

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1
<https://doi.org/10.5194/hess-2022-264-RC1>, 2022
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Comment on hess-2022-264

Anonymous Referee #1

Referee comment on "Producing reliable hydrologic scenarios from raw climate model outputs without resorting to meteorological observations" by Simon Ricard et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2022-264-RC1>, 2022

The paper entitled "Producing reliable hydrologic scenarios from raw climate model outputs without resorting to meteorological observations" by Simon Richard and co-authors proposes a new modeling workflow to derive hydrologic scenarios from climate model projections without resort to the usual step of bias correction of climate projections. The topic is well suited for the journal *Hydrology and Earth System Sciences* and I think the paper raises important and interesting questions at the interface of climate sciences and hydrology.

I found the paper well written and organized, the methods are described in details, and a case study allows readers to have an idea of the performance of the proposed framework for a typical application (in the present case: producing hydrologic scenarios under climate change for a catchment located in Québec - Canada). I enjoyed the comparison with a more conventional approach (i.e. including a step of bias correction of model outputs), and the way the results are displayed and discussed in Section 4: Results.

My only sticking point is the discussion (Sect. 5). In contrast with Section 4, I have the impression that the authors are overselling their approach in the discussion section. Indeed, the general tone of this section tends to make the reader forget that the conventional approach still outperforms the one proposed in the present paper (cf Fig. 6 and 7). I acknowledge the interest of the alternative approach proposed by the authors for both (1) complement the standard approach in areas where meteorological observations are available, and (2) allow hydrologists to perform hydrologic projections where meteorological observations are too sparse to enable reliable climate model bias correction. But I also think that the discussion is too harsh towards climate model bias correction (i.e. the conventional approach), and that the proposed approach does not solve many of the (legitimate) questions raised by the authors in the discussion. More particularly:

* Integrating meteorological observations in the modeling chain. When meteorological

observations are available, I think it is a shame to disregard them. Maybe the conventional approach of bias correction of climate outputs is not ideal (and the authors are right to talk about its limitations), but removing it without replacement is equivalent to do without all the information embedded in meteorological observations. Of course having a method to deal with poorly gauged areas (in terms of meteorological variables) is a plus, but when data are available I don't see why not using them. So I think that the authors should acknowledge more clearly in the discussion that when meteorological data are sufficient for climate model bias correction, this method is still the one that performs best.

* Physical consistency of the modeling chain. I agree that bias correction methods disrupt the physical consistency of climate projections. But clearly the approach proposed in this paper does not provide any solution to this problem. The calibration of a hydrological model directly from raw climate outputs will mix climate biases and hydrologic biases (as acknowledged by the authors in Sect 5.3), which results in a completely non-physical hydrological model. So I think that the authors should acknowledge that both approaches are equally breaking the physical consistency of the hydro-meteorological processes involved in the models used to investigate the hydrological response of our environment to climate change, and that where we decide to do it (and unfortunately often to hide it) within the modeling chain is somehow a matter of taste.

* Interpretability of climate projections by end-users. The questions raised at the end of Sect 5.2 (l 465 - 472) are interesting and legitimate, but with a very few exceptions can also be addressed with bias-corrected climate model outputs. In a slightly different note, I do not think that bringing expertise in analyzing, selecting and pre-processing climate model outputs is in itself a bad thing. An intense and constructive discussion between climate modelers, statisticians performing bias corrections, hydrologists and stakeholders (I probably forget important participants) is of course essential, but I am a bit skeptical about the idea of a more direct (and therefore possibly less careful) use of raw climate model outputs. From my personal experience, the support from experts in climate models biases and bias correction is essential to avoid misuse of climate projections.

To sum up, I think that the present manuscript is interesting and well written, and I my opinion deserves publication after revisions. My main concern is about the discussion, which I believe can be improved by moderate revisions, mostly by rephrasing this section in a more objective way.

Hereafter are some minor comments/questions I had during my reading of the manuscript:

- L119: how many RCM grid cells cover your study area?

- Fig 1: Maybe add information about topography.

- Fig 2: It would be nice to compare with the conventional approach in the figure (i.e. have 2 workflows in the figure) not only in the caption.

- Throughout Sect. 4: also mention relative biases. For instance L 243: "Biases typically range between -1 and +2 mm/day (xx %) depending on ...".

- L 264 and after: Validation of the asynchronous -> I would prefer "assessment" instead of "validation" (maybe personal taste).

- L 298: "which can be considered as comparably performant relative to the conventional approach" -> one of the few places in sect 4 where you are not very fair with your results. Consider rephrasing.

- L 369: typo in percentile definition: 0.5 -> 0.05

- L 386: maybe remind for which period and RCP scenarios the hydrologic scenarios are made.

- L422: we validated -> we assessed the performance

- L 519-521: the way this paragraph is written gives the impression that you consider low and high flows as extreme events. Maybe consider rephrasing?