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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

Anonymous Referee #3

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

General comments:

The article analyses the applicability of a method based on the concept of entropy maximization in the context of rainfall-runoff modelling. The authors show that, in spite of its theoretically appeal, the usefulness of this approach for hydrological modelling is very difficult to demonstrate. In that sense, the article sounds like a failure story, but it could save time to other researchers wishing to use such an approach, which

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makes this paper a valuable contribution for publication. The article identifies some key aspects that should be further investigated to make the approach work in the context of hydrological modelling.

I liked reading this article. My main regret is that the work is based on a single case study (one model applied on one catchment), which makes the conclusions not general enough. I think the authors could improve this by at least adding another catchment with contrasted conditions, and possibly adding a second (more simple, i.e. not over-parameterized) model. This would give a more general reach to their conclusions and help discussing aspects that seem to be related to the characteristics of the catchment and model used. Since the article is quite short, I think this would not make it too long.

Below are some detailed comments. Since I believe new calculations are needed to extend this study, major revision is requested. I hope the authors will be able to account for these comments.

Detailed comments:

1. General: There are some remaining typo mistakes throughout the text that should be corrected (not detailed here)

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

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.

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.

5. Page 11555, lines 11-16: This paragraph could be skipped since it announces the conclusions before results are shown.

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

HESSD

9, C5640–C5644, 2012

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.

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.

8. Page 11557, line 22: “based on interpretable parameters”: same as comment #3

9. Page 11557, line 23: rephrase “define proper definition”

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.

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

12. Page 11560, line 9: I do not understand this equation. Should not it be the minimum of S1 and Pmax?

13. Page 11560, line 10: write “storage height” or “store level” (and also Page 11561, line 1)

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).

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

16. Page 11561, line 17: What PRIMET means?

17. Page 11561, lines 24-27: This part is a bit unclear for me. First, the expression

HESSD

9, C5640–C5644, 2012

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.

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.

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).

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

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?

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

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

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

HESSD

9, C5640–C5644, 2012

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.

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)

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?

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).

29. Figure 3: The 10ˆ2 value for beta does not seem realistic. It is just a mathematical optimum showing that this function is not active. (see comment #19)

Cited reference:

Gupta, H. V., H. Kling, K. K. Yilmaz and G. F. Martinez (2009). "Decomposition of the mean squared error and NSE performance criteria: Implications for improving hydrological modelling." Journal of Hydrology 377(1-2): 80-91.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 11551, 2012.