

The authors have thoughtfully addressed most of my primary concerns. From my perspective, the manuscript will be suitable for publication after the below two minor, but important, comments are addressed.

Follow-up on Comment 1.4: I appreciate the authors' response focused on the SCF/snow depth comparisons; however, it seems lacking. First, does this explanation (that relies on derivation of SCF/snow depth relationships) suggest that the model used in this study does not explicitly calculate SCF? If that is the case, how are SCF biases (e.g., in Figure 9) calculated? If the model does directly derive SCF, then the scheme used to calculate SCF in the simulations should be reported. Is the snow cover area shown in these plots from the model output; if so then there is clearly an SCF scheme used by the model, and that should be noted in the paper. These figures show distinct regimes for snow depth/SCF relationships for both shallow and deep snowpacks, likely from differences in SCF/snow depth relationships during accumulation vs. ablation periods, which is often parameterized in models based on snow density (e.g., Niu and Yang, 2007).

Niu, G.-Y., Yang, Z.-L., 2007. An observation-based formulation of snow cover fraction and its evaluation over large North American river basins. *J. Geophys. Res.* 112, D21101.
<https://doi.org/10.1029/2007JD008674>

Follow-up on Comment 1.7: Although large scale temperature and precipitation forcing may govern interannual variability of SWE anomalies for a given area, land cover characteristics, such as land cover type, can drive large spatial heterogeneity. For example, the same meteorological conditions that occur in a forest could result in substantially different snowpack dynamics relative to a nearby grassy meadow. This limitation is important to address.