Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-321-RC2, 2020

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Assimilation of citizen science data in snowpack modeling using a new snow dataset: Community Snow Observations" *by* Ryan L. Crumley et al.

Anonymous Referee #2

Received and published: 14 December 2020

The manuscript describes use of a new citizen science dataset (snow depth) to guide simulations of SWE via data assimilation (DA). The motivation is to include observations gathered from locations in the landscape that might not be monitored otherwise. The study is focused on a maritime snow climate of Alaska. The model used (SnowModel) has a long history and is well established. A range of different observations (Snotel, field surveys with depth and SWE, and remotely-sensed snow depth) are used to gauge performance of the DA system, compared to simulations that do not include the depth observations.

The results presented are interesting and there appears to be considerable potential

for use of the citizen science depth observations. However, major revisions could help make the manuscript more useful. The following issues should be addressed:

First, a more complete description of the DA approach is needed. In the introduction, a more detailed comparison of the approach used relative to other snow DA efforts should be provided – beyond what is currently included in the introduction (e.g., L80). How does the approach used compare to other methods, including direct insertion (e.g. Hedrick et al., 2018), particle-batch smoother (Margulis et al. 2019), particle filter (Smyth et al. 2019) and possibly EnKF. The methods section provides only a limited description of how the model is adjusted for mismatch with observations ("SnowAssim aggregates all the assimilated observations by date and creates a spatially varying correction surface that covers the entire model domain (Liston and Elder, 2008). These various correction surfaces are applied by adjusting the model precipitation fluxes and snowmelt factors between SWE observation dates during a second SnowModel simulation"). The 'adjustments' to the model are central to the effort, so the method should be described more completely in the manuscript. The results (or discussion) do not include any documentation of the 'adjustments' to the model, yet one of the benefits of DA is that the merging of data and models is one way to more completely understand the entire system (e.g., see Magnusson et al., 2014 and 2017 and their retrieved precipitation correction factor).

Second, uncertainty of the observations and validation data should be described and incorporated into the analysis. One of the benefits of DA is that the magnitude of uncertainty can be explicitly included in the analysis (e.g., Magnusson et al., 2014 and 2017). It appears that uncertainty of the assimilated observations is not included in the analysis – is this the case? If not, why not? The spatial representativeness of the depth measurements is mentioned in the discussion. One component of uncertainty is related to the conversion from depth to SWE, using the density estimation described in Hill (2019). In the region analyzed, SWE estimates based on density from Hill (2019) have an RMSE of 0.2-0.25 (normalized to snow season precipitation). Is this considered in the DA approach? Uncertainty (or biases) of the validation data is not described, thus it is implied that the data are 'perfect'. What is the error or uncertainty associated with the federal sampler data?

Third, something seems strange about the calibration and validation methods and results. Are the NSE values in Table 1 correct? If the best simulation has NSE < 0, this would suggest that the calibration is not working very well. Additional details are required. Is calibration for the entire year? The entire snow year? Why not at peak SWE? Results in Fig 5 also seem strange. Fig 5e: how can this be the 'best' simulation? There is a clear problem during the ablation period; is it really a "best" simulation if ablation is too rapid? If stats are calculated throughout the season, and ablation season is short, it is easy to discount the errors during this time of year. But doesn't timing of snow disappearance matter? Perhaps a metric of snow disappearance date should be included? One could argue the result in 5f is much worse than 5d, so that assimilation is not improving the simulations, but actually making it worse.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2020-321, 2020.