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Interactive comment on "What drives basin scale spatial variability of snow water equivalent during two extreme years?" *by* G. A. Sexstone and S. R. Fassnacht

Anonymous Referee #1

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General comments

The paper investigates physiographic controls of the spatial variability of snow during two subsequent winters in a 2729 km2 catchment in Northern Colorado (USA). To arrive at snow datasets that better represent the study area (@50% SCI) operational snow data from the NRCS were complemented with field data sampled during a series of field surveys centered around April 1st in 2011 and 2012. The authors constructed a snow density model which converted snow depth into SWE values in order to decrease the requisite work required to further sample SWE distribution during the supplementary surveys. Both, the development of the snow density model and the concluding analysis to answer the title question employed multiple linear regression techniques.

In general the paper reads well and most details are available to retrace the authors approach. Taking the effort to sample additional field data to mitigate issues with the representativeness of data from operational snow monitoring sites is an asset of this study. However, the use of multiple linear regression entails limitations that impact the overall assessment of the paper. Below are a few key points to be considered.

i) The snow density model was constructed using historical data from 17 NRCS snow courses. The authors mention that "snow courses are often located in flat open areas, limiting the ability of the dataset to represent the variability explained by those variables", speaking of topography and tree canopy. Two pages beforehand we read that spruce-fir, lodgepole pine, and ponderosa pine forests cover a majority of the study area. From this information it seems that the density model cannot be representative of the study area. The authors may argue that most of the variability in SWE is explained by snow depth not density. However, if this is the case, it is unclear why the authors needed a snow density model at all if it cannot cope with topography and canopy coverage. The respective variables (canopy density and terrain parameters) are exactly the ones that only show minor relevance in table 4.

ii) Regression models are calibrated separately for each of the two years and two datasets: operational data only (O) and operational data supplemented by survey data (O+F). Table 4 lists the model performance. Using the respective calibration datasets as a basis for the performance assessment, results arrive at the misplaced conclusion that operational regression models (O) perform better than their counterpart which additionally include survey data (O+F). The authors would have arrived to a different conclusion if they had included the survey data to assess the performance / utility of the operational regression models (O). This discrepancy within the performance assessment analysis requires reworking.

iii) Using multiple regression models such as equation 1 or in table 4 entails important limitations: Is snow distribution in complex terrain a linear function of a small number of physiographic controls? If not, what can we learn if we assume so in the analytical statistics? The following examples serve as an illustration. First, quite a few studies have shown that the correlation between snow density and elevation is a function of DOY. Inferring from the analysis from equation 1 that elevation is not a major driver of variability in density may be the result of an overly simplistic modeling approach. Secondly and similarly, table 4 seems to suggest that canopy density is not an important factor in explaining the spatial variability of SWE (minor contribution in 2 model optimizations and no contribution in 3 model optimizations). However, common sense as well as a multitude of studies from this region suggest a different story.

Specific comments

p.2949 / l.17: If data for less than 0.13m was omitted, why is this data included in figure 3 (top panel only)?

p. 2950 / l.18: Figure 3 suggests that snow depth is not normally distributed, why did the model diagnostics not suggest a variable transformation?

p. 2953 / l.3: The authors mention maximum upwind slope Sx as a suitable physiographic control, but used terrain curvature instead. Calculating curvature as the second derivative of the DEM at 30m may not be the best alternative. Consider using a larger fetch for the calculations.

p. 2959 / l.6: The authors should rethink their conclusion here. As emphasis, if all but two points are removed one will arrive at a perfect model, but what is then the value of the model?

Interactive comment on The Cryosphere Discuss., 7, 2943, 2013.