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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 3 for his/her positive review and his/her useful comments. We agree with the main point this reviewer make, to test the model in a different catchment as well. In the revised manuscript we will include this.

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

1. General: There are some remaining typo mistakes throughout the text that should

be corrected (not detailed here)
We will check the typos.

2. Page 11552, lines 19-20: This last point is unclear.
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.

3. Page 11554, line 29: What "meaningful parameters" means here? It is well-known that parameters of bucket-type models are generally (very) difficult to link to catchment characteristics.
We agree that that in bucket-type models it is difficult to link parameters directly to catchment characteristics. However, this can be done in a qualitative way. An example of this was given on page 11558, Line 17-21: 'although this is not a soil physical field capacity, this parameter can be interpreted in a soil physical sense. Soils with high clay content store a large amount of water against 20 gravity. Hence, F_C should be close to 0.8. Sandy soils have a low field capacity, thus FC should be around 0.1'.

4. Page 11555, lines 2-3: Since the test catchment was not presented before, this sentence may not be fully clear for the readers not familiar with this basin.
We described the catchment in section 4. In the revised manuscript, we will refer to this section.

5. Page 11555, lines 11-16: This paragraph could be skipped since it announces the conclusions before results are shown.
We agree that these conclusion are announced before the results are shown. But we think it good to mention it here, so readers know better what to expect.

6. Page 11557, Section 3: The authors chose to use a 10-parameter model, which is

clearly overparameterized, to study how the maximum entropy approach can constrain the estimation of these parameters. Results are quite negative, but this may also come from the fact that the problem is too ill-posed and that the approach is of little use in this case. A first step could be to study the behaviour of the approach in the case of a more parsimonious model. This could be done by testing another simpler model.
We applied the MEP principle to different (isolated) parts of the model. For example, for the partitioning of rainwater into evaporation and runoff, we only looked at the unsaturated zone (section 3.1), which has 'only' 5 parameters. We agree with the reviewer that 5 parameters may lead to equifinality, but our aim was to reduce this by applying the MEP principle. The reason for not succeeding in this, is that we did not apply the principle correctly. And one of our conclusions is indeed that with the MEP principle, it is only possible to constrain one parameter (Page 11570, Line 22). In the revised manuscript, we will stress this and refer to the fact that we tried to optimize 5 parameters which, for this reason, did not work.

7. Page 11557, line 18: Write "SUPERFLEX". SUPERFLEX is rather a modelling approach (based on many alternative structures) than a model in the sense of the other model structures cited by the authors.
We will change this in the revised manuscript.

8. Page 11557, line 22: "based on interpretable parameters": same as comment 3
See reply to comment 3.

9. Page 11557, line 23: rephrase "define proper definition"
We will rephrase this into 'proper descriptions'

10. Page 11558, line 9: It is unclear what "effective rainfall" is here. Is it raw rainfall from which something was subtracted? Please clarify this point.
We meant observed rainfall minus interception. This is the part of the rainfall that infiltrates into the soil and is partitioned into transpiration and runoff.

11. Page 11558, line 16: Alpha was not presented before. Where does it act in the

model structure?
Alpha is the albedo: the fraction of solar radiation that is reflected. It is part of the Penman formula. A larger albedo means less absorbed solar radiation and thus less potential evaporation. We will make this clear in the revised manuscript.

12. Page 11560, line 9: I do not understand this equation. Should not it be the minimum of S1 and Pmax?
Thanks for pointing this out. The correct equation is: $min(S_1, P_{max}dt)/dt$

13. Page 11560, line 10: write "storage height" or "store level" (and also Page 11561, line 1)
We will correct this.

14. Page 11561, Section 4: The authors should introduce at least another catchment to test their approach, showing contrasted conditions (e.g. a larger catchment under less humid conditions, where transpiration processes are more energy-limited).
In the revised manuscript, we will also test our approach for the Weiherbach Catchment in Germany: an agricultural catchment with a thick loess layer and a very small portion of overland flow.

15. Page 11561, line 3: Please indicate the country where the catchment is located.
We will do that.

16. Page 11561, line 17: What PRIMET means?
PRIMET is the name of the weather station. Since this data is online, we think it is useful to refer to this name.

17. Page 11561, lines 24-27: This part is a bit unclear for me. First, the expression of the error term that was considered should be more clearly defined. What are the sums referring to? Qerr simply appears as a cumulative error over the test period. Is that the case? Calibrating the unsaturated zone on this criterion is likely not to be fully appropriate. First, since errors can compensate between years, this could lead to *perfect overall water balance but with very poor dynamics. Second, the routing part of the model will introduce some memory effect, which makes that Qd+G of a given year k may not be directly comparable with Qobs of the same year (part of Qd+G will flow in year k+1). Maybe I misunderstood the approach that was chosen by the authors, but they could clarify this point and better justify their choice.*

The sums are indeed referring to the cumulative discharge over a year. We averaged the discharge over 11 years, which will reduce the memory effect of the routing to a minimum (also because this is a small and rather reactive catchment. We agree that the cumulative discharge is not able to account for discharge dynamics, but we used it constrain the parameters of the unsaturated soil, in which the partitioning of rainwater takes place. So we believe that the yearly water balance is a proper metric here (although it clearly has its limitations).

18. Page 11562, line 7: Though widely used, NSE has a number of weaknesses clearly shown by Gupta et al. (2009). The KGE criterion proposed by these authors, which is a more balanced combination of correlation, bias and variance ratio, could be used instead. It also provides direct access to these various statistics, which might be interesting to comment.

We are aware of the weaknesses of the NSE. For the revised manuscript, we will check if the 'KGE' would lead to more insight. But in our manuscript, we mainly showed that parameters, optimized by MEP led to a totally different (and wrong) simulated discharge, where a large NSE still means a 'good' simulation.

19. Page 11562, line 20: The power beta equal to 36 does not seem to be a realistic parameter value (see comments 3 and 8).

In our opinion, it depends on how Q_d is interpreted. We interpreted it a overland flow. A value of beta = 36 then means that there is hardly any overland flow. This corresponds with what we know of this catchment. But we agree that beta is rather insensitive in this range, with only a small change between beta = 36 and beta = 70.

20. Page 11563, lines 5-7: It is unclear how the parameter sets were chosen.

The parameter sets were arbitrarily chosen (with the constraint that they were able to close the water balance for a certain value of beta). One set was chosen in a way that Q_d was the most dominant term, while the other set was chosen in a way that G was the most dominant flux. In the revised manuscript, we will make this clearer.

21. Page 11563, line 10: How can the authors argue that these "values are representative of this watershed"? Was there some preliminary testing of the model showing that parameter values could be actually linked to catchment descriptors?

We believe that the K_s parameter is a parameter that can be rather directly linked to observations made in the catchment (which has been done by Tague and Band, 2001). The F_C parameter canot be confirmed with direct observations, but as we pointed out in our reply to comment 3, we can compare this parameter in a qualitative way.

22. Page 11563, lines 10-13: I agree. Testing on another catchment may more clearly indicate whether this is a coincidence.

We will test this.

23. Page 11563, line 23-24: The way alpha influences Epot could be further explained (see comment 11)

See reply to comment 11.

24. Page 11564, line 17: The title of the section is unclear. What "free calibration" means?

With free calibration we meant that we did not constraint any parameter on beforehand. We subsequently tested if the parameter set that maximizes MEP also closed the water balance. We will change the title in 'Calibration on all 5 parameters'.

25. Pages 11566, line 1: I think the discussion could be improved in light of the additional results the authors would get if they introduce another catchment and possibly another model, as suggested above. Some hypotheses they give may thus be better discussed, ending with more general conclusions.

We agree with this.

26. Page 11570, lines 9-10: I very much agree with the point of view expressed by the authors. Probably not enough is published in the literature on mistakes/failure (see e.g. the special issue of Hydrological Sciences Journal, 55(6), on modelling failures)
Thank you.

27. Page 11571, lines 8-9: Is expected "the big step forward" not contradictory with the current lack of any promising result of MEP application? Can we realistically expect that hydrological models can be modified in such a way that MEP becomes applicable, without losing model efficiency?
Until this moment there are indeed limited studies that applied the MEP principle correctly. Nevertheless, we believe that this is no reason (yet) to abandon this topic, since we still believe that optimality principles may, in future, lead to a paradigm shift in hydrology.

28. Figure 1: All parameters could appear on the model scheme, to help the reader better follow the description in the text. State variable and parameters could be written differently (e.g. parameters in bold).
This is a good point. However, we will check if the figure will not become to messy.

29. Figure 3: The 10ËĘ2 value for beta does not seem realistic. It is just a mathematical optimum showing that this function is not active. (see comment 19)
As mentioned in our reply to comment 19, we agree that at that range beta is rather insensitive. In the revised manuscript we will discuss this in more detail.

References:

Tague, C. L. and Band, L. E.: Evaluating explicit and implicit routing for watershed hydroecological models of forest hydrology at the small catchment scale, Hydrol. Process., 15, 1415–1439, doi:10.1002/hyp.171, 2001.

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