The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-248-AC2, 2020


# *Interactive comment on* "Snow Ensemble Uncertainty Project (SEUP): Quantification of snow water equivalent uncertainty across North America via ensemble land surface modeling" *by* Rhae Sung Kim et al.

**Rhae Sung Kim et al.**

rhaesung.kim@nasa.gov

Received and published: 10 December 2020

Dear Dr. López-Moreno,

Thank you for your constructive feedback. Below we have provided a response to each of the comments provided by the referee.

J. Ignacio López-Moreno (Referee2)'s comments: The paper analyses the uncertainty of snow simulations by different combinations of global Land Surface models (LSMs) and forcing datasets (FDs) over North America. The paper is interesting and provides



useful insights about the applicability of climate models to have reasonable estimates of snow storage for large areas. The paper is well written and structured, and may be of interest for a wide variety of readers. Despite is not its main objective of the paper I miss a bit more of discussion about the causes of the detected uncertainties, and to present some comparison of the individual members of the ensemble with the snow datasets used as references. Such information could offer clues about the origin of some uncertainties (i.e. if uncertainties are more related to the parametrization of the snow processes, or to difficulties of the models to reproduce the driving variables of their snow energy balance (precipitation, temperature. . .); or even an individual analysis of uncertainty may open the possibility to consider reducing the number of members of the ensemble if some of them shows a clear systematic bias with the reference datasets. At some point is mentioned that "combining a variety of model estimates and allowing the individual model errors to cancel each other out (Xia et al., 2012)". I think this may be true when errors are random or the causes behind the errors are not well identified. However, if some member fails systematically because the snow parametrization is too simplistic or is using more limited observations than others, it is possible preferably to leave out such members from the final ensemble. In addition if some of the forcing data uses observations that are not available, or their density is more reduced, out of the domain of this study (North America) and they provide better results than others, it could be discussed in the manuscript as it has implications when used in other regions of the world.

Thank you for bringing out these issues in the interactive discussion. First, we want to emphasize that the main objective of this paper is to establish an important baseline over the continental scales to characterize current capabilities and inform global snow observational requirements by using a range of forcing products and commonly-used operational models. Given this set-up, we are defining uncertainty as the ensemble spread between these, knowing that the entire set might miss the truth, but assuming that times/places with a higher spread do validly represent greater uncertainties at times/places to focus more measurements or efforts-to-improve understanding. In addition, we acknowledge that some of these models are not state of the art (e.g., we know there are publications saying that CLSM-F2.5 isn't really representative of state-of-the-art in snow modeling) and some of these are not independent samples (e.g. the models have similar underlying parameterizations), but these are representative of data that are currently available and that people might use for their own research, given that these models and configurations are featured in operational systems.

To acknowledge your comment, we added the following text on lines 108-112 on page 4 as follows: "Note that some of these models do not necessarily represent the state-of-the-art approaches for snow modeling and their underlying parameterizations may share a similar legacy in terms of code development. Despite these limitations, however, these models and their versions are representative of systems that provide publicly available snow estimates over continental and global scales."

We are also explicitly looking at two sources of spread – the met data and the model structure (again, from an operational suite), because the literature diverges about which is a greater source of uncertainty. Thus, the SEUP effort also attempts to differentiate from the earlier snow model intercomparison experiments (Essery et al. 2009; Rutter et al. 2009), which assessed errors associated with model structure and parameterizations. In this paper, we are neglecting other sources of uncertainty such as the choice of parameters or resolution of the topography. While understanding the causes of the detected uncertainties and assessment of uncertainty of the individual members of the ensemble is important, we consider it to be outside the scope of this study and leave it for future follow-on efforts.

We have added the following clarification in lines 506-507 on page 16 as follows: "We acknowledge that other sources of uncertainty such as the choice of model parameters and spatial resolution of topographic features are not examined here."

For forcing data concerns in other regions, we briefly discussed this in lines 299-301 on page 10 referred to Yoon et al. (2019)'s study: "Similarly, Yoon et al. (2019) re-



cently showed that the forcing data drove the uncertainty of model simulated estimates (i.e., precipitation, evaporation, and runoff) over High Mountain Asia due to significant differences in the quality of reliable reference measurements over the domain."

Specific comments – It is a bit surprising to me finding in the areas with deeper snowpack the largest uncertainties, since simulating shallow snowpacks is often more challenging than deeper ones.

We agree that simulating shallow snowpacks is often more challenging due to their sensitivity to changes in air temperature, influence of substrate conditions and wind redistribution processes that are typically not included in LSMs. From our results, there is a greater spread in absolute SWE in the deeper snowpacks (in the mountains) where the average SWE and standard deviation between ensemble members are largest. However, we see large uncertainty in terms of normalized SWE across a large portion of the domain (Figure 3b), and there is a greater spread in the total water storage (SWS) in the shallow snow areas that accounts for differences in the aggregated snow estimations over the large areal extent.

I would like to hear the hypothesis of the authors about this result. Are the climate (i.e. precipitation) in these areas more difficult to be simulated?

Yes. The coarser resolution atmospheric models generally do not simulate enough snowfall in the mountains due to their inability to resolve the steepness of the topography.

We've discussed this in lines 378-379 on page 12 as follows: "The coarser resolution atmospheric models generally do not simulate enough snowfall in the mountains due to their inability to resolve the complexity of the topography (Lundquist et al., 2019)."

Is it more/less affected to uncertainties in snow-rain separation?

A static environmental lapse rate of 6.5 K/km from NLDAS1 and NLDAS2 projects is used to apply an elevation adjustment to the coarse meteorological fields. We acknowledge this is not perfect and doesn't work everywhere, but it is a standard approach used in other products. This constant factor could be a source of uncertainty in the rain-snow transition, particularly over mountainous areas. The partitioning schemes used by the models (simple temperature threshold versus transition temperature range) also play a role.

These issues are described in lines 248-253 on page 8 as follows: "These highly complex terrains have relatively high snowfall precipitation, and the large spread is partially due to different rain/snow partitioning schemes in each LSM. While Noah2.7.1, JULES, and CLSMF-2.5 use a simple temperature threshold of 0oC to distinguish rainfall and snowfall precipitation, Noah-MP3.6 includes a transition temperature range described in Jordan (1991) (see Table S1). While our lapse-rate correction method is based on approaches used in other products (see Section 2.3), the lack of considerations of spatial variability in the snow-rain partition is a limitation, particularly over mountainous areas."

Same for different uncertainty between flat areas and mountains (Forcing datasets have less observations in mountains?

According to NOAA National Weather Service's National Operational Hydrologic Remote Sensing Center (NOHRSC) National Snow Analyses, 61% of snow observations (e.g., SWE and snow depth) are located in mountainous areas (defined as a landform that rises at least 1,000 feet) in the U.S. On the other hand, 91% of met stations (e.g., snowfall stations) are located in flat areas. Over Canada, there are fewer available ground and radar measurements than in the continental U.S., which could lead to larger differences in terms of input meteorology.

The uncertainty in mountains is associated to the downscaling technique, or again the reproducibility of the climate there?

Both have implications and these are areas we are hoping to focus future efforts on.



We have briefly discussed this in lines 361-363 on page 12 as follows: "Previous studies also highlighted the limitations of coarse-resolution models, particularly in capturing snow accumulation in mountain areas, and suggested using a resolution of <10 km (Ikeda et al., 2010; Kapnick & Delworth, 2013; Pavelsky et al., 2011; Wrzesien et al., 2017)."

and in lines 378-379 on page 12 as follows: "The coarser resolution atmospheric models generally do not simulate enough snowfall in the mountains due to their inability to resolve the complexity of the topography (Lundquist et al., 2019)."

If the individual members are compared to the reference snow datasets, the complexity of the snow-vegetation parametrization could be used to explain results when forested and non-forested areas are compared?

Thanks for this suggestion. Different snow-vegetation parameterization was used in each LSM (see Table S1 in supporting information). For example, Noah-MP3.6 uses a dynamic vegetation model and semi-tile vegetation sub-grid scheme, but Noah2.7.1 doesn't have both. We are planning to make an evaluation of this parameterization a focus of future efforts, but choose to exclude the details of that discussion in order to maintain a reasonable manuscript size.

The use of 1 or several layers for snowpack could be also identified as potential source of uncertainty shown in the study.

We agree with your comment. If we compare individual members, the snow layering scheme could be a source of uncertainty. Again, we are trying to differentiate this work from previous SnowMIP studies and considering the assessment of uncertainty of the individual members of the ensemble as a new and novel manuscript advancement.

- Line 73: 5 km instead of 5km

We have corrected this.

- Line 207: "Section 3.1 compares the ensemble with observations derived from data assimilation techniques" I would not call them observations, perhaps is better using comparison with the reference snow datasets

We have corrected this sentence (in lines 214-215 on page 7).

Section 3.2.2. Not sure if seasonal variability is the most accurate tittle for the subsection, what about: "Timing of annual peak SWE. . ."

We have revised the title to "Timing of annual peak SWE"

- Figure 5 and 6: Is it possible to add boxplots with the values for the snow datasets used as references? It can give a good indication about some specific LSM or forcing dataset clearly biased from "reality".

Thanks for suggesting this. However, it's difficult to add those for the snow datasets due to their limited spatial coverage. For example, UA and SNODAS only cover the CONUS. CMC only has snow depth information.

-It would be also good to show a figure as 5 (probably as supplementary) but specifically for areas where the annual behavior of snow patterns is known to be well contrasted (often with opposite anomalies; i.e. Cascades and Rockies US) because the average over the entire domain smooths the differences and lead to very little interannual variability.

Thanks for this suggestion. We've added Figure S1 in supporting information and the following text in lines 283-287 on page 9.

"Figure S1 shows two time series of domain-averaged daily mean SWE of the Rockies Mountains and the Cascades in the United States (See Fig. 1b and Section 3.3.1) where the annual snow behavior is known to be well contrasted (Marshall et al., 2019). In the U.S. Rockies, the spread across the ensemble is smaller and the annual maximum SWE is relatively unchanged as compared to those of higher elevations in the Cascades."



- The subsection Section 3.2.5 directly links higher uncertainty with lack of observations. I am not sure if this statement is sound because the reasons of the uncertainties are not well identified along the manuscript.

We agree that the causes of uncertainty warrant greater inspection, and we hope to focus future efforts on these questions. We still feel that this analysis provides insight as to when and where spaceborne observations may help improve SWE and SWS estimation, regardless of the cause. We have added the following sentence in lines 306-308 on page 10 to acknowledge the limitations of the existing study:

"While additional analysis is needed to understand and improve the model parameterizations that are driving the ensemble spread, remote sensing observations have the potential to reduce uncertainty in global SWE and SWS estimation."

Further, lines 506-509 on page 16 adds: "We acknowledge that the influence of the sources of uncertainty such as the choice of model parameters and spatial resolution of topographic features are not examined in this study. Additional work is needed to understand the specific drivers of uncertainty within model physics, better characterize the snow storage over mountain and non-mountainous regions, and quantify the regional influence of SWE uncertainty on water availability."

-Line 368: referring to GRACE: "total terrestrial water storage (TWS) anomaly observations showed reasonable results (not shown)." I do not understand what authors really mean.

We agree it's confusing and have removed this from the text.

- Caption of Figure 8 says "Rockies Canaidan"

The caption has been corrected.

- Section 3.4 is interesting and indeed the topic could be a new manuscript itself. However, as it is presented I have the feeling that many of the conclusions derived from this section are not fully supported needing deeper analyses. Authors may consider remove this section that could compensate to develop more other sections (i.e., assessment of uncertainty of the individual members of the ensemble).

Thanks for this suggestion. We agree that this section is describing a preliminary analysis that requires further exploration. We think this section helps to set up the discussion on required future work and the need for improved snow data to improve streamflow estimation. We've tried to explain explicitly this in lines 461-464 on page 15.

"While this is a preliminary analysis that requires further exploration, it helps to provide insight into the need for improved snow data to improve streamflow estimation. A more detailed examination of the influence of SWE-runoff uncertainties, an investigation into the utility of SWE observations to reduce SWE uncertainty, and thereby runoff uncertainty, are left for future work."

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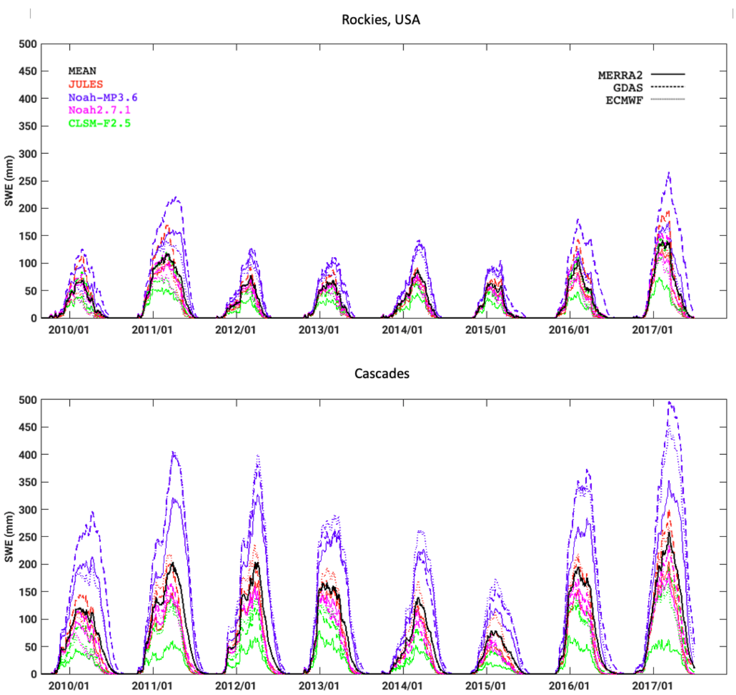


**Figure S1: Time series of domain-averaged mean SWE of Rockies, USA and Cascades. Different colors and line style were used to represent each ensemble; a bold black solid line represents the domain-averaged ensemble mean; the units are mm.**

**Fig. 1.**