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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

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We would like to thank reviewer 2 for his/her extensive review and his useful comments. Before we reply to each comment in detail, we would like to say that we have the feeling that the reviewer thinks that we question the MEP principle itself. However, we would like to stress that we do not question the MEP principle, but whether MEP can be used to constrain degrees of freedom during calibration of conceptual hydrological models. In this manuscript we show that model structures that are commonly (and successfully) used in hydrology to simulate discharge, are not suitable to apply the MEP principle to reduce the set of acceptable parameter sets that can reproduce observed rainfall runoff behaviour.

Below we reply to the comments (the comments by the reviewer are in italic).

1. The central question presented in this manuscript is, if MEP is suitable to reduce equifinality of hydrological models. Subsequently, MEP is applied to two different types of hydrological models (p. 11554, line 27): Model A predicts catchment properties based on input data and the constraint of a closed water balance. Since these catchment properties cannot be determined unambiguously, MEP is used to reduce the number of possible catchment properties. Model B predicts the components of the water balance based on input data. In this case, catchment properties are derived by MEP to obtain unique values for the components of the water balance. It is important to make clear that these are two different modelling approaches. A reader of the manuscript might get the impression that the authors used a constraint (the water balance) to predict this constraint. This should be avoided, as pointed out in e.g. Schaefli et al. [2011].

It is not correct that we applied two different types of model. The difference between the two 'avenues' we mention in the introduction is the number of free parameters. When we did not constrain any parameter a priori (second avenue) we validated our results with the long term yearly water balance. When one or more parameters were constrained a priori, we made sure that with these parameters the water balance could be matched. So we did not 'use a constraint to predict this constraint', but we used the yearly water balance to validate our results. In the revised manuscript we will make this clearer.

2. Throughout the manuscript it is stated that MEP is "tested" in the context of hydrological modelling. The preconditions for such a test, however, are not sufficiently explained in the manuscript:

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a) It has to be made clear that MEP is treated here as a physical principle and not as an inference algorithm [Dewar, 2009]. If MEP is treated as an inference algorithm, it can be used to test models, but it cannot be tested itself. This issue has already caused a lot of confusion regarding MEP. Since the manuscript aims at reducing this confusion, these two viewpoints of MEP should be mentioned.
We will make this clear in the revised manuscript

b) If model predictions based on MEP and observations do not match, two possible reasons for this outcome exist: First, MEP cannot be used in this case to make correct predictions. Second, the model structure is not suitable to make correct predictions. Thus, in order to be able to falsify MEP, the model structure should be trustworthy. By "trustworthy" model I mean a model that has already been evaluated and proven to make correct predictions. Since most "trustworthy" models still contain tuned/calibrated parameters, it is usually not a problem to apply MEP to these models. The model presented in the manuscript, however, has not been evaluated previously for reasons of equifinality, as far as I understood. I think this is a fundamental problem when it comes to testing MEP. This issue should at least be mentioned in the manuscript.
The model we used in our manuscript is a small extension to the widely (and successfully) used HBV model (Lindström et al., 1997), and several other models exist that are based on the same principle of coupling different reservoirs: e.g. the HYMOD conceptual watershed model (Moore, 1985), the GR4J model (Perrin et al., 2003) or the SUPRFLEX model environment (Fenicia et al., 2011). These models appeared to be very successful in reproducing observed discharge. All of these so-called conceptual bucket models (but also all physically based models) suffer from equifinality due to a lack of observations. Our idea was to use the MEP principle to reduce equifinality of such a conceptual bucket model: MEP serves as an extra objection function. One of our conclusions is that a conceptual bucket model, although it successfully can reproduce observed discharge, cannot be used to apply MEP to; due to a lack of thermodynamic consistency (e.g. fluxes are not always described as gradient times conductance). We will make this clearer in the manuscript

c) The manuscript mentions the problem of determining more than one model parameter by MEP. The authors suggest a stepwise approach, where the number of free parameters is successively increased. They do not, however, provide an explanation for this approach. The only setup that makes sense to me without further explanation is the one described in Sect. 5.3, "Free calibration" where all 5 free model parameters are varied and total power is maximised. Figure 5, however, does not seem to be adequate to analyse the outcome of the "Free calibration" experiment: The authors implicitly assume that a maximum in total power can be found as a function of one of the 5 free model parameters. The power, however, is a function of all 5 free parameters. Thus, if total power is to be maximised, all simulations have to be sorted according to total power. Then, the set of 5 parameters that corresponds to maximum total power is the one predicted by MEP. If this set corresponds to incorrect model predictions, MEP could be falsified. Since there are red dots everywhere in the panels of Fig. 5, it is not clear where the set of parameter values corresponding to maximum total power is located and which value of runoff is associated with it.

We agree with the reviewer that figure 5 is not the best way to visualize the free calibration results. In the revised manuscript we will show in a better way. In principle there is no reason why we chose to stepwise increase the number of free parameters, except for the reason that optimizing β was the only case where we find an optimum with regard to MEP. It is thus an arbitrary choice, just as it is an arbitrary choice to test a model with 5 free parameters. The conclusion made by the reviewer that *If this set corresponds to incorrect model predictions, MEP could be falsified* is not completely correct as we pointed out at the beginning of this reply: Incorrect model prediction could also occur if the MEP principle is applied in a wrong way. And this is the case in our test.

Moreover, no reason is given why it is total power that is maximised. In this context, the authors mention the approach of Porada et al. [2011]. In their Fig. 4, the entropy production of root water uptake and also the entropy production of baseflow are shown as a function of 2 free model parameters. Instead of maximising total entropy *production, the entropy production of each flow is maximised as a function of the 2 free parameters. The set of parameters where both the entropy production of root water uptake and baseflow are at their respective maximum values is used for evaluation. Hence, in contrast to the manuscript presented here, the study of Porada et al. [2011] assumes that only the entropy production of the flux that is controlled by a certain free parameter should be maximised to determine this parameter. Although this assumption is not discussed in detail it is clear that there are at least two ways to apply MEP to hydrological models. Since the manuscript aims at helping other researchers to apply MEP, this should be mentioned.*

This is indeed a good point. First of all we think that if nature strives to be in a state of MEP, this refers to total entropy production and not only individual fluxes. However, if this is true, we should probably simulate ALL feedbacks on ALL spatial and temporal scales, which is virtually impossible. For example, if not all feedbacks are present, the model would configure the parameters in such a way that the flux driven by the largest gradient would be maximum. So, the reason why we maximized total entropy production is because we thought that it should be. Only after understanding the MEP principle better, we realized that this is not the case and we also realized that Porada et al. (2011) did not do this either. The reason why we did not realized this before, was that Porada et al. (2011) reported produced entropy by all fluxes in the model, including the ones that were not optimized. By showing our own learning curve on this, we hope that others will not make the same mistake. Nevertheless, we are still not totally sure that in nature each flux optimizes its own power, although we recognize that in models such as the one of Porada et al. (2011) fluxes can mathematically only be optimized when a flux only optimizes its own power.

3. The authors correctly state that without implementing feedbacks into the model, power/entropy production cannot be maximised. They give some examples of feedbacks that were not implemented into models, but they do not analyse or discuss in which situations these feedbacks are necessary or not. The topographic gradient, for example, (p. 11566, line 26) is indeed constant at the time scale of overland flow. This

simply means that the entropy production of overland flow cannot be maximised at this time scale. To implement the feedback of overland flow on the topographic gradient, the model has to include erosion, sediment transport and uplift and it has to run on long time scales. My point is, the fact that feedbacks are missing from a model does not mean that it is inconsistent in a thermodynamical sense. It is virtually impossible to create a model that contains all possible feedbacks. The authors should distinguish between thermodynamical consistency and inclusion of feedbacks in Sect. 6.1.
We agree with the reviewer that this is indeed the case. We will stress this in the revised manuscript.

4. The six points how to improve the application of MEP to hydrological models are largely incorrect:
a) The system does not have to be in a steady state to apply MEP. The assumption of steady state is just convenient for calculating entropy production because dS of the system does not have to be considered (Eq. 1). This statement occurs throughout the manuscript and should be corrected.
We do not agree with the reviewer. If a system is in a state of MEP, it means that that all free energy is used to drive fluxes. If this is the case, there is no free energy left to change the (structure of the) landscape. This means the system is in steady state. One can also state that if fluxes maximize their power, there is no power left to change the landscape, which also means a steady state.

b) This is correct.

c) Regarding MEP, a feedback between flux and gradient only needs to be implemented if the power/entropy production of the respective fluxes is maximised (see above).
We agree with the reviewer. In the revised manuscript we will point this out.

d) A tradeoff between two or more fluxes is not necessary to apply MEP. Consider, for example, a Benard cell with a fixed temperature at the bottom and a variable temperature at the top. MEP is observed at an intermediate temperature gradient and an

intermediate flux, although only one flux exists. A tradeoff between at least two fluxes is only necessary for MEP in case the total flux through the system is prescribed.
The reviewer is right that two or more fluxes are not always needed. However, in the kind of models we use in hydrology, where within a reservoir or grid cell complete mixing is assumed, two or more fluxes are needed to apply MEP. We will stress this in the revised manuscript.

e) The application of MEP is not constrained to one flux. Although Porada et al. [2011] determined the resistance of a flux by maximising only the entropy production of the associated flux, I see no reason why other setups should be per se invalid (e.g. maximising total entropy production, see above).
As we also pointed out above: maximizing total entropy production may only be applicable if all feedbacks and spatial and temporal scales are considered. Since this is virtually impossible, we only have models of subsystems, in which (at least mathematically) we can only determine the resistance of one flux. As we already mentioned in the manuscript, this can be slightly relaxed, by using the iterative approach Porada et al. (2011) used.

f) The expression "the right degrees of freedom" is not very specific. From Sect. 6.3 I could not determine what the authors mean when they talk about "degrees of freedom". One way to represent the degrees of freedom of a hydrological system is to include free parameters in the model of such a hydrological system. If the authors want to discuss the importance of these parameters, it should be made clear.
With the right degrees of freedom we meant the right amount of feedbacks that are able to constrain a flux. For example, if we want to optimize the conductance for transpiration, without a feedback from the atmosphere, this may lead to a model that maximizes transpiration (and thus minimizing runoff). But with more transpiration, the atmosphere will become closer to saturation, decreasing transpiration and leading to more precipitation: thus more runoff. We will make this clearer in the revised manuscript.

In additional to the general comments, I have some specific remarks. These points should also be clarified:

p. 11552, line 6: The paper by Porada et al. [2011] does a test against observations.
This is indeed the case. However, the comparison they made is rather general, with only one average value over 10 year (see their figure 5). We mentioned this in the introduction, and we will rephrase this sentence in the abstract.

p. 11554, line 18: This sounds like 4 of 6 free parameters in Porada et al. [2011] were calibrated, rendering the application of MEP pointless. The 4 parameters tuned in a previous study, however, were not retuned and thus treated as constraints, same as all other fixed model parameters.
We agree with the reviewer. We will rephrase this in the revised manuscript.

p. 11554, line 22-26: The authors seem to confine "rigorous" model testing to the regional scale. I see no reason why global models cannot be tested, albeit on a coarser scale. If such statements are made in the manuscript, at least some criteria for a "rigorous" model evaluation should be defined.
In the revised manuscript, we will not use the word rigorous in this context, but we will explain that the tests was on a coarse scale.

p. 11555, line 26: Disorder is not a good description for entropy. A crystal, for instance, corresponds to a state of maximum entropy.
That is indeed correct. The reason to phrase it like that is that in our experience many non-experts in entropy described maximum entropy as maximum chaos or maximum disorder. We wanted to refer to 'their' understanding while stating that this is only valid for closed systems. We will extend this with stating that is only the case for fluids or gasses.

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p. 11556, line 7: I think the authors describe a negative feedback here, not a positive one (gradient becomes weaker, so does the associated flow, which keeps the gradient from being further reduced).
Correct.

p. 11556, line 19: It would be nice to have a reference or a short explanation for Eq. 2.
We will add this.

p. 11568, line 26: It is stated that in the model, water limitation is assumed. But the Penman-Monteith equation also accounts for energy limitation, as is mentioned in line 25. This seems contradictory.
In our model setup, the Penman-Monteith equation gives a constraint for the maximum evaporation. We only dealt with water fluxes and the only way to limit evaporation in our model is if there is a shortage of water. That is why we stated that we implicitly assumed a water limited environment.

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