The Cryosphere Discuss., 8, C1265–C1270, 2014
www.the-cryosphere-discuss.net/8/C1265/2014/

The Cryosphere Discussions
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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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Received and published: 13 July 2014

This paper is at the cutting edge of snow research at large spatial scales. This is the first large-scale LiDAR snow depth (actually change in depth, in this case) data set to be presented in the literature and it's being used to validate the important SNODAS modeling product, which is available for the continental US. The paper is concise, clear and well done. The overall approach is sound.

This work represents a potentially very important contribution towards advancing snow modeling and its validation at large spatial scales. However, it needs to go a bit further and isolate key directions for future research, both in terms of LiDAR-based validation

and assessment of SNODAS. This is essentially the main complaint of the first reviewer and I agree. Forging ahead with a (potentially speculative) discussion will be challenging but should be attempted regardless because of its potential value. I have several suggestions in this regard. I hope they are useful.

Summary of my comments is

1: A comparison of SNOTEL and SNODAS is lacking which will greatly improve the context of the results. Because SNOTEL roughly governs SNOTEL (via data assimilation), this context will provide substance for extended discussion. 2. A discussion should more generally frame the results in terms of potential water yield. More importantly, what are the next steps for improving SNODAS using future LiDAR data sets similar to those in this paper? What aspect of the LiDAR acquisition will be key to get right next time? Are the LiDAR errors actually small enough for a comprehensive validation? This is important to consider because LiDAR snow depth is perhaps the best opportunity to understand and improve the SNODAS products or other, similar model estimates at large spatial scales. Could the LiDAR be assimilated? How much ground truth would be necessary to properly bias correct the LiDAR? Etc.

— Specific comments (roughly from major to minor/editorial) —

1. Interpretability of results

Given the analysis in the paper, my lingering question is "why is SNODAS wrong?" Or, why is SNODAS right?

It will be impossible to answer this comprehensively because we dont know the exact assumptions in SNODAS. To address comment 1 of reviewer 1: yes, there are "MODS" in SNODAS (at least this is generally believed). This is (still) fairly standard practice for operational products (e.g. Seo et al, 2009). New validation products, such as presented in this paper, will hopefully lead to comparisons of MODS assimilations and automated assimilation procedures and advance the science. The upshot of the MODS is ambiguity in how to improve the results. This will make for challenging speculation in the discussion. However, efforts along these lines could be a significant benefit to the community and help push the science forward.

My thoughts on the initial question:

a) SNODAS is going to be correct owing to SNOTEL observations

b) it's going to be wrong moving away from SNOTEL in space as (MODS) assumptions about variability break down.

To me this explains why SNODAS is not simply biased, but the line of best fit intersects the 1-1 line. I'd guess that the intersection is roughly near the magnitude SNOTEL observations. That's not going to be exactly true, but makes a reasonable story. I think this general idea is sketched on P3154 L3, but it deserves clarification and expansion along with the relationship of the SNOTEL observations to the results. There should be speculation about why the assumptions moving away from SNOTEL are likely wrong and how we might fix that. Wind is mentioned in passing. How about vegetation? Other differences in physiography with the assimilated SNOTEL obs? The entire regression analysis centered on page 3154 strongly suggests to me that the SNODAS assimilation/MODS (which are the errors away from SNOTEL) are not based on any of these explanatory variables which we commonly expect to govern snow depth. This may or may not be true, but it appears that such predictors are not the basis of the MODS. I currently know of now snow depth/SWE assimilation technique which actually uses such variables. So, it's not really surprising.

I know there are already multiple analyses of different products in this paper, but a comparison is needed of the SNOTEL and SNODAS used in the study. This will help to illuminate the above points. For the two SNOTEL locations in the LiDAR footprint, I would suggest also including the LiDAR spread and mean information for the SNODAS pixel and also for a, say, 10-15m radius centered on the SNOTEL.

Also hindering interpretation of the results is that SNOTEL information is somewhat hard to see where it does exist in the paper. While it's nice to see the SNOTEL positions overlaid, it makes it difficult to interperet the colored values in figs 2, 4, and 9 at the location of the SNOTEL, which is of acute interest. This is challenging to fix and make obvious, and is part of the reason I suggest treating these comparisons in a separate figure. Improving the readability of this figure is needed to help with interpretation in the spatial context. I dont have a great suggestion for how best to do this. Smaller symbols would help fix this, but be more difficult to see. Perhaps empty squares or diamonds centered on SNOTEL? Similarly the HR symbols can block the information which they overlay and make interpretation of the underlying values difficult.

Figure 1 is very nice. My concern, again, is about promoting interpretability. Showing the ΔLidar in the figure detracts from our ability to use topography in that region as context for interpretation of results. I suggest removing it here and combining it with Figs 2 and 4, in this order 1, 4, 2 as panels of a single figure. I think being able to compare ΔLidar, ΔSNODAS, and ΔSNODAS-ΔLiDAR in the same figure is important to the interpretation. Flipping pages detracts from the interpretation. I'd also include SNODAS values in another panel in the same figure. This will help promote a coherent discussion where these things are easily compared.

Also, woud a figure showing vegetation (e.g. NLCD or MODIS) contribute to interpretation?

As mentioned above, the results (the difference between LiDAR and SNODAS changes) should be put into the context of difference in potential water yield or potential energy balance effects. I'm talking about back of the envelope calculations with simple assumptions. Other suggestions for discussion questions were also offered above.

The last thing I'm interested in, which maybe less relevant and more of my own pet interest, is the subgrid distribution of ΔLiDAR for each SNODAS pixel, where the

SNODAS value falls in the distrbution, and trying to explain the variance using predictor variables. (OK, This could easily be a separate paper).

2. Horizontal error bars on figures 5 and 6 could place SNODAS more broadly within the measurement context.

3. (Same as reviewer 1, comment 7; P3153 L18) It nags at my conscience that you're using $\pm$13cm from RMSD as the error range for the LiDAR. I think a simple discussion (1 sentence?) justifying why this is appropriate would be helpful. My concern is that the errors are biased so that they are not symmetric about zero. If you plot the distribution of these errors, the mean is not zero. The assumption of $\pm$13cm is similar to assuming 1 standard deviation of a mean-zero distribution? Also why is 1 standard deviation, or whatever exactly RMSD represents, appropriate? It's not the same as 1 stdev if there's a bias. The conclusions are somewhat dependent on this assumption, so it should be clearly argued.

4. (Same as reviewer 1, comment 3; P3150 L20) The argument about why melt being insignificant is lost on me. Why is this important? Clarification needed. Related to this, a time series "spaghetti-plot" of all the SNODAS pixels along with their mean would illuminate SNODAS behavior during the Δtime.

5. (Same as reviewer 1, comment 5; P3150 L22) Do you mean just change in depth (what I'm calling Δ) instead of melt? Sublimation doesnt really cause melt, it probably has the opposite effect like sweat cools the body.

6. (P3151 L19) The word "model" is a bit ambiguous. You could change to "its" or "SNODAS".

7. (P3152 L26) "mean HG" wasnt defined as "mean HG difference" previously. I assume that's what you mean. Generally I'd suggest revising the notation to use deltas, it would be clearer: Δlidar, Δsnodas, Δhg.

8. (P3153 L21) Seems like bias and RMSE should be mentioned in this paragraph. It's

on the figure and important.

9. (P3153 L21) "potential explanatory physiographic variables" might be a better expression than "potential physiographic parameters".

10. (P3155 L6) This paragraph would benefit by starting with it's final sentence.

11. (P3155 L17) SNOTEL used for assimilation are also in the trees which affects solar radiation as well. This is a point which seems to be worth exploring or mentioning.

12. (P3155 L22) "changed" is vague. Is "accumulated more snow" better?

13. (P3156 L3) "over-distributing" is just vague. You could use more words if you think the point is important, but I'd just remove "over-".

Reference:

Seo, D. J., Cajina, L., Corby, R., & Howieson, T. (2009). Automatic state updating for operational streamflow forecasting via variational data assimilation. Journal of Hydrology, 367(3), 255-275.

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