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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A Multi-Objective Ensemble Approach to Hydrological Modelling in the UK: An Application to Historic Drought Reconstruction" *by* K. A. Smith et al.

Anonymous Referee #2

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This paper uses a multi-objective approach to calibrate a fairly simple hydrologic model to predict discharge at a large number of catchments in the UK based on precipitation and temperature observations. The stated purpose of the exercise was to hindcast streamflow during historical early 20th century droughts that occured prior to the systematic collection of discharge observations on UK streams, but (crucially) not before available meteorological records. The results show that the relatively simple hydrologic model that was used (4 parameters) was able to capture streamflow variability well, over the wide range of catchments included in the survey. The study showed little evidence of non-stationarity in parameter calibration, which allowed historical droughts to be hincasted with a decent level of confidence.

Major remarks The study is methodologically solid. The paper is well written and methods and results are described clearly and in sufficient details. However, I am not sure I understand the contribution of the paper beyond a solid regional study of UK streams. This is without a doubt a useful practical contribution for the UK water resources community, but you should do a better job at discussing general implications of the research in the introduction and discussion. To be excessively blunt, as a scientist that has no particular interest in UK streams (like a large chunk of HESS readership), why should I care? To be a bit more specific, you explicitly lists the intended contributions of the paper in the conclusions (L527). At face value, these contributions are sufficiently general to interest non-UK readers and should be stated upfront (the intro is very much UK specific currently). However, I think that these arguments currently lack substance and should be further developed:

1. You mention your multi-objective calibration approach as the first general contribution of the paper. As you admit yourself (L91), the concept itself of multi-objective calibration is not new and the section where you describe model selection (3.4) is particularly cryptic. If multi-objective calibration is indeed a key contribution of the paper, please describe the approach specifically (How are the model parametrizations "ranked"? How are each of the criteria weighted to come up with a composite ranking?) and spell out clearly what the novelty is compared to existing approaches.

2. Second, you claim that the approach can be used not only to hindcast droughts but also to predict catchment responses to future climate change. In order to make such a claim, you ought to address th elephant in the room, which is that your approach does not accomodate non-stationarities in the calibrated parameters (e.g., related to land use change and human adaptation). Your result suggest that these factor were not much of a problem for historical simulations (except for heavily altered catchment), but if there is one thing that climate studies tell us is that the past is not necessarily representative of the future. I do agree that your results are interesting and can be leveraged to study the hydrological impacts of climate change, but the implied caveats

âĂŤ and potential avenues to go around them âĂŤ should be discussed. I am specifically thinking of the potential to leverage satellite observations of land use change and/or modules integrating human adaptation to large scale hydrological models (e.g, Bierkens 2015, Calvin 2018).

3.Third, you argue that the study provides important spatio-temporal data on historical drought in the UK (so far so good) which can be used to plan and forecast the onset, duration and termination of drought events in the UK and overseas. First off, it is not clear to me how, specifically, how the historical reanalysis you describe can be used to forecast and mitigate the effect of future droughts (see previous point) – if you have a specific idea here, please make it explicit. Most importantly, your method relies on the fact that a large volume of high quality meteorological observations (for both P and PET) were available in the early 20th century, before river discharges were systematically gauged. This was definitely the case for the UK, but in order to argue that the approach you propose is applicable beyond the UK (which would make it more relevant to the global hydrologic community), you have to show that what happened in the UK is not an exception. It can very well be that met data was collected way before flow data in other countries as well, but you have to make this argument explicit (and ideally back it up with some data).

Minor comments

L210 I am not sure I understand your multi-objective approach to select catchments. How do you weigh different criteria when ranking the parametrization (e.g., how do you differentiate a parametrization A with a NSE of 0.64 and a Q95APE of 34 from a parametrization B with a NSE of 0.70 and a Q95 APE of 40 – which one dominates?). What optimality concept is your approach consistent with (pareto, maxi-min (i..e maximizing the worst performing metrics), . . .)

There are lots of accronyms to remember. A Table summarizing the abbreviations would be useful

Fig 5: labeled pointers showing the catchemnt that you specifically discuss in the text would be useful.

L132, 502: Please refrain from citing work in preparation.

Refs Calvin, Katherine, and Ben Bond-Lamberty. "Integrated human-earth system modelingâĂŤstate of the science and future directions." Environmental Research Letters 13.6 (2018): 063006. Bierkens, Marc FP. "Global hydrology 2015: State, trends, and directions." Water Resources Research 51.7 (2015): 4923-4947.

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