Review of 'Modelling bulk density and snow water equivalent using daily snow depth observations'

Authors: J.L. McCreight and E.E. Small
Year: 2013
Publication: The Cryosphere Discussions
Review date: November 28, 2013

Overview

The manuscript presents a new statistical method for estimating daily snow densities from snow depth observations. As pointed out by the authors, snow density is a fundamental snowpack property that links snow depth to snow water equivalent – which is a valuable parameter for many hydrological applications. Methods for simulating snow density, particularly those that don't rely on vast observational datasets, are in demand. Overall, the manuscript is scientifically relevant, interesting and likely to be quite useful to the snow research community at large.

One of the major strengths of this manuscript is the recognition and consideration of snow density dynamics at two different timescales: daily and seasonal. In this study, daily variability in snow densities (from fresh snowfall, fresh snow compaction and melt percolation) are considered as anomalies to seasonal-scale density climatologies. Previous research in this field has typically focussed on either short-term densification (Chen et al., 2010) or seasonal densification (Bormann et al., 2013; Sturm et al., 2010) – and predominately on the latter. Interestingly, this manuscript presents a novel synthesis of these two previous research streams. As one of the main strengths of the manuscript, I think that the combination of these two streams of research could be explicitly highlighted in the introduction.

The vast quantity of observational data used in the study is another major strength of the manuscript, which broadens the applicability of the study across much of the United States and across many different climate types.

While there are some minor points that could be clarified to improve the coherence of the approach and the transparency of the new model, overall I find the manuscript to be clearly written, well structured, of an appropriate length and quite well presented.

Minor comments

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.

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.

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.

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.

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.

Line comments

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

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

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.

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.

Page 5021, Line 22: should 'systematic errors in both density SWE' read 'systematic errors in both density and SWE'?

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

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.

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'.

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.

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

References

Bormann, K. J., Westra, S., Evans, J. P. & McCabe, M. F. (2013) Spatial and temporal variability in seasonal snow density. *J. Hydrol.* **484**, 63–73. doi:10.1016/j.jhydrol.2013.01.032

Chen, X., Wei, W. & Liu, M. (2010) Change in fresh snow density in Tianshan Mountains, China. *Chin. Geogr. Sci.* **21**(1), 36–47. doi:10.1007/s11769-010-0434-0

Sturm, M., Taras, B., Liston, G., Derksen, C., Jonas, T. & Lea, J. (2010) Estimating snow water equivalent using snow depth data and climate classes. *J. Hydrometeorol.* **11**(6), 1380–1394.