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Discussions

Interactive comment on "Factors influencing spring and summer areal snow ablation and snowcover depletion in alpine terrain: detailed measurements from the Canadian Rockies" *by* Michael Schirmer and John W. Pomeroy

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This paper examines whether uniform melt assumption applied to depletion curves is reasonable for a site in northern Canada. It takes a bit of reading to figure that out, but that is the essential scientific contribution being addressed.

Unfortunately, 1) it is not framed in the context of other related work showing how replication can be used well to advance the science in this particular area, and 2) there are a few questions about the statistical and sampling procedures that require

addressing. These problems could be addressed with some effort.

The most important issue is that the paper does not make a strong or compelling argument for its primary purpose or the need to replicate earlier experiments. It could be written more efficiently so that the primary scientific contribution was more prominent and readily apparent. The purpose is described in the paper as "determine factors which influence areal snow ablation patterns in alpine terrain," which is a bit vague and overarching, and the paper does not fully accomplish that task. The abstract and introduction spend most of their opening lines on the general subject of heterogeneity in snow without narrowing down to the specific issue addressed in this paper. The paper eventually goes into some depth in the introduction about depletion curves and relative contributions of melt versus accumulation variability. This is a good subject and an important subject in this field. As the authors note in P4L2-5 this is still a debate for the modeling community. An important question for the authors is why one would raise this question on Page 4 and not Page 1. Upon raising the question then, it is important to bring to bear the various answers and measurements contributing to that uncertainty already in the literature.

If better framed, the introduction should also address the need for replication of experiments on this subject in multiple places. The primary problem here is that the background material presented is by-and-large based on citations of their work or that of close colleagues. This is maybe fine for a general discourse or more obviously unique contribution. However, if one needs to make a case that more replication is needed on a subject, one needs to make a specific effort to find as much of the related literature as can be reasonably applied and explain why this particular replication is useful.

I'll pick on one citation that is already used for a different subject (general heterogeneity), but which has a nearly identical conclusion as this paper, Luce et al. 1998. We stated several times and in various ways:

"This result implies that spatial variability in snow drifting has a greater effect on the behaviour of Upper Sheep Creek than spatial variability in solar radiation and temperature."

It would be great to discuss this and the four related papers also giving similar findings on P3L29-31 in more detail and explain why measurements in more places are useful to answer the questions brought up 3 lines later. Without some explanation (e.g. that these conclusions were derived based on only 4 sites) the lines saying that the answers are unclear following four (now five) articles that agree with each other seems almost contradictory. There is some text in the preceding page-long paragraph that describe some differences in findings, but again one has to tease out that apparently one set of findings is from forests and one from windswept areas.

I think it quite reasonable to summarize from the antecedent papers that the relative dominance of accumulation versus melt processes varies from place to place, and that adding information about another location to that list, particularly with some more detailed physical insights, could be useful. Certainly, one could bring up that there might be more value in a synthesis (e.g. along the lines of Clark et al., 2011) when trying to sort through that problem, but that requires many sites to have been sampled.

Page 19 Lines 1-4 present the key problem needing to be addressed. One would hope that the paper advances the theory and process understanding necessary to solve this problem rather than simply presenting one more example, however. It looks like there is capacity to do so with these data, but I'm not entirely certain. A well written introduction could probably narrow the subject enough that one could ask whether the finding that accumulation distributions are more important than melt distributions is a general finding for windswept sites with primarily low vegetation, or whether there are other contextual variables or information that would alter that simple generality? Alternatively, is there capacity to explore processes or causes for the lack of correlation that might otherwise be expected? For example, is the cause of low correlation a result of 1) the high sun angles during the melt, 2) dust deposition mirroring snow deposition (e.g. a process likely to cause a positive correlation between melt and accumulation

anomalies), or 3) substantially greater variability in accumulation than in melt as might be predicted from an area dominated by low slope angles and southerly and windward aspect?

At least some degree of coherent synthesis is necessary to support the addition of another paper on this subject that shows results similar to others. The heavy reliance on one or two heritages for many of the citations throughout the paper hobbles it considerably. Many of the papers cited in Clark et al., 2011 have information relevant to the discussion in this paper, and there are a number of others. There is also a need to become better acquainted with the literature. Some papers are cited for one thing when they are more relevant for another argument, or even several throughout the paper. There are also several citations in the paper (of the authors own work) that provide relatively poor support of their sentence compared to other well-known work.

Additional Points:

With respect to the analysis of correlation between HS0 and dHS, only Pearson (linear) correlation is tabulated. It would be useful to see the plots and better understand the causes for the apparent lack of correlation.

There is a great deal that should be explained about the potential effects of the sampling on the results. On P8 L17-19: In addition, one can note that the ESE wind direction is subparallel to the main ridge line. Would this have anything to do with the results? Also from Figure 2, most of the area does not look to be particularly steep, and it is by and large south facing. These do not seem like circumstances that would be likely to produce substantial variance in melt. Furthermore, most of the winds are from the south-ish, implying an expectation of relatively more scour on much of the area with only a few subdrainage/subridges causing enhanced deposition (Figure 3c) with only a little participation by the main ridge, and there mostly with slightly south facing (?) areas. And in Figure 3d, only a few areas are really analyzed. Given that areas with shallow snow tend to have more vegetation poking through (northern part of

3d), it seems like a lot of the locations with shallower initial snow are excluded from the analysis, and it is hard to sort through the impacts of that choice in finding a correlation between initial snow depth and melt rate.

On P7 L22-24: Stepwise regression is a notoriously poor method for model selection. See Burnham and Anderson (2002), for example. I would not be surprised to see similar results from a more formal model selection procedure, but it seems important to use our best understanding when applying statistics.

P2 Lines 8-10 appear to contradict lines 10-12. Lines 10-12 apply only to the special case where wind deposition occurs on multiple aspects.

P19 L5-9: I would like these authors (and, to be fair, a large number of other authors) to comment on how more time series in one place help us to transfer models to other places. This seems to be a fundamental underpinning of modern hydrological science as it is practiced, and I have not been presented with much in the way of evidence to support it.

If any further details can help clarify any of these points, please feel free to contact me. Sincerely, Charles Luce

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Burnham, K. P., and Anderson, D. R.: Model Selection and Multimodel Inference, Second ed., Springer, New York, 488 pp., 2002.

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