian (1992) at the beginning of Section 3.1. Now, even having all the respect for Balian, it is odd that they do not mention previous authors such as Onsager (1931), or De Groot and Mazur (1962), or Glansdorff and Prigogine (1971).
At page 1693 they cite Yang et al. (2012) which deals with a convective scheme, but there is nothing about parameter tuning in a General Circulation Model (e.g. Murphy et al. (2004)). At the same page and line 27 the cite Dewar (2003) – which is an outdated study about demonstrating MEP – but they omit more recent studies such as Dewar and Maritan (2014) and references therein.
Also, the authors mention an alternative method for parameter tuning (page 1693, line 17) in the case of complex models based on maximizing (minimizing) a suitable functional (e.g. entropy production), but totally ignore previous studies (Kunz et al. (2008); Pascale et al. (2012)) in which such an idea has been tested for GCMs of various complexity. Concerning efforts made to extend MEP generality, the authors might want also consider the work by Gjermundsen et al. (2014).
In the revised version the authors should therefore pay more attention to this aspect, which is important to put their work in the right wider scientific context.

I naturally added the reference of Onsager (1931) when I introduce the entropy production. I also add the recent work of Dewar and Maritan and the work of Pascale.

5) Diffusion. At page 1703, line 6, the authors say that "If the system is at equilibrium then $f_{maxEP} = f_{maxKS} = 0$ and the system is purely diffusive". This is not true, (molecular) diffusion is also an irreversible process which leads to entropy production
R
(_jrTj2)=T 2dV _ 0. This also points out to me that such an entropy production in not taken into account when the simple ZRP is considered. Perhaps the authors concentrate on f because this, in a real atmosphere, is associated with the nonlinear quasi-turbulent atmospheric flow (midlatitude baroclinic

eddies), but this has to be clarified in the revised manuscript.

In this article I did not say that if the system is diffusive there is no entropy production. I show that for the ZRP, if the system is close to equilibrium, the state chosen by MEP corresponds to a diffusive state. Nevertheless, in order to make this clearer I change " if the system is at equilibrium" by "if the system is close to equilibrium"

Minor points and suggestions

1) Some typos and minor mistakes: (p. 1692, l 9) deviation of/from equilibrium; (p. 1693, l 3) to/too large; (p. 1693, l 17) the/an alternate/alternative road..; (p. 1694, l 3) the citation should be within brackets; (p. 1694, l 11) distance to/from equilibrium; (p. 1695, l 17) do not start a new statement with a mathematical symbol (furthermore in lower case); (p. 1696, l 25) particule ???; (p. 1696, l 25) stationnary/stationary; (p. 1697, l 2) explain/explained; (p. 1698, l 9) The P/physical interpretation; (p. 1698, l 17) reach ie/ reached; (p. 1699, l 11) picked up???; (p. 1700, l 23) for/For N fixed; (p. 1701, l 8) fixe/fixed; (p. 1702, l 1) by compute/computing; (p. 1702, l 1) It is not depends of ?????; (p. 1702, l 8) let's/let us; (p. 1703, l 1) We remark than/that; (p. 1703, l 7) different than/from 0; (p. 1703, l 17) equation of second degrees???/second order equation; (p 1704, l 4) it might fails/fail; (p 1704, l 15) seen as functions/function; (p 1704, l 23) typical of that/those adopted; (p 1705, l 1) research patterns/research avenues.

I thank a lot the referee for all these corrections that I have all changed.

2) Often in the text, new variable or mathematical symbols are suddenly introduced without a previous definition. This is quite annoying and very confusing. For example, immediately in the abstract the symbol f is thrown (line 6). But how can the reader know what f stands for and thus understand that sentence? At page 1696 line 1 the fugacity z is mentioned without being previously defined; at the same page, line 19 the "chemical potential"; at page 1697 an Hamiltonian is mentioned (line 15), and this is completely out of the blue; at page 1698 the flux of mass c unexpectedly appears in an involved relationship (eq. 7). I really suggest the authors to introduce/define these quantities when they first discuss the model.

In fact, the symbol f line 6 was badly introduced. Thus, I add that " f is parameter connected to the jump probability".
Concerning the fugacity, I add that this is a quantity related to the average particle density.
I also precise that the Hamiltonian equation is found from the quantum formalism.

3) page 1695, line 4: shouldn’t it be f = □uT + _rT?

I have corrected the error

4) page 1698, line 16: Why is the thermodynamic force X equal to rlog _ and not

r_?

For the thermodynamics force I took the definition of Balian where X is proportional to the gradient of $\log\rho$.

5) page 1692, line 1-3: The way it’s written, this sentence seems to mean that, through a Markov model, the authors demonstrate the link between MEP and MKS in general, which is not the case. I would therefore say: “We derive rigorous results on the link between the principle of maximum entropy production and the principle of maximum Kolmogorov-Sinai entropy for a Markov model of the passive scalar diffusion called the Zero Range Process”

I change the sentence as suggested by the referee.

6) page 1692, line 13: Climatologist also use GCMs, actually nowadays climatologists hardly use box-models as those in Paltridge (1975), except people studying MEP. Same applies for page 1704, line 23. Please make this sentence more precise.

In order to make this sentence more precise I add “climatologist working on MEP”.

7) page 1703, eq. 21: Actually I can’t see any “=” in the equation;

I corrected the error.

8) page 1703, line 20: what’s a “dominant” coefficient?

I have changed dominant by “leading “coefficient.

9) Eq. (1): given that, spatially, u is a function only of x, wouldn’t it be more precise to write $@_x$ and $@^2_x$ in place of r and r^2?

This remark is well justified and I corrected the equation.

10) page 1695, line 1-3: Said like that, it seems that such an untold equation is something mysterious and esoteric; but this is just the conservation of momentum (NS equation) and, for baroclinic fluids, also energy and mass conservation;

11) page 1695, line 25; page 1698, line 14: right to left, or left to right?

I corrected the mistake page 1698

I really want to thank the referee for all these constructive comments.