

The revised manuscript has improved substantially, and the authors have addressed the concerns raised by the previous reviewers, particularly with respect to the SWE-focused analysis. I also share some of the concerns noted by Reviewer #2 in the previous round, namely that minor revisions could further strengthen the discussion section. Overall, I find this manuscript close to being suitable for publication.

Q1. Lines 495-505: Could the author comment on the ROS events observed in the study region? For example, information on changes in peak runoff magnitude or timing at sites where INF improved performance would provide valuable insight.

Q2. Table 3: A brief description of the dominant climate, land cover, and snow classifications in the basin (i.e., the relative area covered) would be helpful contextual information to accompany this table.

Q3. Section 6.2: It appears that the default configuration performed better for colder or forested locations. For colder environments, could this be because the STC configuration does not yield significant improvements in runoff, as discussed in Section 5.4? For forested environments, might this suggest that other alternative parameterizations (e.g., canopy-related processes) could further improve model performance?

Q4. Section 6.4: An additional limitation related to frozen soil processes that could be discussed is the representation of preferential flow through macropores (e.g., <https://doi.org/10.1029/2022WR033075>). Could the authors also comment on the finding from this study that increased permeability parameterization for frozen soils improves Noah-MP performance across the continental United States?

Q5. Figure S1: I suggest using a different symbol for the default to distinguish it from the experimental configurations.