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Hydrology and Earth System Sciences Discussions

Interactive comment on "Maximum entropy production: can it be used to constrain conceptual hydrological models?" *by* M. C. Westhoff and E. Zehe

M. C. Westhoff and E. Zehe

martijn.westhoff@kit.edu

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We would like to thank Dr. Schaefli for her constructive comments on our manuscript. However, we do not agree that our style of the paper (i.e. 'a story about the author's learning curve') is not a scientific one. We postulated a clear hypothesis, which we subsequently tested. Most studies only reported on their success, without reporting all trials that did not work. In our case, we had to reject the hypothesis, since we believe that our test is incomplete. We thus decided to report all steps we did to test the hypothesis and that have led to our final conclusion. This reads indeed as a story about

our learning curve, but in our opinion, that does not mean that it is not a scientific paper. The main insight of this work is that those models we like to use for predictions, because they work nicely for rainfall runoff processes, are not the models we can employ to use MEP for constraining the degrees of freedom in the model identification process. We were not aware of this in the beginning of this exercise, because our first findings were very promising. We think that most hydrologists who are wondering about the value of "candidate" organising principles are not aware of this fact either. We consider this a important scientific message.

The main point Dr. Schaefli made, was that we have to make clearer what our findings were and what was already known a priori. We fully agree with her, and in the revised manuscript we will clearly separate them. However, a couple of our findings appeared to be already applied in other studies, but were not reported in such a way that we understood them. Only after analysing our results we realized that others already applied it in a correct way. For example, Porada et al. (2011), did only optimize the entropy of one flux at the time (point 5 in the conclusions). But because they reported the produced entropy of all fluxes in the model, we were confused and therefore did not get this message in once.

Below we reply in more detail about the points which should, according to Dr. Schaefli, into the introduction/methods section (comments by Dr. Schaefli are in italic).

the fact that MEP/power can be defined in many different ways for a given flux/model component, what adds a priori a very 'fuzzy' component to the method.
In principle, power of a flux can only be determined in one way: namely as a flux time a gradient, where a flux is defined as a gradient times a conductance (or divided by a resistance). What makes the component 'fuzzy' is that in the type of model used here, the fluxes are not described as a gradient times a conductance. This makes the determination of the gradients fuzzy, and is also one of the reasons why our method

did not work. This was one of our findings and therefore we'll leave this finding in the discussion. But we'll make this clearer in the revised version.

the problems related to the identification of the system limits
We assume that Dr. Schaefli refers to not all processes that limit a flux are incorporated: e.g. the fact that Zehe et al. (2010), was not able to get an optimum in total numbers of worm burrows. In the revised version, we will mention this more clearly in the introduction, but we think it is important to stress this again in the discussion.

MEP relies on a steady-state assumption and thus applies only to certain time/spatial scales
We fully agree.

MEP can identify only one resistance (if this is a result identified by someone else)
As pointed out above, this was indeed applied by others, but not fully understood by us, before we analysed our results. Since we hope that with this paper, we prevent others of making the same 'mistakes' as we did, we think it is good to leave this point in the discussion/conclusion. However, we will stress that others have done this before in a correct way.

MEP is applicable if the processes have feedback (except if this is your own finding)
It is indeed true that this was known a priori. However, it was unclear to us which feedbacks are needed. For the case of transpiration, we included a feedback between the transpiration and the storage high in the unsaturated zone (S_M). However, in reality there is also a feedback between transpiration and atmospheric water demand, which we did not incorporate.

MEP is not useful for the identification of model parameters that have clearly a monotonically increasing / decreasing relation to MEP (for traditional calibration criteria, this can usually not be seen a priori).
Based on the above, it can be partly anticipated which model components can potentially be constrained and which ones not (e.g. what model component can be

constrained, a priori, by MEP at the given time scale and with the given system limits ?); the numerical experiments would than confirm this.
To be able to do this, one should fully understand the MEP principle. This paper shows that we did not fully understand how to apply the MEP principle. However, due the 'failure', we learned several things about how to apply the principle. Knowing this we should be better able to identify a priori which parameters could be optimized. The point of Dr. Schaefli is valid and we will mention this in the method section. But we will also stress, that this is only possible to know a priori if the MEP principle if better understood.

'Detailed comments'

I suggest to use wherever practical the name of the fluxes rather than their variables; this would help the reader; while E is commonly used for evaporation, e.g. Qd is less intuitive. It is also worth repeating in the results section what the analyzed parameters do in the model
We will do this in the revised manuscript

No parameter names in the conclusion without any details about their role in the model
We will do this in the revised manuscript

the problem of topographic gradient depletion should be discussed from a time/spatial scale point of view : this gradient is depleted but at a very different time scale than the one modeled here
We agree with this.

the abstract should probably also contain an outlook not end with a question
We will do this in the revised manuscript

it could be interesting to reflect the question of process feedbacks for traditional param-

eter calibration (absence of complex feedbacks is often considered to be an advantage to identify parameters) and to further emphasize that optimality modelling might well shed a very new light on the problem of model identification
Feedbacks are often neglected in traditional parameter calibration. However, when applying optimality based principles, these feedbacks have to be described to obtain (mathematically) an optimum. This does not make the modelling exercise simpler, since it can be difficult to describe the feedback correct. From the practitioners' point of view, this may be a disadvantage of optimality based modelling.

the limit of identifying only one flux resistance with MEP should be reflected in the context of what MEP can do for model identification ; what does actually create this certainty ? are there theoretical reasons for this ?
The reason that only one resistance can be optimized is a mathematical one. Optimizing two resistances by considering produced power by one flux does not lead to an optimum. And by optimizing two resistances by maximizing the total power of both fluxes results in maximizing the flux that is driven by the largest gradient.

eq. 11 : something is missing, it is the maximum of a flux and a storage
Thank you for pointing this out. The correct equation is $min(S_1, P_{max}dt)/dt$

p.e 11560, line 15 : the slow reservoir ?
Correct

does the model not have any overland flow ?
Yes it does. Q_d is defined as overland flow (Page 11559, line 3)

p. 11564, line 20: this is a bit too categorical, of course it should lead to a closed water balance but not based on the observations (given the observational uncertainties, the model should probably not close the water balance, see K. Beven's commentaries on this)
We agree with this. Nevertheless, parameter sets that produce more and more power should at least point towards a 'unique' value in the water balance. We will stress this

in the revised manuscript.

conclusion: I would not say that you have not understood the principle, it is a question of how to implement it
Maybe we should not state it so strong. But in the end, we did not implement the principle correctly.

is the reasoning about the thermo dynamical consistency entirely your reasoning ? this should be clearer
We assume that the reviewer refers to section 6.1. In this section we describe what we have learned from the modelling exercise. However, after having understood the principle better, we realized that most of these lessons have been described (or applied) by others. But when one has limited knowledge about the MEP principle, this may be difficult to understand since it is often not stated explicitly. We will make this clearer in the revised manuscript.

overall the language is good but there are several mistakes (namely conjugation of verbs)
We will check this in the revised manuscript.

References:

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