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

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General comments

1. *Specific deficiencies could be identified in the SNODAS model, in order to diagnose the sources of bias in the SNODAS output.*

   Response: This avenue was discussed among the authors prior to submission, but it was ultimately decided to be beyond the scope of this paper to delve quantitatively into the inner workings of the model. Future work is planned to identify specific shortcomings of the model for different local environmental

   variables such as elevation, vegetation cover, and slope/aspect by working with the NOHRSC analysts that work with the data assimilation aspect of SNODAS. However, a thorough investigation of regional environmental variables such as weather patterns and long-term climate fluctuations may require higher temporal resolution of the validation datasets (mentioned in #2).

   We don't have access to the source code so we aren't able to test any of the specific output biases from SNODAS, but we have added more speculation within the Conclusions section. In particular, the SNODAS results prior to assimilation were not available for this study, which prevented us from distinguishing errors due to model physics or implementation from errors caused by the assimilation.

2. *The analysis could be bridged to additional years of data, either from SNOTEL or other sources in order to add an assessment of interannual model performance to the analysis (although I realize there are major limitations and challenges to this).*

   Response: The SNOTEL network is much too sparse to perform an analysis of this scope and extent. We will include text to outline the fact that SNODAS is considered a spatial average of conditions over a 1-km^2 pixel, while the measurements that are used for assimilation are only representing one point within that pixel. The major advantage of this LiDAR dataset was the data continuity of the snow depth changes over an area large enough to effectively assess a 1-km^2 resolution snow model. All SNOTEL and other publically available sources of data have been used in SNODAS during assimilation, therefore there are not additional years of independent data available. Performance assessment therefore is not possible in other years; this was a unique opportunity for this reason, but was unique as LiDAR surveys of this scale and resolution are expensive and rarely performed.

Specific comments

1. *Page 3145 line 24: ". . .while an assimilation step give analysts the ability to decide every day whether to augment the model estimates. . ." I don't know the details of the SNODAS assimilation approach, but this statement implies that there is manual intervention by analysts with respect to the use of observations and the assimilation is not standalone. Is this true? If so this raises a lot of potential ambiguities.*

   Response: Yes, there is manual intervention by analysts. All available nation-wide electronic point data is used to adjust the estimates created by the down-scaled NWPs and NOHRSC Snow Model (NSM). Given the difficulty of keeping an automated sensor functional, the analysts must perform rigorous quality control on the data to make sure it can be used. Although this causes ambiguities for the scientific use of SNODAS, it is the only operational SWE product and is produced for operational use every day at 1 km resolution over the entire lower 48 states. This study evaluated the accuracy of this product and speculated the potential causes of error, however determining the causes of error was not possible since the source code as well as open-loop model estimates were not available.

2. *The Introduction is well written and thorough. But it's not until the final paragraph of the section that objectives of the paper are referred to. I suggest moving this forward to near the beginning of the section to engage the reader earlier in the goals of this study. The objectives are not explicitly stated at any point in the introduction.*

   Response: Agreed. Some of the Introduction will be reorganized and some statements added to more succinctly state the objectives of the paper.

3. *Page 3150 paragraph 1: I like the hourglass approach to in situ sampling over a 500×500 metre area. But were only ~50 snow depth measurements made at each hourglass? This seems like a very small number. Calculating conservatively, a 500×500 metre hourglass is composed of over 2000 metres of linear*

   sampling distance. 50 measurements equates to a snow depth measurement only every 40 metres. Does this capture the local scale variability? You have to walk the whole hourglass anyway, why not make more measurements?

   Response: The manual measurements were conducted to complement the CLPX-II POLSCAT airborne radar mission, for which bore different goals for ground validation. The resolution was meant to provide an accurate estimate of the mean SWE over the hourglass footprint, which also included multiple manual density profiles that were not part of this study, and had to be completed quickly by a two-person team. At the resulting resolution, the surveys definitely does not accurately characterize small-scale variability, and 40 m spacing was chosen to be less than the estimated correlation length.

   Additionally, because of location uncertainty stemming from GPS technology, we will never have an exact 1:1 correspondence of any ground truth measurement with any remote sensing measurement. Though use of other types of ground truth such as terrestrial LiDAR can provide more accurate ground locations, horizontal uncertainty still exists in the 5-meter airborne LiDAR grid of snow depth change. Increasing the sampling resolution on the ground only changes the sample size of available comparison measurements, but the horizontal uncertainty remains the larger issue. With the current sample size we are able to get all the necessary summary statistics (mean, std dev, RMSD, etc.). Regarding water resources applications, we are more interested in how the hourglass sampling technique represents the snowpack over significant areas than we are at points.

4. *Page 3150 paragraph 2: Perhaps I'm missing something, but why is it 'paramount' that no snow melt occurred between the two dates of LiDAR acquisition? You have produced a snow depth difference field from all three datasets, and the assessment is of delta depth. Given that the region was not snow free at the first date, it's okay to have a negative change.*

   Response: You are absolutely correct. The way the text was written implied that no melt could have occurred in order to conduct this study using snapshots of snow distribution. This is simply not the case. What we intended to show by including the melt discussion and figure was to rule out possible causes of model error. Since the model showed insignificant melt during this period and temperatures were rarely above freezing, we expect snowmelt physics are not contributing factors to model bias with regards to this study. This works to narrow down the possible causes of discrepancies between modeled and measured snow depth change and argues that melt processes are unlikely to have been the cause. This portion of the manuscript will be rewritten to better reflect the reason for showing total SNODAS melt.

5. *Page 3150 line 22: "...estimates of snow melt due to incoming solar radiation and sublimation...". This statement requires clarification. Snow melt and sublimation are two different phase changes that will reduce the snow mass, and they are driven by different processes.*

   Response: This text was part of the melt discussion that will be rewritten. The routines within SNODAS that estimate snowpack mass loss incorporate the physics of sublimation due to wind and solar radiation. Therefore, where model estimates of melt were negligible, we can narrow our focus to other processes (i.e. new snow density, densification, and compaction) that may have caused discrepancies between SNODAS and LiDAR.

   A similar yet inverse study could be tackled by conducting LiDAR surveys at peak SWE then again at a date near the complete melt out, thus looking only at the physics at play during the ablation season. In such a scenario we would want to make sure that no accumulation occurred between the LiDAR surveys so we could assess the accuracy of the melt and sublimation routines within SNODAS as opposed to the new snow density and compaction routines.

6. *Figure 5 is effective at showing the tendency for SNODAS to increasingly un-*

derestimate snow depth relative to the in situ measurements as snow depth increases. Figure 6 essentially shows a similar pattern for the LiDAR (although lower in magnitude). Why not combine these figures, using different symbols for the in situ vs. SNODAS and in situ vs. LiDAR results? This would provide a direct comparison relative to ground measurements, including the systematic bias over deeper snow.

   Response: This is a terrific suggestion. A combination of Figures 5 and 6, shown here as **Fig. 1**, condenses the information that both figures are conveying and we will combine the figures for the final submission.

7. *Page 3153 line 18: a value of ± 13 cm is provided for the LiDAR data relative to the in situ measurements. But this is somewhat misleading as there is systematic bias in this comparison: LiDAR snow depth is always shallower than the hourglass measurements. Would it not be possible to bias correct the LiDAR estimates of snow depth based on these results?*

   Response: For the twelve in situ sites, the upscaled 1-km^2 LiDAR snow depth change is consistently 5-10 centimeters less than the upscaled manual measurements. This does result in a negative bias, which can physically be explained by the combination of a few important factors:

   - LiDAR snow-free surface elevations are over-estimated due to limited penetration of vegetation and/or imperfect vegetation filtering.
   - Manual snow depth probing can often penetrate the ground and some studies have indicated a small 1-5cm positive bias.

   There is a relatively constant bias between SNODAS and in-situ, but the difference between LiDAR and in-situ has a bias that changes with snow depth. Since it is not clear what is causing the bias and which depth estimate is the cause, we chose to not apply one to make this more clear. The average 5-10 cm underestimation in the LiDAR measurements is also well within the vendor-recommended uncertainty (10-15cm absolute vertical accuracy) and does not ultimately have a strong effect on the comparison with SNODAS.

8. *Page 3154 line 15: Is it worth summarizing the regression results in a table? There is not much detail provided here.*

   Response: The results were not included due to the fact that the LiDAR snow depth change was overwhelmingly the greatest cause in discrepancy between LiDAR and SNODAS. The other predictor variables that were hypothesized to have an impact on SNODAS – LiDAR differences turned out to not be nearly as important as the actual snow depth change. We will leave the inclusion of a table of the regression results up to the discretion of the editor.

9. *Figure 8: I suggest using different symbols for the points corresponding to each of the outlined areas in Figure 9. This would explicitly show which points come from which area.*

   Response: Agreed. Revealing each region specifically within the scatter plot better quantifies the results and strengthens the hypothesis of region-specific uncertainties.

Technical corrections

1. *Drop 'Independent' from the title.*

   Response: The word 'independent' was included to highlight the fact that validation datasets for SNODAS are extremely rare, especially at the regional scale.

2. *Page 3143 line 5: 70% of the water supply to what geopolitical region?*

   Response: 70% of the water supplied to populated regions of the Western U.S. originates in mountain snowpacks, which is directly from Carroll, et al (2006).

3. *Page 3153 line 20: The relationship between SNODAS and LiDAR snow depth has an R^2(coefficient of determination) of 0.72, but note that this is described as 'correlation' in the text which should be expresses as r not R^2.*

   Response: We will remove all instances where r^2 is referred to as correlation and replace them with explanations of the independent and dependent variables.

Interactive comment on The Cryosphere Discuss., 8, 3141, 2014.

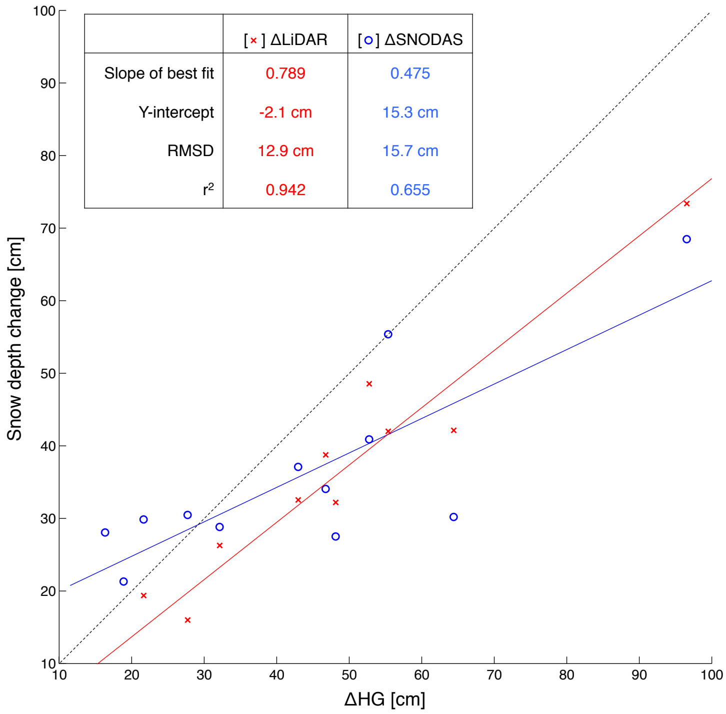

Fig. 1. The combined Figures 5a and 6, which conveys the same information but increases readability.