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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.

Anonymous Referee #1

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General Comments

This manuscript utilizes airborne LiDAR measurements and in situ snow depth observations to evaluate snow depth changes simulated by SNODAS across an alpine study area. The analysis is sound, is described clearly, and provides a well validated comparison of three different snow depth estimates (SNODAS; airborne LiDAR; manual snow depth measurements).

The overall scope of the paper is, however, relatively limited, and the final conclusions section is thin. (recognizing, of course that the acquisition of the airborne LiDAR data and the ground surveys required a major effort). The paper would be significantly

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improved if (1) specific deficiencies were identified in the SNODAS model, in order to diagnose the sources of bias in the SNODAS output; and (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). As it stands, the paper uses high quality and validated measurements and the analysis is sound, but the overall innovation would benefit from some additional analysis. In addition, I have a number of other suggestions outlined below.

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.

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.

3. Page 3150 paragraph 1: I like the hourglass approach to in situ sampling over a 500 x 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 x 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?

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

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of delta depth. Given that the region was not snow free at the first date, it's okay to have a negative change.

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.

6. Figure 5 is effective at showing the tendency for SNODAS to increasingly underestimate 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.

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?

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

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.

Editorial Comments

Drop 'Independent' from the title

Page 3143 line 2: "...an important ecological component of Earth's water cycle." Not clear what is meant by 'ecological component'.

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

Page 3143 line 9: consider changing ‘hydrologic snow models’ to ‘distributed snow models’

Page 3151 line 4: change ‘snow height’ to ‘snow depth’

Page 3152 line 10: the use of ‘substantiate’ seems odd here

Page 3153 line 20: The relationship between SNODAS and LiDAR snow depth has a Rˆ2 (coefficient of determination) of 0.72, but note that this is described as ‘correlation’ in the text which should be expresses as r not Rˆ2.

Page 3155 line 9: provide a reference to the Figure 8 along with the mention of the ‘pink vertical stripe’. I would change ‘stripe’ to ‘shading’.

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