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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.

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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

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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.

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. 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? Is it more/less affected to uncertainties in snow-rain separation? Same for different uncertainty between flat areas and mountains (Forcing datasets have less observations in mountains? The uncertainty in mountains is associated to the downscaling technique, or again the reproducibility of the climate there? 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? The use of 1 or several layers for snowpack could be also identified as potential source of uncertainty shown in the study. - Line 73: 5 km instead of 5km -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 Section 3.2.2. Not sure if seasonal variability is the most accurate tittle for the subsection, what about: "Timing of annual peak SWE. . ." - Figure 5 and 6: Â£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". -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. - 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. -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. - Caption of Figure 8 says "Rockies Canaidan" - 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).

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