Hydrology and Earth System Sciences
Discussions

Hydrol. Earth Syst. Sci. Discuss., author comment AC1
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Reply on RC2

Sebastian A. Krogh et al.

Author comment on "Diel streamflow cycles suggest more sensitive snowmelt-driven streamflow to climate change than land surface modeling does" by Sebastian A. Krogh et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-437-AC1, 2021

Answers provided in bold

The authors present a new means of considering the sensitivity of snowmelt timing and streamflow response under warming climate conditions based on space for time substitutions. Their metric (DOS_20) is based on diel fluctuations in streamflow that correlate with solar radiation (after a time lag of 6-18 hours). They use this metric to assess regional sensitivity to warming across an array of small montane basins in the western U.S. They compare their approach to one using a physically-based modeling framework, highlighting differences in snowmelt-streamflow sensitivities derived from each method.

I think the approach presented here can provide valuable insights into the implications climate warming holds for water forecasting and management. However, I found the paper somewhat difficult to follow. I believe significant revisions are necessary to improve the clarity of the analysis. These are enumerated below.

We greatly appreciate the positive comments.

- Devote more space to background information. Numerous concepts are discussed with minimal introduction (e.g. space for time substitution, mean annual autocorrelation, diel streamflow cycles, etc). I understand that the authors are snow hydrologists writing for other snow hydrologists, but the paper would be significantly easier to follow with a proper setup for many of the concepts being discussed.

We appreciate the reviewer´s feedback and we will provide a more detailed introduction to the terms highlighted by the reviewer.

- Streamline extremely dense figures and captions. There is a ton of information included in each figure--particularly Figures 1-3. I think it would be beneficial to break some of these into multiple figures in order to make them more digestible. At the very least, the authors should consider changes such as increasing the font size (overall, but particularly in the tiny inset histograms) and increasing the clarity of the captions, even if that means making them longer. It took me a long time to understand that the "thick line" referenced in the Figure 1 caption referred to the border of the text box itself.

We appreciate the comment and agree with the reviewer that figures are quite dense in information. We will do our best to increase readability by increasing font size, extent, and split some of them if necessary.

- Reduce the number of abbreviations in the text. Overall, there are a lot of abbreviations in this manuscript. Certain sections (e.g. Section 3.3) are particularly dense with abbreviations, and correspondingly hard to follow. I would recommend cutting down on the number of abbreviations for clarity.

This comment was also provided by Prof. Lundquist, and we are reducing the number of acronyms in the manuscript. In particular, STS and PGW will no longer be used and spelled out instead. However, we do believe that DOS20, DOQ25 and DOQ50 are necessary to avoid making the paper already longer, and are also easier to follow in our opinion.

- Elaborate on the NoahMP-WRF simulations. It's hard to draw conclusions on this section of the analysis, because relatively little information is given about these simulations. An important feature of NoahMP is that it has multiple options for simulating rain-snow partitioning and snowpack albedo. It also has multiple snowpack-related parameters to which both snow and streamflow are quite sensitive. Without knowing the model physics options and parameters used, it is difficult to conclude whether the biases the authors observed is a structural problem with the model or just a poor setup.

We will provide more details about key information relevant to our work about this simulation as suggested by the reviewer, in particular we will add more details about snow-related processes. However, the details about simulations are provided by Li et al (2017).

Liu, C., Ikeda, K., Rasmussen, R., Barlage, M., Newman, A. J. A. J. A. J. A. J., Prein, A. F. A. F., Chen, F., Chen, L., Clark, M., Dai, A., Dudhia, J., Eidhammer, T., Gochis, D., Gutmann, E., Kurkute, S., Li, Y., Thompson, G. and Yates, D.: Continental-scale convection-permitting modeling of the current and future climate of North America, Clim. Dyn., 49(1–2), 71–95, doi:10.1007/s00382-016-3327-9, 2017.

- Rain on snow. This seems like an important point to discuss in a paper about snowpack and streamflow under climate warming. How well does this new metric handle rain-on-snow events? Can they be resolved and included/excluded? Or are they a confounding factor?

As also noted by reviewer 1, rain on snow events are problematic in our method as we have no explicit way to address the impact of rainfall due to lack of reliable rain/snow observations. It is likely that our method does not capture rain-on-snow events due to the lack (or unlikelihood) of a diurnal shape in the streamflow response, and a solar radiation cycle that can have discrete hourly changes due to changing between clear sky and cloudy conditions (and backwards), resulting in very low correlations. We will improve our discussion to incorporate the reviewer's suggestion. Also, please note the new screening method for rainy days that we are proposing in the answer to Reviewer 1 first mayor comment.