Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-508-RC1, 2016

Hydrology and Earth System Sciences
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Discussions

Interactive comment on "A Hydrological Prediction System Based on the SVS Land–Surface Scheme: Implementation and Evaluation of the GEM-Hydro platform on the watershed of Lake Ontario" *by* Étienne Gaborit et al.

Anonymous Referee #1

Received and published: 2 December 2016

General comments

The manuscript presents interesting topic, but in its current form it is very difficult to read and reads more like a technical report rather than a scientific paper. I have several comments, which might be considered for a revision:

1) The formulation of research hypothesis and novel scientific contribution is not clear. The first objective (as formulated in the manuscript) is to propose a methodology for

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calibrating distributed hydrologic model, but the results refer to an apriori selected number of model runs (in model calibration), without testing whether this approach is better (and in which aspects) than some methodologies used for model calibration. The introduction refers to numerous previous studies but does not clearly indicate what unanswered question is investigated here. Just implementation reads more as a technical than a scientific question. In its current form it is formulated as a case study. Why it should be interesting for international audience of the journal? What can be learned/generalised from the results which will be interesting/relevant also for other regions in the world?

2) The number of model runs does not seem to be very large (i.e. adequate), so some more deep analysis and justification of such setup is needed. E.g. I wonder whether 400 runs/combinations for 16 model parameters are enough representative.

3) The 2. objective of the manuscript is to compare different models, but it is not clear why not ISBA and SVS are compared, as in the Introduction it is referred to the replacement of ISBA by SVS. This would allow to better demonstrate the potential of the new model/implementation.

4) The manuscript refers to many different other studies within the study region, but not all are relevant to the main objectives, so it distracts the reader from the main story line. Moreover, it is than not quite clear, in which respects is this study novel, so a more clear discussion of the novelty would be helpful. There are numerous references to studies in press or preparation, which does not allow to justify to what extent the study overlaps with previous/recent studies. I would suggest to consider streamlining the text flow, and do not refer much to studies which are not directly linked/relevant with the research questions studied here. For example references to lumped modelling results in the Study area section are not placed/relevant (well) here. Please consider also not to use so many abbreviations, because some parts are then very difficult to understand (e.g. p3., l14).

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Summing up, the topic is interesting and within the scope of the journal. The manuscript however needs some revision and transformation to a more scientific than technical report.

Specific comments

1) Abstract: Please consider to be more specific (i.e. provide numbers, efficiencies, etc), particularly when referring to results found. The context part for the research does not have to be such long.

2) Introduction (p.4., l.5-10): It will be important to clearly formulate in which respect is this study new in comparison to the first GRIP-O study. Please consider also discuss/show how specific was the model performance and how similar/different it is with respect to this study.

3) P.5, l2.: Please consider be more specific about the calibration strategy of Haghnegahdar et al. (2014).

4) p.5, l.14-18: This part is messy and not clear. Please consider to revise.

5) P.5, l19-21: Why are the used time-steps different? Has it some implications for interpreting results?

6) P.6, l.27: Why is lumped model mentioned here? Are the findings (good performance) for the right reasons? The reference of Gaborit is not accessible so it is difficult to see.

7) P.7, l.6, l.9: Which hydraulic parameters? Is the maximum soil depth calibrated for each grid cell or entire domain?

8) P.7, l.19: what is RDPS?

9) P8, l.3: how many runs has typically local calibration?

10) P.8, l.14: Please be more specific how were the values contrained?

11) P.9, l.1-6: This part is not clear. On how many points is then the model verified/compared /calibrated?

12) Strategy for ungauged basins: Typically, the prediction in ungauged basins is verified by leave-one-out approach. How do the results compare with such method? Please consider to discuss.

13) P.11, l.28-29: Please consider to show some results supporting this statement.

14) P.16, l.17: Please consider to update XXXX.

15) P.21, l.5: “the most-downstream flow gauges” is not clear.

16) Table 1: Please be more specific what is radiative forcings

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