

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
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Comment on hess-2021-437

Anonymous Referee #2

Referee comment on "Diel streamflow cycles suggest more sensitive snowmelt-driven streamflow to climate change than land surface modeling" by Sebastian A. Krogh et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-437-RC2>, 2021

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 (DOS₂₀) 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.

1. 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.
2. 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.
3. 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.
4. 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.

5. 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?