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Reply on RC1

Robert Hull et al.

Author comment on "Using simulation-based inference to determine the parameters of an integrated hydrologic model: a case study from the upper Colorado River basin" by Robert Hull et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-345-AC1, 2022

We respect and appreciate the referee's remarks. They are thoughtfully considered, evidence genuine
interest in the manuscript, and will make this work better. We see three main critiques by Dr. Beven.

The first critique is that we don't compare the results of the method we are presenting – which we term
surrogate-informed Simulation-Based Inference (SBI) – to existing alternatives. We would like to point
out that we do discuss some of the new and long-standing approaches to efficient inference, model
calibration, and uncertainty estimation in our field [lines 43 -52], though of course not all of them; in
particular, we regret omitting the 1992 GLUE paper. The shortness of our discussion is not intended as a
dig against those alternatives; it's just that wading into a detailed exposition of what constitutes robust,
efficient parameter determination (moreover the significance of the likelihood function) has been done
elsewhere [Beven, 2015; Nearing et. al., 2015].

Our work is unique in its use of specialized ML components – in particular, a conditional density
estimation approach to inference [Bishop, 1994; Papamakarios and Muray, 2019]– and we think that
this novelty warrants its own publication. Still, we understand Dr. Beven's point. We initially considered
presenting the results of our shiny method alongside traditional (for hydrology) approaches used for
parameter determination in the face of uncertainty (i.e. GLUE and Approximate Bayesian Computation
(ABC)). In the end, we felt we couldn't do justice to such an analysis and fit it in this manuscript. We
stand by this, even though it needs to be done somewhere.

The second critique is that the framing of our work is misleading. Specifically, the reviewer notes that it
"tells us nothing at all about simulating the Taylor River Basin in the Upper Colorado Basin". We hope
the referee trusts us that it was not our intent to mislead. The title, abstract, and introduction speak to
the larger context in which this study was conducted. This context is reflected by a collection of studies
(some written by members of this group of authors) that apply ML methodologies alongside process
understanding for hydrologic prediction. Much of that work has centered on the headwaters of the
Colorado River, an important fixture of water in the American West. We felt this context was relevant
for human and hydrological reasons; in our view, the study of hydrology should never be separated from
place, no matter how 'theoretical'. We discuss some potential changes to this framing in the concluding
paragraph.

The third critique, and in Dr. Bevens' view the fatal one, is that this study "uses only simulated data". We
agree with Charles Pierce (a quoted authority in some of the referee's work) that the goal of scientific
inquiry is truth, and that truth dwells in the realm 'real' observable phenomena. But to say "there is
absolutely no point in publishing a paper that compares only model generated data .... without any
resort to real observations" seems to us a bit dogmatic. There is no shortage of studies that utilize
mostly or only synthetic data to demonstrate proof-of-concept in hydrology and other simulator-
intensive fields.

So we disagree that this 'flaw' is fatal, or even a flaw at all. Our purpose here is to rigorously present and
evaluate a method for parameter inference given well-defined constraints. The challenge of this goal is
real and relevant; in fact, this work seems to show an upper bound for the performance of SBI where
undiagnosed structural error exists [lines 578-615]. Comparing to observations would instead shift our
focus from the quality of a method to the quality of the underlying hydrologic model. Because we leave
observations for later, we have a more generalizable, model agnostic (i.e. not just about ParFlow) paper.

We applaud Dr. Beven for suggesting a follow-on paper with observations. Such an effort requires an
expanded model and additional concerns about structural and observational errors, as has been noted.
In the spirt of walking before you can run, we are happy to have laid the groundwork for an effort
focusing on observations – though not necessarily to be done here or in a companion paper.
We propose the following changes to address Dr. Beven's concerns while remaining true to

our intended
purpose with this manuscript:

- Reframe the abstract and introduction to make it clearer that the system under study is
  synthetic, and that comparisons are not directly extendable to 'real' hydrologic systems; and a
  related title change.
- Give more detailed overview of the methods used for parameter determination in the face of
  uncertainty in hydrology, such as GLUE and ABC, in the background section; though again, there
  is not space to conduct a comparison in our study.
- More clearly state the sources of uncertainty in each experiment, and how they relate to the
  limitations of physically- based models, the complex nature of 'real' response surfaces, the
  influence of disinformation in observations, and the challenge of defining limits of acceptability.

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