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The Cryosphere Discussions

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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A short excerpt of each referee comment is provided and followed by our response (which is denoted by an asterisk)

Anonymous Referee #1

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)...

In general the paper reads well and most details are available to retrace the authors

approach...

*We would like to sincerely thank Anonymous Referee #1 for their review. The referee's comments provided useful information on limitations that needed to be addressed within the revised manuscript. The suggestion of evaluating a larger fetch for curvature led us to perform additional analysis that improved the consistency of the models developed. Also, our conclusions based on model results were improved based on their recommendations. We believe that the revised manuscript clearly outlines limitations and provides a more robust description of the results presented. Below we address each of the referee's comments and concerns.

i) The snow density model was constructed using historical data from 17 NRCS snow courses...

*There are certainly limitations in the representivity of the snow course dataset that the snow density model was derived from (and we have tried to address this in the revised manuscript). However, we believe that the snow density model is an important component of this basin scale analysis and the historic operational data is the only large dataset of snow density that is available at this scale. Referee #2 also raises a similar concern as stated above: "it is unclear why the authors needed a snow density model at all". We have changed the title of our paper and tried to state more clearly within the text the reason for the breadth of the research presented and why the snow density model is important in the context of basin scale assessments of snow distribution. We believe these changes have resulted in a more succinct paper the clearly outlines why this analysis was useful for future basin scale applications. Please see our response to the main concern number 4 of Referee #2 below for our discussion on the importance and robustness of variables that show minor relevance as referenced in this comment.

ii) Regression models are calibrated separately for each of the two years and two datasets...

*We have revised all sections related to the SWE model performance (as well as Table

5 and Figure 6) to reflect the assessment of model performance using all operational and field-based survey data for both models O and O+F.

iii) Using multiple regression models such as equation 1 or in table 4 entails important limitations. . .

*Referee #2 voices a similar concern. We recognize the limitations of using multiple linear regression to model non-linear processes and have acknowledged these limitations in the discussion of the manuscript (citing previous work that highlights these limitations). However, it should be noted here that a multiple linear regression model is required to be linear in the combination of its parameters, but variable transformations can allow for inclusion of variables that are not normally distributed within the model and produce non-linear response surfaces. We do believe that our choice of multiple linear regression for the models developed within this study was appropriate given the goals of the study. The historic snow density dataset used to develop the snow density model was shown to be normally distributed and was best fit as a linear function when plotted against SWE, which lead us to choose the linear modeling approach. We would have ideally used binary regression tree models to develop the O and O+F SWE models, however, these decision tree models require large datasets (at least > 200) in order to provide meaningful results. Given that the main goal of the SWE models was use as a tool to evaluate the importance of individual variables rather than use strictly within a predictive framework, the multiple regression models provide simplicity of the interpretation of model coefficients. Therefore, although snow distribution in complex terrain is not simply a linear function of a small number of physiographic controls, we can gain more insight into what the most important variables are that drive this distribution by modeling it with this simplistic approach. Although previous studies (e.g. Jonas et al. 2009) have found that the relation of snow density and elevation is dependent on day of year, our dataset shows that there is a positive correlation between snow density and elevation throughout the snow season. Please see our response to main concern number 4 of Referee #2 below for our discussion on the problems observed with the

canopy density variable.

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

*We have removed the snow depth data less than 0.13 m and SWE data less than 50 mm from Figure 3a. This was an oversight and these data were not used within the snow density model development.

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

*We agree that the historic snow course snow depth data are not observed to be normally distributed, although the residual diagnostic plots of the snow density model did not suggest that a variable transformation was necessary to satisfy the assumptions of the linear model. However, after applying a square root transformation to snow depth within the model, the constant variance of model residuals was improved, thus we have included the square root transformation in an updated model. The updated snow density model was then carried through within all calculations in our analysis. The updated model resulted in very minor differences in model performance statistics compared to the original model presented.

p. 2953 / l.3: The authors mention maximum upwind slope Sx as a suitable physiographic control, but used terrain curvature instead. . .

*In our original analyses, we evaluated profile curvature only, however upon further testing (based on the recommendation of this comment as well as a request for clarification from Referee #2), we have included terrain curvature (profile and planform curvature combined) at a 30 m resolution and 100 m resolution in lieu of profile curvature only. Overall, the terrain curvature is shown to be a stronger physiographic control than profile curvature, and was selected within each of the revised models. The larger terrain curvature fetch was shown to be more important for the 2012 dataset while the smaller fetch was more important in 2011.

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?

*We have revised our conclusions here. Please see our response to the Referee's second key point above.

*Thanks again for your helpful comments.

*Please also note the supplement to this comment.

A. Winstral (Referee #2)

This research develops basin scaled snow density and SWE models to evaluate the physiographic controls on basin scale SWE spatial variability. . .

*We would like to sincerely thank Adam Winstral (Referee #2) for his comments and suggestions that have helped to improve the quality of this manuscript. This review was able to point out limitations that needed to be more clearly addressed within the manuscript and also provided helpful advice on how the pieces of this study could be better integrated to meet the main objective. Additionally, the suggestion of comparing our snow density model with previously published models was useful for making recommendations for future basin scale snow surveys similar to what is presented here. We believe that the revised manuscript is more focused on a singular objective and more clearly outlines the limitations in the work presented. Below we address each of the referee's comments and concerns.

Main Concerns:

i) The authors develop a snow density model and evaluate the physiographic controls on the observed snow distributions. . .

*Referee #1 also raises a similar concern to the one stated above: "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". We have changed the title of our paper so that the main objective is focused on evaluating snowpack properties (SWE and snow density) at the basin scale rather than SWE only. We have added additional text to the manuscript about the importance of the snow density analysis and also the limitations of snow density datasets representing the variability of canopy and terrain at the basin scale. Additionally, we do feel that the snow density model we develop was ultimately a key component in our basin scale analysis of SWE variability, as one of the main goals of this paper is to aid in the methodology and sampling strategy of future basin scale analyses. Although we may have had sufficient snow density measurements to fill in the gaps of our snow depth measurements, this will likely not be the case with future basin scale field snow surveys and lidar snow depth retrievals, so there was a need to test this methodology at this scale. We have added more text within the manuscript to make these motivations for developing the snow density model clear.

ii) The authors used multiple linear regression to model non-linear processes. . .

*Referee #1 voices a similar concern. We recognize the limitations of using multiple linear regression to model non-linear processes and have acknowledged these limitations in the discussion of the manuscript (citing previous work that highlights these limitations). However, it should be noted here that a multiple linear regression model is required to be linear in the combination of its parameters, but variable transformations can allow for inclusion of variables that are not normally distributed within the model and produce non-linear response surfaces. We do believe that our choice of multiple linear regression for the models developed within this study was appropriate given the goals of the study. The historic snow density dataset used to develop the snow density model was shown to be normally distributed and was best fit as a linear function when plotted against SWE, which lead us to choose the linear modeling approach. We would have ideally used binary regression tree models to develop the O and O+F SWE models, however, these decision tree models require large datasets (at least > 200) in order to provide meaningful results. Given that the main goal of the SWE models was use as a tool to evaluate the importance of role of individual variables rather than use strictly within a predictive framework, we identified the multiple regression models as ideal given the relative simplicity of the interpretation of model coefficients.

iii) The authors present a new density model. . .

*We have applied the snow density alpine model developed by Sturm et al., 2010 to our dataset and directly compared the results to those of our snow density model. We saw this as an appropriate exercise given their model was developed for global applications and data from the western U.S. were used for model development. We did not however test the model developed by Jonas et al., 2009, as this model was developed specifically for Switzerland and requires regional and elevational parameters that are specific to this area, and was not intended for use in other areas. Overall, the Sturm et al., 2010 snow density model performed well when applied to this dataset, but was slightly out-performed by our model. We have made direct comparisons between the models as well as made recommendations for future basin specific applications within the discussion of the manuscript (see supplement).

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 variables that were evaluated within bivariate screening as well as within the model selection process were done so with reason, based on known or hypothesized influences on the variability of snowpack properties. We have included additional text within the manuscript that clearly explains how we would expect each variable to influence snowpack variability (please see explanation of the physical meaning of eastness within the manuscript supplement). We have also indicated which bivariate correlations (within Table 1 and Table 3) were shown to be statistically significant. Additionally, we have inserted a new table (Table 4) that includes the partial correlations between SWE and terrain/canopy variables when the correlation effect of elevation, UTM Easting, and

UTM Northing is removed. These partial correlations help explain why variables with seemingly low bivariate correlations can show up in the final model selection. All variables that were included within any final model are statistically significant and we have stated this explicitly in the manuscript. Additionally, in an effort to ensure that our models are physically robust, variables were only included in a final model if their respective coefficient influenced the model in a manner that is to be expected. Finally, upon further evaluation of the canopy density variable we have noted that SNOTEL stations, which are known to be located within open areas (with zero canopy density), tended to have high values of canopy density, which was producing the confounding results of higher canopy density equaling higher SWE. This brings up another limitation that we discuss further in the revised manuscript of how representative a SNOTEL pillow (as well as snow courses and our field measurements) is of the 30 m resolution GIS pixel is sampled. We have removed the canopy density variable from our analysis and replaced it with the categorical variables of “canopy cover” in which the presence of canopy = 1 and open = 0. This variable is based on field notes that we recorded at each of the sampling locations. The variable shows a negative correlation with SWE as expected.

v) I also question how the distributions of some of the predictors might be affecting the regression results. . .

*We acknowledge the limitations of our sampling strategy (e.g. non-uniform, non-random spacing), and have included more text in the revised manuscript to describe these limitations. However, given the extent of the study area we are evaluating (1493 km2), it was necessary to employ this transect based sampling strategy because of the formidable challenge of accessing these areas throughout the basin on skis. The “closely spaced” systematic sampling transects with 500 m spacing of sampling locations where generally around four kilometers in length, which is a similar extent (in length) as that of many of the watershed scale alpine studies that have been evaluated in the past. Each of these transects were located along an elevational gradient with generally varying terrain which provided a range of snow conditions to be sampled. We have done further evaluations (which we discuss in the revised manuscript) of the bivariate correlations within each of the three sampling clusters that the referee mentions. The significant correlations in each of the individual sampling clusters are similar to those of the operational dataset, which suggests that the 500 m spacing of field-based sampling locations is likely large enough to represent our scale of interest. Please see the supplement for further discussion on the importance of the clear sky solar radiation variable.

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. . .

*We have provided an additional figure (Figure 7) that shows a plot of UTM Easting and UTM Northing with SWE to further explore the influence of UTM coordinates on the distribution of SWE. The plot shows that although there are clear regional trends with the distribution of SWE across the study area, the importance of UTM Easting and UTM Northing is not dependent on the sampling strategy of the dataset. This is easily verified, as the operational models (modelO) tend to show similar importance and effect of UTM Easting and/or UTM Northing as that of the operational and field-based model (modelO+F) from the same year. We believe that the variables or UTM Easting and UTM Northing help to quantify how storm tracks have moved across the basin throughout the snow season.

Line-by-line comments:

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

*Basin scale has been defined in the abstract.

- Provide further insights into results from density model

*This has been included.

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. . .

*This change has been made in the revised manuscript.

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

*We included this information.

- 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.

*This sentence has been edited.

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. . .

*This statement has been inserted to the first sentence of this section.

- 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. . .

*We have included a callout sampling diagram in Figure 1 and also revised some of the text to make the different terms used here more clear.

- 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. . .

*Elevations have been inserted here.

- p. 2948, lines 5-6. How come these numbers (42 and 121) don’t match up with Table 2 (28 and 104). . .

*These numbers (42 and 121) were a mistake and have been fixed. They included a number of sampling locations outside of the 50% SCI that did not have snow during the survey, and therefore were not sampled. The n = 84 snow density measurements include monthly snow density measurements that were made throughout the 2011 and 2012 snow seasons. This dataset was not used within this analysis with the exception of testing snow density model performance. We have included a statement clarifying this in the revised manuscript.

Methods - p. 2949, lines 1-11. I think you could swap the positions of sentences one and two and eliminate sentence three...

*These changes have been implemented.

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

*This statement has been included, however most snow courses around the study area include approximately 10 measurements.

- Paragraph 3. This paragraph belongs in the Introduction/Background section, not in Methods...

*We have moved this paragraph within this section and relabeled the section as Background and Methods as to not require an additional Background section alone that would clutter the manuscript. We have mentioned the works of Jepsen et al., 2012 and Balk and Elder, 2000 in the context of inter-annual variability of density.

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...

*This citation has been removed.

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

*Watt hours per meter squared. These values have been converted to the more standard unit for solar radiation of Watts per meter squared.

- p. 2953, lines 3-7. Curvature is defined as the second derivative of slope and is calculated from all surrounding cells regardless of direction...

*In our original analyses, we evaluated profile curvature only, however upon further testing (based on the recommendation of Referee #1 as well as a request for clarification from this comment), we have included mean curvature (profile and planform curvature combined) at a 30 m resolution and 100 m resolution in lieu of profile curvature only. Overall, the mean curvature is shown to be a stronger physiographic control than profile curvature, and was selected within each of the revised models. The larger mean curvature fetch was shown to be more important for the 2012 dataset while the smaller fetch was more important in 2011. We have stated more explicitly the definition of the curvature that we use in this study.

- 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.

*This citation has been included.

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

*All of these instances have been changed

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

*We have used the CV to make this point.

- 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.

*This sentence has been reworded to state "positive correlation" instead of "strong positive correlation".

- p. 2954, line 25. What are the p-values for these predictors. . .

*All p-values are statistically significant ($p < 0.05$) as this was used as a condition for variable selection within the model. We have stated this explicitly in the manuscript.

- 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"

*We have made this change.

- p. 2956, lines 9+. This paragraph begs for a better introduction. . .

*We have edited the introduction to this paragraph. SCI has been defined more clearly within the study area section when it is first introduced. The K-S test compares each WY's distribution to the SCI distribution to assess how representative the sampled physiographic variables are of the study area. We have edited the text to make this more clear.

- p. 2957; lines 14-21. Be clear that n=2 and that the two years differed. . ..

*We have edited the text based on this recommendation.

- 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. . .

*Done.

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

*Yes, we have mentioned in now.

Discussion - p. 2959, line 21. I think "though" should be "through"

*This change has been made.

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

*We have eliminated this sentence.

- 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. . .

*Our point here with this citation was not that Erikson et al., did not have any consistent predictors from year to year, but rather that the additional predictors included (outside of the most important variables) tended to vary from year to year (e.g. slope, elevation). Nonetheless, we have removed this citation to avoid confusion. We do not see the other part of this comment as a very good comparison with our study. Erikson et al. (as well as many other snow distribution studies) assess snow variability across a small (2.3 km2) alpine watershed. It cannot be assumed that the same processes dictating snow distribution in these areas are consistent across the basin scale, especially considering the impact of forest canopy on solar radiation and wind.

- I would prefer to see direct comparisons, similarities and differences between the density model presented here and those of Jonas and Sturm. . .

*As discussed in the response to main comment number three, these changes have been implemented.

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

*We have included the NRCS in our acknowledgements.

Table 4 - In the caption mention that the dependent variable was SWE (mm). . .

*Table 4 referenced in this comment is now Table 5 in the supplement. We have updated the table caption, units, and n values per your recommendation. As mentioned earlier, we have removed the canopy density variable from our analysis and are using the categorical variable canopy cover (1 = canopy cover, 0 = open), which shows up in modelO+F12 with a negative coefficient as we would expect. Also, we have described the physical meaning of eastness in our response to main comment number four above. All predictors are statistically significant, as this was a requirement to be included within a model.

Figure 1 - I found this figure very difficult to discern when printed out. . .

*We have edited Figure 1 to make it more easily readable when printed out (including font size and line weight) and inserted contour labels. The elevation legend in the main figure does also apply to the callouts; therefore no changes were made here. The figure caption was edited to include a description of the study area as the Front Range of Colorado as well as a definition of SCI. Finally, we included an additional callout (sampling diagram) that will help when referencing the sampling strategy in the Study Area and Datasets section.

Figure 4 - Both the American-English and British-English versions of modeled/modelled used.

*The American-English version “modeled” is now used exclusively.

Figure 6 - Include in the caption a description of the callout bar graphs.

*Figure 6 now includes a description of the Beta coefficient bar graphs.

*We would like to thank Adam Winstral again for his thorough and helpful review.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/7/C2186/2013/tcd-7-C2186-2013-supplement.pdf

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