The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-167-RC2, 2017

The Cryosphere Discussions

Interactive comment on "Estimating relationships between snow water equivalent, snow covered area, and topography to extend the Airborne Snow Observatory dataset" *by* Dominik Schneider et al.

Anonymous Referee #2

Received and published: 28 November 2017

General Comments

This paper uses observational snow data collected as part of the Airborne Snow Observatory (ASO) in the Tuolumne Basin, California over multiple years and at different times of the melt period to examine temporal persistence in the patterns of snow water equivalent (SWE) and their relationship with various terrain parameters. Multiple regression analysis is used to model the relationship between SWE and the other parameters as independent variables–not from the original high resolution ASO products (3 m horizontal resolution), but from coarser spatially aggregated (to 500 m resolution) representations of these data. The main reported contribution is in using fractional

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snow cover area (fSCA) information from the ASO dataset to improve the transferability of the regression models to other dates when the detailed ASO snow observations are lacking, with the premise being that other available fSCA products, such as from MODIS, could be used instead at these times.

While the purpose and rationale seem clear enough and there is value in using the exemplary dataset from ASO to develop better approaches for predicting SWE and snowcover patterns when detailed observations are not available, the study suffers from major conceptual and methodological flaws that greatly limit its practical usefulness. From a conceptual perspective, it makes little sense and it is not particularly useful to develop a set of different regression models for each of the ASO acquisition dates, and then to later determine which one to use based on some similarity criteria from snow pillow observations. It would be more useful, for example, to develop a single model or approach to characterize spatial patterns of SWE where observations such as SWE at the snow pillows and remotely sensed snow cover information are used in combination to inform this. Or more simply, why not just use the original ASO datasets themselves based on similarity with the snow pillow data? Of course the limits with this are clearly apparent. Given the empirical nature and lack of any incorporation of snow accumulation, redistribution, and ablation processes in the approach, the results are not likely to transfer well outside of this basin, and quite possibly not even to other years and seasons under altered climate conditions.

Even more concerning is the methodological approach. The ASO data provide an opportunity to explore patterns of SWE variability and their changes over time at a high level of detail, and yet the approach here has been to aggregate these data and lose that valuable information. The local scale patterns of SWE accumulation are no longer captured, which is the whole purpose of relating SWE to terrain parameters (i.e. the drifts form in sheltered locations while exposed sites are more wind scoured, and much of this variability occurs at scales from meters to 10s of meters in complex mountain terrain). Moreover, the terrain parameters that are derived from this aggregated DEM become physically meaningless. How useful is average vegetation height over a 500 m grid? What do vector ruggedness or topographic position index really mean at this scale, for example? Certainly nothing from a physical sense that relates to patterns of SWE accumulation due to drifting, wind scouring, trapping of blowing snow by exposed vegetation, etc., and especially when relating to the averaged SWE over a 500 m grid.

In addition to these concerns, the approach itself seems quite overcomplicated for what is essentially just an examination of the temporal persistence in SWE patterns over time, and ultimately an attempt to determine which regression model best fits the conditions of a given time based on a set of in situ SWE observations. The methods are not well described and hard to follow in some parts, leaving some major doubts. I will explain more in my specific comments below.

In the end, the study makes only a very incremental contribution to this important topic. While the ASO data provide unique opportunities to explore relationships between SWE, terrain, and snow cover patterns, and to advance understanding of the process and terrain controls in a way that could lead to improved predictability of SWE patterns, this study takes this in a different direction and instead focuses on coarser resolution regression analyses that largely miss this. Although the authors argue that this approach is novel and no one has previously used fSCA in such a regression, there is no major theoretical advancement, no practical utility as a result of the flawed methodology, and the results are severely limited in their broader applicability elsewhere. I would therefore recommend this paper to be rejected.

Specific Comments

Page 5, 6 - Data Sources: The ASO data includes a 3 m snow-free DEM, 3m snow depth, 3 m vegetation height, and 50 m SWE information. From this it would be straightforward to estimate 3 m SWE, and this is the appropriate scale to work at in deriving relationships between snow cover, SWE, and terrain. It makes little or no sense to aggregate to 500 m. This greatly affects the subsequently derived terrain parameters and alters the SWE distribution through averaging. The rationale in this study is to move to a scale where fSCA can be used as another variable in the analysis, but this is done at the cost of the detailed information on SWE patterns, which is key for such an analysis.

Page 7, lines 173-174: The “PHV” model is a multiple regression that uses only physiographic variables as independent variables. This is fine, but to use such an approach in a predictive sense would require other information to adjust the results for a particular time (i.e. SWE observations at snow pillows, other in situ data, etc.). What is instead done here is to develop a whole suite of regression models for the different ASO dates and cross-compare against the observations on other dates to pick which fits best.

Page 7, lines 178-179: If you are masking the regression estimates to the ASO observed snow cover areas, are you not greatly influencing the results and the reported performance of the model? Shouldn’t areas without snow cover be included? This is not clear. And what about when the model is to be used in a predictive sense when there are no ASO observations?

Page 8, lines 281-222: To pick which model dates exhibits the greatest similarity with the transfer date, the mean error in SWE at snow pillows is used. As currently described in the paper, this is unclear. Is this error between the model predicted SWE (for 500 m grid squares) and the snow pillows? How would this be later used in a predictive sense? As I understand, to find the selected model, you need to compare the results of all of them against the snow pillow observations.

Page 15, lines 358-359: the text refers to selected models being shown as open circles in Fig. 6. There are no open circles in Fig. 6.

Page 18, lines 432-434: The selected model is a simple linear regression to be applied in real time. How? As noted above, this is not clear. Do you need to simply look at all of the models and see which fits best to the SWE at the snow pillows, and then choose that one to predict SWE patterns? How exactly is this helpful at all and how does it advance the science? For instance, if this is indeed the case, I would argue

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that it would be far better to look at the original ASO data, with its fine scale detail and high accuracy, and simply compare that against the snow pillow observations for a given date. If all that is gained is to use fSCA as another parameter in the model for prediction, what about the point the authors make on page 20, lines 499-501 that there are anomalous SWE and fSCA distributions that limit the usefulness of using fSCA as an indicator for model selection? How are predictions to be made with any confidence in circumstances where the observed SWE (and perhaps fSCA) differs greatly from the previous conditions observed during ASO campaigns, and thus how can these results extend the ASO record as suggested? This is a fundamental weakness of the study.

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