

## ***Interactive comment on “Comparative predictions of discharge from an artificial catchment (Chicken Creek) using sparse data” by H. M. Holländer et al.***

**Anonymous Referee #1**

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The paper by Hollander et al. entitled "Comparative predictions of discharge from an artificial catchment (Chicken Creek) using sparse data" is an interesting and enlightening paper outlining how catchment modellers approach a common problem, and in general, how poor existing models are at predicting discharge and other hydrological parameters based on limited data. While the paper does not present scientifically novel concepts, it has utility in that it exposes the dangers and dilemma of modelling ungauged basins with limited data. It is not worthwhile here to judge the models themselves, or the modeller, but simply gain insight from the comparative exercise. In addition, this is a substantial effort by a large number of authors and research groups that is worth of attention. I look forward to future papers where additional information is introduced and further prediction improvements are realized. Clearly the "tipping point"

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between information and robust predictions has not been reached.

There are a number in which this manuscript can be improved. The English, while acceptable, is often clumsy and I have provided a list for consideration below. There are also several technical and/or editorial points for consideration.

1. There is no discussion section and the conclusions are quite short. While I appreciate the authors comment in each section on the issues at hand, a general discussion regarding the models and implication of this study are important. It is left to the reader to draw conclusions themselves. For example - out ability to model anything in the hydrological cycle below freezing seems absent! Issues like this are brushed under the rug or simply stated.
2. I am wondering how "fair" this model inter-comparison is considering that the catchment had not achieved a "steady state" (if there is such a thing) at the onset of experimentation. While I agree that natural variability precludes the idea of steady state, having an artificial catchment wet-up and achieve some sort of equilibrium before the models were applied, I believe, would have been a more appropriate method of assessing model efficacy. While there is no explicit discussion section in the paper (see additional comments), this is an important point, and the modellers are implicitly misled by the nature of this system and its transitional state.
3. I would argue that 6ha is not the largest experimental catchment worldwide. There are other reclaimed mine sites with areas greater than 6ha set up as research watersheds. I would suggest the authors expand their literature search or simply state the site description.
4. Is there any way that the conceptualization of catchment features be brought into a table. The processes are sorted nicely into tables, but the actual conceptualization in tabular format would help as section 3.4 is a bit clumsy.
5. How were evaporative losses from the lake considered? In general, the "validation"

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data collected from the catchment are poor and sparse considering the small size. I'm somewhat surprised by this as there are natural experimental catchments with excellent data sets that could have provided a better test of the models and the modellers. However, I understand the element of mystery is essential in this work.

6. I am unsure as to the utility of examining potential evapotranspiration (PET), as it is not a true hydrological flux, and one could argue a false hydrological concept. Actual ET on the other hand is a critical hydrological flux to measure accurately. That said, this research is hampered by the fact that actual ET was not monitored via eddy covariance, lysimeters, or other techniques, but estimated via an antiquated technique. This is a serious shortcoming as it almost appears that the experiment was set up by modellers (with a focus on the input-output relations and a lack of soil moisture, AET, snow storage, etc., data). More discussion on this issue is warranted.

7. p. 3229. Some would argue that the slope of the recession curve has much physical meaning (see Kirchner 2009, WRR). I am unsure why the authors are so quick to dismiss its lack of physical interpretation or meaning.

Editorial Notes.

- p. 3210, ln 8-10. Please reword as this is unclear.
- p. 3210, ln 11. what is the saturated zone model - subject needed.
- p. 3214, ln 1. "does not allot to explicitly....." - poor English, revise.
- p. 3215, ln 15-18. Poor English. Please revise the sentences.
- p. 3216, ln 10. Reverse the order of "the all" at the beginning.
- p. 3218, ln 10-11. "CoupModel was not...." - poor English, revise.
- p. 3218, ln 14. Furthermore should be one word, not two.
- p. 3222, ln 2. Trice is not commonly used in standard English.

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- p. 3222, ln 9. Beginning sentence needs a subject.
- p. 3222, ln 20-21. This is an important point but poorly written. Please revise. "is mostly not considered" is poor structure.
- p. 3223, ln 4. A period is missing.
- p. 3223, ln 23 "somewhat increased". This is poor English. It either did or it didn't. There is an excessive use of the passive voice throughout.
- p. 3224, ln 11. "got both" should be reworded.
- p. 3227, ln 15-16. Repeated sentence.
- p. 3227, ln 25. "both not using" - reword.
- p. 3232, ln 5. "at present...." reword.
- p. 3234, ln 17 "was leading to too...." reword.
- p. 3236, ln 16. "soilw ater" correct

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