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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

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This research develops basin scaled snow density and SWE models to evaluate the physiographic controls on basin scale SWE spatial variability. The research is conducted in the northern Front Range of Colorado, U.S.A. The authors use primarily GIS data to drive the models and a combination of operational and manually-collected data to develop and evaluate the models. The paper is generally well written and organized. I would like to see the authors better integrate the snow density model with the SWE variability analysis (this should be a simple matter). I would also like to see them further evaluate the density model against two recently published density models and make recommendations for future research (i.e. is it worthwhile to formulate a basin-specific model like they did rather than apply one of the others?). This latter recommendation shouldn't be too difficult either. There are however, limitations to the manually collected datasets (e.g. non-uniform, non-random spacing), and unanswered questions regarding how this affected the presented results that may prove a little more difficult. At the very least, I recommend the authors be more conservative in evaluating their results and the conclusions they present. While acknowledging some of these limitations (p. 2960: "The model is however constricted to its spatial domain, range of physiographic inputs, as well as temporal coverage, thus may not be applicable to areas outside of the study area, ...") in the next paragraph they state, "... suggesting this method is applicable for basin wide, regional, and global scales". I recommend a far more critical evaluation before including statements akin to the latter. My main concerns and recommendations are outlined below followed by line-by-line comments after that

Main Concerns:

i) The authors develop a snow density model and evaluate the physiographic controls on the observed snow distributions. At many times these seemed like two separate paths. It is not clear how they are linked or why the density model was necessary in order to achieve the main goal of this work: "What drives basin scale variability of SWE?" It seems that there was already sufficient snow density data to make these evaluations. I would like to see the authors better integrate the two paths to show how the density model furthered their evaluation of basin scale SWE variability. Another option would be to change the title of the work so that it is representative of the full breadth of research presented.

ii) The authors used multiple linear regression to model non-linear processes. The limitations of this approach have been widely recognized and the authors should acknowledge these limitations in their work.

iii) The authors present a new density model. I would like to see them delve further into

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how this new model differs from the two recently published density models (Jonas et al. and Sturm et al.). I would like to see questions such as these answered: a. How would the other models have performed in this basin? b. Does this reveal regional tendencies that might affect and guide future applications? c. What, if anything, was gained by having a model directly calibrated to your basin?

iv) It seems as though a host of predictors were thrown into the mix to see what stuck to the wall so to say. The authors never present any data (e.g. p-values) to validate the ones that "stuck" (i.e. appear in Table 4 and the final snow density model). It is hard to believe that some of these predictors listed in Table 4 have p-values less than 0.05 – commonly taken to represent statistical significance – given respectively poor bivariate correlations with SWE presented in the Table 3 survey data (e.g. slope in modelO+F11 with a correlation of -0.04 in Table 3). This may be why confounding results appear in Table 4 that are not adequately addressed in the text. If I correctly understand Table 4 increasing canopy density leads to increased SWE in two of the models. This contradicts all findings of forest canopy effects on SWE distributions prior to substantial melt. Eastness is also used in three of the regression models depicted in Table 4, yet what is the physical bearing of eastness on snow accumulation? Table 3 indicates that eastness can be positively or negatively correlated to SWE. I would like the authors to delve further into these issues in order to validate that their model is physically robust and that their conclusions based on 2 years of data can be considered representative of the drivers of basin scale spatial variability in other years.

v) I also question how the distributions of some of the predictors might be affecting the regression results. Since the spacing of samples is not consistent – three large clusters of closely spaced data (∼500m) and wider spaced operational data (10s of km) – it is very difficult to discern what is driving the results. Are trends in the closely-spaced data affecting the basin-scale analysis? Or is it the opposite? And how does this vary for each predictor? On a slightly different note, the authors acknowledge that the sampled distributions of elevation, slope, northness and solar radiation are not

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representative of the basin/50% SCI distributions. Solar radiation, widely recognized as a strong influence on snow distribution, only appears as a predictor in one of the five SWE distribution models in Table 4. The authors need to address the robustness of their conclusions given that one of the most important controls wasn't adequately sampled.

vi) Another example of how the distribution of predictors might be affecting the results is that the combination of northing and easting can clearly separate the data into the three concentrated sampling areas. As the authors note these three areas have very different snow regimes. Northing and easting were found to be the most reliable predictors of SWE. It is entirely possible that these conclusions are essentially being driven by $n \sim 3$. Plots of easting and northing with SWE and a deeper exploration of these data would help to differentiate clear physical trends from sampling-dependent implications on the derived statistics.

Line-by-line comments:

Abstract

- Define basin-scale in terms of actual values/areas here where the reader first encounters this term

- Provide further insights into results from density model

Introduction

- p. 2945, lines 7-11: You could leave out "in such complex terrain . . ." on lines 8-9 as these were mentioned in previous sentence. Then combine these two sentences into a single statement on scale issues between observations and snow heterogeneity.

- p. 2945, lines14-15: hourly SNOTEL data is also available

- p. 2945-2946: This 8-line sentence would be a lot easier to consume if it were broken up into at least two maybe three sentences.

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Study area and datasets

- p. 2947, line 8. Prior to this sentence there hasn't been a statement that the research area is in the Front Range of Colorado. A statement of this nature should be made early in this paragraph.

- p. 2947-8. I think this could be explained better and a figure depicting "sampling locations", "transects", and the "systematic sampling spacing" would be helpful. I got confused with the term "location". On line 18 "location" seems to refer to one of the three sampled regions (?). While on line 23 "location" seems to refer to an individual point (?).

- p. 2947, line 15. You mention the "similar" elevations of the two SNOTEL stations; it would be helpful to the reader if they knew just how close these were. Line 11 would be the perfect place to insert their elevations.

- p. 2948, lines 5-6. How come these numbers (42 and 121) don't match up with Table 2 (28 and 104)? Does this have to do with which sites had co-located density measurements? If so, state this ... and why some locations did not have a density value. Though I wonder if this is the case as n=84 for the density measurements (Table 1). Obviously I am confused. Either I'm lug-headed or a better explanation is in order.

Methods

- p. 2949, lines 1-11. I think you could swap the positions of sentences one and two and eliminate sentence three. The fourth sentence sounds more like a conclusion statement rather than a methods this-is-what-we-plan-to-do statement.

- p. 2949, lines 12-20. You should mention that each snow course data point consists of the average of approximately 5 measurements.

- Paragraph 3. This paragraph belongs in the Introduction/Background section, not in Methods. I'm not familiar with, nor did I have the time to review the Mizukami and Perica work. Certainly however there are others that have shown that density can exhibit inter-annual variability, particularly in this continental region (e.g. Jepsen et al., 2012, Table 2; Balk and Elder, 2000). This bears mentioning.

Basin scale SWE variability

- p. 2952, line13. Winstral et al., 2002 used a derivation of slope (maximum upwind slope) which is very different from a pixel's slope angle as referred to here. The Winstral et al. work does not support/substantiate the application of slope as applied in this work.

- p. 2952, line 26. What is H in W H m-2 ?

- p. 2953, lines 3-7. Curvature is defined as the second derivative of slope and is calculated from all surrounding cells regardless of direction. Profile curvature is curvature measured parallel to the direction of maximum slope. The authors describe profile curvature on line 7 yet consistently refer to this as curvature. Please be clear on which was used in this application.

- p. 2953, lines 20-23. Montesi et al. 2004 would also be an appropriate reference here as that study was conducted in the Colorado Front Range.

- p. 2954, line 3. I would swap field and operational to maintain consistency with the "O+F" terminology (i.e. operational first; field second).

Results

- p. 2954, lines 18-20. This point can best be summarized using the coefficient of variation (CV).

- p. 2954, line 21. "Snow density . . . shows a strong positive correlation with snow depth" contradicts statements in the previous paragraph and the data presented in Fig. 3b.

- p. 2954, line 25. What are the p-values for these predictors? Are they really all significant?

- p. 2955-6. “. . . was one of the least snowy years on record” might be better said as “one of the lowest snow years on record”

- p. 2956, lines 9+. This paragraph begs for a better introduction. Let the reader know exactly what is coming and why you are doing it. Also explain what SCI is. Additionally it wasn’t clear to me what was being compared. In the K-S tests are the WY11 and WY12 distributions being compared to one another or is each WY’s distribution being compared to the SCI distributions?

- p. 2957; lines 14-21. Be clear that n=2 and that the two years differed. The only solid point you can make is that the snow distributions relative to northing exhibited variability. Make that point and then follow with reasons why that might have been.

- p. 2958, first paragraph. Make it clear that when RMSE values are stated these refer not to the modeled RMSE for the transformed variable, but that they have been converted back . . . if in fact that is the case.

- p. 2960, line 3. Shouldn’t LiDAR be mentioned here?

Discussion

- p. 2959, line 21. I think “though” should be “through”

- p. 2960, lines 13-16. This sentence could probably be eliminated.

- p.. 2961, line 9. This reference to the work of Erickson et al. takes a finding of Erickson et al. out of context in order to substantiate the results of this work. From the abstract of Erickson et al., “The terrain parameters considered were elevation, slope, potential radiation, an index of wind sheltering, and an index of wind drifting. When nonlinear interactions between the terrain parameters were included and a multiyear data set was analyzed, all five terrain parameters were found to be statistically significant in predicting snow depth, yet only potential radiation and the index of wind sheltering were found to be statistically significant for all individual years.” So in fact Erickson et al. did find that certain predictors were temporally consistent predictors of

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SWE distributions over a seven year period. Interestingly the two consistent predictors could not be properly assessed in this study: solar radiation was insufficiently sampled and an index of wind sheltering was not considered (though I suspect at these sampling sites wind shelter like solar radiation would not be representative of the basin wide distribution).

- I would prefer to see direct comparisons, similarities and differences between the density model presented here and those of Jonas and Sturm. I would also like to see the authors make recommendations on when and where each of these models might be more appropriate.

Acknowledgements

- Might be nice to acknowledge the NRCS for collecting and providing all the SNOTEL and snow course data.

Table 4

- In the caption mention that the dependent variable was SWE (mm)

- You should also put in units for each variable

- n values for each model would also be nice, so the reader doesn't have to refer back to Table 2

- Greater canopy density means greater SWE? Care to explain?

- What is the physical meaning of eastness? I know how to derive it, but what physical bearing does easting have on increasing snow deposition?

- As I mentioned earlier, are all these variables really significant predictors? What are the p-values for these predictors?

Figure 1

- I found this figure very difficult to discern when printed out. Though you can zoom

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in on the electronic version and see things plainly there are some who might prefer to have a print copy.

- Increase font sizes throughout, including on the scale bars.

- Add a paragraph to the caption describing what SCI is. The reader encounters this figure early in the work, but SCI isn't addressed (and at that vaguely) until much later in the work

- Contour lines are very hard to see and at least some contour lines should be labeled

- Does the legend in the main figure also apply to the callouts (e.g. elevations)?

- On a minor note, if possible remove the basin outline that isn't the Cache la Poudre from the main figure

Figure 4

- Both the American-English and British-English versions of modeled/modelled used.

Figure 6

- Include in the caption a description of the callout bar graphs.

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