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Comment on hess-2022-136

Anonymous Referee #2

Referee comment on "Precipitation biases and snow physics limitations drive the uncertainties in macroscale modeled snow water equivalent" by Eunsang Cho et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-136-RC2, 2022

Manuscript #: hess-2022-136

Precipitation Biases and Snow Physics Limitations Drive the Uncertainties in Macroscale Modeled Snow Water Equivalent by E. Cho et al.

Cho et al. use a 12-member ensemble from 4 land surface models (LSMs) and 3 sets of meteorological forcings to assess uncertainties in snow water equivalent (SWE) estimates. Using principle component analysis (PCA), they try to identify the source of error in the SWE estimates and consequently attribute the uncertainty in SWE to various factors such as precipitation bias, etc.

The paper is well-written and easy to read. Results from this paper can guide advancements in land surface modeling of SWE, which is important since errors in SWE can translate into errors in other hydrologic variables such as runoff and soil moisture. The paper highlights the extent that biases in precipitation can contribute to uncertainty in SWE as well as other factors.

General Comments:

It is important to acknowledge that there are uncertainties in the SNOTEL measurements early on in the paper. Also, SNOTEL measurements are not necessarily representative of the surrounding regions. Please clarify this as it relates to l.149, which seems to contradict l.341-349, where the authors do acknowledge that in-situ measurements may not always be representative of surrounding areas.

Several of the findings appear to be consistent with previous work. It is not always clear what additional findings beyond those consistencies would add to the existing body of literature. Include additional discussions that clearly show the value of this work instead of simply confirming what others have previously found.

It would be valuable to consider biases in timing as it could introduce uncertainty into the season totals and subseasonal dynamics of the snowpack. Has the timing of melt events been carefully evaluated as event timing impacts the total accumulation of SWE?

There are a few different analysis windows that are referred to as winter in the paper. For instance, 1st October to 31st May is used in some cases; however, winter accumulated precipitation is cumulative precipitation from October 1st through the date of the maximum SWE. It is unclear why a consistent time period is not used. Also, different terminology should be employed to differentiate these various "winter" periods, especially since 1st October to 31st May includes other seasons.

Specific Comments:

Section 3.1/Figure 1. It seems that difference maps would be more powerful for conveying the uncertainty across LSMs relative to SNOTEL than the current maximum SWE maps and dates. Consider including difference maps in the main text with the SNOTEL column from the current Figure 1 and moving the other 4 columns to the supplement to show the actual magnitudes and dates.

Figure 2b. In the Figure 2b caption, mention what the vertical lines represent.

Section 3.3/Figure 4. Include an equivalent map for the SNOTEL snowfall and how the snowfall is distributed across elevations. Difference maps relative to SNOTEL would be useful.

l.271: Remove language such as “seem” here. In an effort to avoid any potential confusion since NLDAS is not used by Cho et al., it may be better to rephrase as “Our results differ from Pan et al. (2003) who concluded that the winter temperature bias was generally constant with elevation when using NLDAS.”

l.359: Should l.359 read 9 and 3 “km” as opposed to “km^2” since Pavelsky et al. (2011) and Wrzesien et al. (2017) use resolutions of 27 km, 9 km, and 3 km?