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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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This manuscript tries to use an optimality principle (maximum entropy production, MEP) to identify the parameters of a conceptual rainfall-runoff model. MEP has been successfully used in other ecohydrological modeling contexts, but as the authors explain in this manuscript, cannot be readily used for parameter identification in classical rainfall-runoff (RR) modeling. Accordingly, rather than presenting how to make successfully use of MEP for RR-models, the paper reports the failure of their experiment . It is of course very unusual to present an experiment without any positive outcome but

given the increasing interest for optimality modeling and MEP, I think that the paper is nevertheless interesting for the readers of HESS. However, the current version of the paper is written as a story about the authors learning process rather than as a scientific paper. While I think the paper reads well, I nevertheless recommend to considerably modify it to make it namely more concise. My most important critic is the fact that some technical details of the method are presented in the discussion section rather than in the literature review or in the methods section. Accordingly, it is not entirely clear which points of the discussion section correspond to previously known results and which findings are new. This should be made much clearer. The following points should probably be discussed already in the introduction / method sections:

- the fact that MEP / power can be defined in many different ways for a given flux / model component, what adds a priori a very Âń fuzzy Âż component to the method
- the problems related to the identification of the system limits
- MEP relies on a steady-state assumption and thus applies only to certain time / spatial scales
- MEP can identify only one resistance (if this is a result identified by someone else)
- MEP is applicable if the processes have feedback (except if this is your own finding)
- 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).

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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.

Detailed comments (in random order):

- 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
- No parameter names in the conclusion without any details about their role in the model
- 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
- the abstract should probably also contain an outlook not end with a question
- it could be interesting to reflect the question of process feedbacks for traditional parameter calibration (absence of complex feedbacks is often considered to be an advantage to identify parameters) and to further emphasize that optimality modeling might well shed a very new light on the problem of model identification
- 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 ?
- eq. 11 : something is missing, it is the maximum of a flux and a storage

- p.e 11560, line 15 : the slow reservoir ?
- does the model not have any overland flow ?
- 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)
- conclusion: I would not say that you have not understood the principle, it is a question of how to implement it
- is the reasoning about the thermo dynamical consistency entirely your reasoning ? this should be clearer
- overall the language is good but there are several mistakes (namely conjugation of verbs)

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