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The Cryosphere Discussions

Interactive comment on "Independent evaluation of the SNODAS snow depth product using regional scale LiDAR-derived measurements" *by* A. Hedrick et al.

Anonymous Referee #2

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This paper describes the evaluation of gridded snow depth results from SNODAS against LiDAR and in situ data gathered during CLPX-2, and provides a rare evaluation of an operational product–SNODAS–which, as the paper states, is difficult to evaluate directly against observations "as a consequence of the framework's data assimilating nature." The evaluation is performed using two sets of independent measurements, which makes the results particularly compelling. This aspect of the work is enough by itself to recommend the paper, given that the analysis appears to have been done with sufficient rigor and competence, and is described quite clearly.

The method used for quantifying uncertainty in the LiDAR estimates, which affects the choice of physiographic regions for further discussion of results, as well as the various

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speculation about the nature of SNODAS errors in those regions, could benefit from some refinement, as discussed in my comments below. Finally, additional comments on the potential for improving SNODAS estimates would be beneficial to the text.

Comments on content:

1. I do not understand the comment "it is paramount that no snow melt occurred between the survey dates in order to properly assess the snow depth component of SNODAS using LiDAR estimates alone." (p. 3150, l. 20-22). I can understand that snow accumulation is the primary phenomenon being examined, but why is it a problem if snow melt occurred during the 81-day span separating the measurements? In section 4 the integrative nature of the measurements is discussed (p. 3153, l. 21-24), and it does not seem to be an obvious problem if snow melt is another contributing process.

2. SNODAS uses quantitative precipitation as its forcing, and assumes a constant bulk density for newly fallen snow. Therefore, SWE is the primary state variable produced by that system, while snow depth is derived as a function of SWE and snowpack density. This relationship is acknowledged (but with the positions of SWE and depth reversed) in the introduction (p. 3143, l. 14), but the following comment that "snow depth varies considerably more than bulk density over space" (p. 3143, l. 16-17) serves to diminish the importance of modeled snow density. However, this came back to me as I read the discussion of the in situ vs. SNODAS comparisons shown in Figure 5b (p. 3152, l. 14-19). SNODAS appears to underestimate snow depth when the observed snow is deep: is it possible that SNODAS overestimates compaction or initial density in these areas of high accumulation?

3. I'm having a little trouble with the comparisons shown in Figure 8. Basically, the RMS difference between the in situ and LiDAR data is treated as a random error, but the data shown in Figure 6 depict a systematic error causing the datasets to differ, as discussed in section 4 (p. 3153, l. 3-18). I suppose the use of the RMS difference between the data sets means that neither is considered authoritative, and is a hedge

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against preferring one over the other. However, given that the LiDAR depths are systematically lower than the in situ depths, it seems more appropriate to treat the LiDAR data as negatively biased, and for example to depict the uncertainty associated with them as a more narrow range centered around a negative value. Perhaps this is not the ideal approach given that the difference between the two sets of observations seems somewhat proportional in nature (i.e., the slope of the line in Figure 6 is significantly less than 1.0), but further discussion of the choice to treat the RMS difference as a random error estimate seems warranted.

4. The discussion for "region #1" (p. 3156, l. 5-19) suggests that SNODAS has failed to account for persistent snowpack sublimation in that region, which is certainly possible, even likely. However, the discussion implies that wind speeds that drive SNODAS and its simulation of snow sublimation are inaccurate because there are no nearby SNOTEL sites. I don't find this convincing. I don't see that the proximity of a SNOTEL would make any difference with respect to whether or not wind speeds are well represented in SNODAS forcing data in the area. Isn't it possible that observed wind speeds are assimilated into the forcings used by the SNODAS model, since it uses NWP analyses as its primary forcing data, and that many of these come from observation stations other than SNOTEL sites? Perhaps the winds are well represented, but the SNODAS model nevertheless fails to simulate the extent of snow sublimation occurring in the region.

5. In the discussion for "region #2", why does sub-kilometer scale heterogeneity of snow distribution cause SNODAS to underestimate, and not overestimate, snow accumulation (p. 3156, l. 10-12)?

Editorial comments:

1. P. 3148, l. 1: organizeded -> organized

2. P. 3155, l. 22-23: the geographic location of the pixels are in a region -> the pixels are in a region

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