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

Reviewer's comments

The study presented here describes the application of the principle of Maximum Entropy Production (MEP) in the context of hydrological modelling. The goal is to determine if MEP can be used to reduce the equifinality of a hydrological model. The authors suggest, however, that their model setup is flawed and consequently is not suitable to answer this question. They rather propose to use their work as a "guide" for future studies to avoid conceptual mistakes when applying MEP in hydrology. In general I think it is useful to publish such a study. The manuscript, however, contains several inconsistencies and mistakes that make it inappropriate to help others understand more about this potentially important topic. I therefore suggest major revisions of the manuscript before publication. General comments are listed below, followed by some more specific remarks.

General comments:

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].

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:

   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.

   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.

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. 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.

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.

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.

    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).

    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.

    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).

    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.

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.

- 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.

- 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.

- p. 11555, line 26: Disorder is not a good description for entropy. A crystal, for instance, corresponds to a state of maximum entropy.

- 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).

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

- 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.

References

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