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Interactive comment on "Modeling bulk density and snow water equivalent using daily snow depth observations." *by* J. L. McCreight and E. E. Small

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Authors' Reply to Comments

We sincerely thank the two anonymous referees and Dr. Jonas for taking the time to provide us valuable feedback on our research. Their summaries help us contextualize our work from the point of view of others in the field. Their questions and criticisms are invaluable to improving the relevance and impact of our work. We answer each question and suggestion below.

We have not replied to the comment of V. Mahat. In the absence of a particular question, the suggested references did not appear directly relevant to improving our work.

Following our responses to the reviewers, we added two of our own suggestions which have developed since the paper was submitted.

Because all reviewers' comments are minor, we have phrased our replies in terms of the actual revisions to our manuscript when possible.

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Interactive Comment

Anonymous Referee 1

Minor comments

1. One of the major contributions of this paper is the consideration of snow density dynamics at two different timescales. The distinction of snowpack dynamics at both short timescales (days) and longer timescales (months-seasons) provides a unique synthesis of previous snow density research. There is an opportunity to highlight this synthesis explicitly in the Introduction by mentioning previous research focussing on short-term snow density dynamics and those focussing on long-term dynamics.

   In the introduction and discussion, we now mention the work of Chen. We state that observational data supports the importance of distinct processes governing density at different timescales as well as the importance of thermal/energy processes at longer timescales.

   Intro:
   "Our main innovation is to separate the observed negative correlation between depth and density over short timescales from their positive correlation at larger timescales. The importance of different governing processes at separate timescales has been observed in field studies of bulk densities (e.g. Chen et al., 2010). Incorporating behavior at distinct timescales into our new model provides more realistic daily timeseries of bulk density."

Discussion:
"While a strength of our model is the requirement of only snow depth observations, the associated drawback is that energy-related processes are not explicitly included via predictor variables. Energy-related processes explain a large fraction of bulk density variability at long timescales (e.g. Chen et al., 2010) and are only implied via their relationship with snow depth variability and the climatology of fit."

2. If the Sturm and Jonas models were not developed for daily estimation then why use them for comparison? The answer is because there really aren't any methods developed for daily estimation without using physically-based models. I think this point needs to be made more clearly in the manuscript particularly when the authors re-iterate that the Sturm and Jonas models were not developed for daily estimations.

   We have clarified in the introduction:
   "Because no previous models exist for the daily time step, we compare our model against these earlier models, highlighting improvements offered by our model when applied to daily snow depth observations."

3. While the motivation of the paper was to make best use of the snow depths measured by an existing GPS receiver network (along with other depth observations), the model/method may be more useful to the broader snow research community if there was an option to remove the dependence on neighbouring snow density observations. Many regions simply do not have sufficient snow density data (i.e. 3 sites within 70km with observations taken at 5 day intervals) to implement this type of model. I wonder how much influence the climatology predictor has on the model performance? While this is partially addressed by the author in Appendix B, perhaps a breakdown of how much influence each of the predictors (in particular the climatological predictor) has on the estimated snow density could

be included to improve model transparency and transfer to regions without any density observations.

We've improved the discussion to more clearly state the challenges of and potential avenues for investigating parameter transfer of our model to wider physiographic situations. We clarified that the climatologies we use are climatologies of fit and depend on local training data as much as the parameters. We indicate that a general sensitivity analysis (including both data and predictors) is needed to move forward:

"Widespread application of our daily density model is hindered by the use of local (70km) data for fitting model parameters and deriving the climatology of fit. We have characterized model accuracy/errors for this scenario. A study of model sensitivity to proximity of training data, the effects physiographics on model parameters, and to predictor variable quality would provide greater context for wider applications. While measurements of the spatial variability of snow depth have improved drastically with the advent of LiDAR measurement techniques (McCreight et al., 2012), the spatial variability of bulk density remains poorly understood (Moreno-Lopez et al, 2012.). In this study, we have considered the spatial variability of density over scales of tens of kilometers. However, the density observations in this study come from SNOTEL which have their own kind of homogeneity when compared to the larger environment. They are most commonly located in forested, sub-alpine locations. Our findings are likely to be more representative of these locations as opposed to unforested, or wind-exposed locations (e.g. Clow et al., 2012). Transferring parameters derived at SNOTEL sites to snow depth measurement locations such as for GPS, which are necessarily unforested and tend to be at lower elevations, may introduce model bias. Though some issues related to parameter transfer can be studied with SNOTEL observations, understanding parameter transfer in a broader context may require new and physiographically diverse timeseries observations of snow bulk density."

"Alternatively, our model might also be explored or extended using hybrid approaches. Model sensitivity to more general climatologies such as those of Borman et al. (2013) could be investigated. Physical models could also provide simulated data or climatologies over a wide variety of physiographic conditions. Our statistical model could be fit to synthetic data to explore its sensitivity or to develop general-use parameters (e.g. Sturm et al., 2010)."

In appendix B, we have added the following sentences to address the issues of parameter importance:

"While the figure indicates the importance of the individual predictors, it is important to realize that this is specific to our experimental design. The relative influence of the predictors may change under different parameter transfer scenarios."

4. The manuscript focuses on correlations between snow depth and snow density, which addresses the mechanical compaction processes, and precipitation influences. Where do snow metamorphisms (which are largely temperature driven) fit into the new model framework? I am not suggesting any additional analysis, as precipitation is the major driver of snow density variability, but it might be nice to briefly mention snow metamorphisms in the Discussion and how they might affect the results.

   We added a paragraph to the discussion (also mentioned above):
   "While a strength of our model is the requirement of only snow depth observations, the associated drawback is that energy-related processes are not explicitly included via predictor variables. Energy-related processes explain a large fraction of bulk density variability at long timescales (e.g. Chen et al., 2010) and are only implied via their relationship with snow depth variability and the climatology of fit. Because air temperature can be easily measured or estimated with reasonable accuracy, it seems that the best way to improve our model formulation with additional observations would involve adding an air (or other) temperature vari-

able. Research would be required to understand how such observations would most appropriately modify the current terms. Successful inclusion of an air temperature predictor in the model would likely improve transferability of parameters between physiographically dissimilar locations. Temperature observations might also be used to constrain changes in density or SWE to physically realistic scenarios.”

5. In the new model, deviations from the climatological snow density profiles (interannual variability) are controlled by inter-annual variability in precipitation, through observed snow depths. While previous research has shown that precipitation is the dominant source of inter-annual variability (Bormann et al., 2013), I wonder how the model performs at sites with large temperature variability (i.e. frequent fluctuations about the freezing point). Perhaps a breakdown of highly variable sites (such as those along the western US) compared to less variable sites (such as those in the inland continental US) – based on climate characteristics - could be included. Again, this is just a comment and not a formal suggestion.

   Because model performance at such locations is so intertwined with parameter transfer in the context of this study, we think this is best addressed the context of a separate study focusing on these issues. This was touched upon in the discussion, which we have slightly expanded based on this comment. We now also discuss how the model might be expanded with a temperature observation/predictor, which touches on this topic.

Line comments

1. Page 5009, Line 2: I’m not sure what is meant by ‘basic’, consider removing or improving the scientific context.

Removed. Expanding/improving seems unnecessary in the context of “The Cryosphere.”

2. Page 5010, Line 13: I think prior to this statement, the introduction should support why daily snow density estimates are required.

   In the introduction, we stress the recent and expanding wealth of snow depth observations at daily or better time resolution.

3. Page 5011, Line 17: I am not entirely clear on which three processes you are referring to here. I think they are: a) rapid short-timescale reduction in snow density with new snow; b) rapid short-timescale compaction after new snowfall; c) percolation of surface melt to rapidly increase density with depth. This needs to be clearer perhaps list them with numbers or letters or explicitly state them.

   Yes, these are the three. I have reworked the paragraph with enumeration to make it clearer.

4. Page 5011, Line 28: Physically at seasonal timescales the snowpack undergoes internal metamorphisms, along with mechanical compaction, which combine to cause an increase in pack density. While the manuscript focuses on mechanical compaction processes (inferred by adopting snow depth as the main predictor variable), I think metamorphism processes should be mentioned.

   Now addressed in the discussion. The paragraph was quoted above in our reply.

5. Page 5021, Line 22: should ‘systematic errors in both density SWE’ read ‘systematic errors in both density and SWE’?

   Yes.

6. Page 5025, Line 3: correct the (14%/20%) I’m not sure what this means. 14/20? Or 14-20?

I took it out and readers can consider the "one-third" as needed. It's not particularly difficult to understand.

7. Page 5029, Line 26: '. . . compared to existing approaches which were not intended for daily application.' Prompts the reader to question why the comparison was made using these models, a question which should be addressed clearly in the introduction so there is no need for re-iteration here. Consider revising this statement to '. . . compared to daily application of existing statistical approaches.' or likewise.

   As above, we have clarified in the introduction:
   "Because no previous models exist for the daily time step, we compare our model against these earlier models, highlighting improvements offered by our model when applied to daily snow depth observations."

8. Page 5030, Line 10: the abstract states that 'at least 3 stations are available for training' and the conclusion states 'trained to more than 2 sites'. While these statements both infer that 3 or more stations are required to apply the model the wording should be the same to avoid ambiguity. Consider revising the conclusion to 'trained to at least 3 sites'.

   Changed.

9. Page 5047, Fig 9: consider increasing the x-axis label interval to avoid the staggered x-axis labels, particularly in the bottom panel. This is an aesthetic comment only.

   I prefer the extra information and the staggered labels, especially as many of the plots only show 3 numbers. Could potentially drop zero, but the small improvement is not worth the effort.

10. Page 5049, Fig B1: Sturm has been misspelt in the legend.

    Fixed.

T. Jonas (Referee)

Specific comments

1. Page 5013, line 23-25. In fact, month is an implicit predictor by using separate parameterizations per month. Moreover, the model can be used for any singular snow depth measurement, and multiple observations within a given month are neither assumed nor required.

   Yes. You are right. I've fixed this.

2. Page 5015, line 23-24. Maybe nitpicking, but using the given accuracy values in the error analysis may not represent a realistic scenario. I suspect these values represent resolution, not accuracy.

   This is not nitpicking at all. Your are correct and I agree. We just needed to be clearer about our assumptions. I have expanded the section and reworded.

   "Several authors have noted systematic errors with SWE pressure measurements used by SNOTEL observations (Johnson and Marks, 2004; Meyer et al., 2012). We do not attempt to identify or correct any such errors, which would require temperature observations at the ground-snow interface or site-by-site comparison of SWE against accumulated precipitation. Also, because these studies indicated that measurement errors may be of both signs, it is prohibitively difficult to reason about the impact of such errors on our results. After the manual quality control mentioned above, we assume the SNOTEL measurements are accurate to within their observation resolutions. The resolution of the SNOTEL SWE measurements is .1 inches (.254cm) and that of snow depth is 1 inch (2.54cm). Assuming these are our only sources of error, an error analysis of the calculated density over our full data set using half these accuracies as the error in each variable found 80% of the density errors to be less than 1% and 99.5% of density errors to be less

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than 20%."

3. Page 5018, line 26-27 / Page 5021 line 20-22. As correctly cited, the Jonas model was never intended to be applied to daily time series. However, in cases where this is done nevertheless, best practice for a given HS measurement is to calculate SWE for the 15th day of each month and then interpolate the resulting SWE time series to the specific date at which the HS value was measured. This simple procedure allows to get completely rid of the jumps in bulk density between months. I realize that this procedure was not mentioned in the original publication, but it is common practice in all applications I am aware of. For obvious reasons I will not ask the authors to adopt this practices and redo the analysis, but I'd appreciate if the authors would manage to include this information as a short note.

   We address this on (what was formerly) page 5018 line 26-27:
   "Though we do not include the methodology in our analysis, an unpublished modification to the original Jonas model has been implemented in practice which eliminates these discontinuities between months (T. Jonas, personal communication and review of this paper). The modification requires estimating SWE on the 15th day of each month and linearly interpolating it on to intervening snow depths before training the model."

4. Page 5022, line 10, and similar occurrences. I'm probably biased here, but I think the authors could refer to "structural errors" if they'd find bad model performance in cases where these models where used as intended (c.f. page 5028, line 9). How about "structural simplicity" or "simple model structure"?

   Throughout, I've changed "structural errors" to "structural errors at the daily time step" - or something similar. Being more specific about the errors is definitely warranted, especially given that the Jonas and Sturm models were not intended for daily data and that your model (Jonas) is more appropriate than ours for the timescale which you intended it.

5. Page 5029, line 4-7. An interesting topic that is brought up here. Probably content for future research, but adding a retrospective variant of the full model in Appendix B would certainly serve readers that deal with real-time applications.

   As this analysis is not critical to our paper and may benefit from additional research (e.g. further investigation of smoothing/filtering techniques), we leave analysis of real-time application to future investigation.

Anonymous Referee 3

Specific comment

1. Section 4.1. Page 5021. Line 22. Missing "and" between "density" and "SWE"

   Yes.

2. Section 4.2. Page 5024. Line 2. Sentence starting "However, ...". I don't understand meaning or implication of this sentence. Maybe restate different way, or can be deleted?

   This is simply an observation on the nature of the range of the SWE errors. I've rewritten to make it clearer:
   "The width of the inner 90th quantile of SWE errors increases with day of water year while the width of inner 50th quantile is constant after April (though there are many fewer data points after May). Notably, the SWE errors do not follow the seasonal increase and decrease of snow depth."

3. Section 4.3. Page 5025, Line 18 - . The figure shows pdf of the coefficients of

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your model is quite tight (which is good) compared to the others. Maybe worthwhile stressing that this supports transferability of model coefficient.

This may be right. But without actually studying it properly, I'd prefer to not speculate on the transferability of the coefficients. I think this is a topic for a separate paper, which we elaborate on the need for in the discussion.

4. Section 5.1, page 5026, line 19 - . It sounds like the authors did not use cross-validation (e.g., develop the model with one-year-out and applied the model one year)?? If I am right, I wonder why you did not do cross-validate at single site.

   We've clarified by rewriting part of the paragraph:
   "Because density climatology (ρ_{clim}) is a predictor in our model, we fit the model separately for each year while dropping the year to be estimated from the training data set (leave-one-out cross validation on a water year basis). We do not apply this cross-validation to the Sturm and Jonas models because it will have a negligible effect. This procedure is more stringent for our model, but only slightly penalizes years with extreme densities."

5. Appendix A. page 5031, lines 2-5. This sentence intrigues readers. Wondering about the proportions of each phase at the other sites. Might be good to state approximate range of percentages over all the sites.

   The appendix discussion has been expanded to read:
   "It is important to note that we present an example for a single location in two different years. Different locations and even different years could have drastically different distributions of these snow phases. Sites with ephemeral snow pack may not have main accumulation or main ablation phases."

   Expanding the analysis, as suggested, is in the realm of a separate study of density timeseries related to physiographics. Such an analysis would really require some way of automating the detection of these phases, which could prove difficult.

6. Appendix B. page 5032, line 20. I would delete this sentence. Slight increase in RMSE on 31th day seems to happen by accident, and I don't think it is statistically significant. Furthermore, RMSE decrease again after 31th day.

   We have removed this.

Authors' Own Comments

1. Why is it better to use a regression model than to interpolate nearby SNOTEL in the same year?

   We added the following paragraph to"3.2 Experimental Design":
   "Because we use local data to train the models, one might question if interpolation of concurrent density observations is easier than regression modeling and equally accurate. Regression modeling has the advantage of using historical data and does not require concurrent observations. Also, at the 70km scale there may be important differences between snow depth timeseries at the locations where densities are observed and modeled. A regression model can account for such disparities in snow depth timeseries which should lead to more robust estimates."

2. Stats are likely poorer (than necessary) because they are considered over full season, including large errors related to extreme density variability at the beginning and ends of the seasons. Could limit calculation of stats using a depth threshold or limiting in time.

   We have now separately calculated the skill statistics (bias, RMSE, and R2) for the 3 week period centered on peak SWE for each station*year. These stats include 62324 observations and are included in Table 1 and the results. We justify presenting the two sets of statistics and the first paragraph of section 4.2 has been expanded to 3 paragraphs which describe the additional results, showing much stronger improvements for our model during this period.

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