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Interactive comment

Interactive comment on "Forest impacts on snow accumulation and ablation across an elevation gradient in a temperate montane environment" *by* Travis R. Roth and Anne W. Nolin

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Reviewer #2 comments:

1) "Link energy budget to snowpack observations: The authors' present two very interesting datasets, namely snow physical properties and micrometeorology/energy budget. However not enough effort is made to explain snowpack characteristics with energy budget." a. "Do you see correspondence between melt events and energy inputs, both in terms of seasonal and episodic melt? This is difficult to assess because annual snow data is not presented."

Author response: We regret not including annual snow data into the paper as the inclu-

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sion of these data would provide necessary clarity to the corresponding link between energy budget calculations and measured snow accumulation/melt. We do see temporal links between episodic events and calculated energy budget magnitudes. The revision of this paper will have an updated figure that both includes daily snow depth dynamics and daily energy budget component magnitudes. We are confident that the new figures will address this comment and comment #3 sufficiently.

b. “What about correspondence between melt rates and energy estimates?”

Author response: Comment 1a and 1b are linked and we will address these both by including new, clearer energy budget figures. Additionally, within the results section we will provide clarity as to the level of correspondence between melt rates and energy estimates.

c. “Can you leverage the different climate (and in particular temperature) to talk about sensitivity of different sites to temperature?”

Author response: We will discuss this in our response to comment #2 (below) as this sub-comment pertains to our response directly.

2) “Lacking a main take home about “sensitivity”: While I think the above comments will help draw out more implications from the results, I would like to see the authors go further in describing the larger implications for ‘sensitivity’ to drought and warming across these elevations. While the paper’s conclusions focus on differences between open and forest canopy, they do not effectively make the case for how the underlying elevation gradient modulate those effects and their corresponding scheme. A lay might be some similar to the Nolin and Daly, 2006 classification scheme. I think that the authors should consider how to use the inter-annual variability to explain sensitivity. Consider leaving in 2015 or using it as an example vis a vis the Nolin and Daly classification. How do these differences in snow-vegetation interactions overlay on snow risk for change? What forest position are more likely to see exaggeration of current open/forest snowpack differences? Which are more buffered and why?

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Author response: The question of the sensitivity of the snowpack to changes in temperature is extremely important for this study region and we welcome the chance to discuss this here. Nolin and Daly (2006) demonstrated that much of the Oregon Cascade snowpack is at-risk, the ForEST network sites included, by looking at temperature only. Similarly, Sproles et al., 2012 showed that the lower boundary of the snow zone has little resilience to a warming world. Both show that there is essentially no resiliency to warming temperatures. Our paper goes further and shows that the understanding the energy budget of the snow surface is most important because we look at the influences of vegetation on snowpack. By showing the mechanisms of how vegetation effects the sub-canopy snowpack energy balance this study provides the basis for truly understanding the sensitivity these temperate forest snowpacks have to warming temperatures. As the climate warms, not only will the frequency of precipitation falling as rain increase longwave radiation will amplify melt as the forest warms. The forests will exacerbate melt where we see dense mature forests. Yet the higher, cooler site with a less dense forest can mitigate that to some extent by retaining the snowpack longer through lower relative forest longwave emission and lower canopy interception. As to the comment about inter-annual variability, the network experienced high inter-annual variability and a key finding that was that throughout a definitive pattern emerged within the energy budget and snowpack dynamics across the network. Sproles et al., (2017) shows that WY2014 can be used as an analog for “average” winter snowpacks in a warming climate whereas the extremely warm WY2015 will continue to be considered a low probability winter in the future. The energy budget format that we present here goes beyond the temperature only approach while getting at the casual effects and mechanisms of the challenge of vegetation for a warming climate.

3) “Better show data in figures/tables: The energy budget time series are useful but difficult to compare. It might be possible to summarize all the sites into a single barplot figure using monthly means. I also think you should show the continuous snow depth time series either as a separate figure or overlayed onto Figure 2. You might consider breaking out Figure 2 by year (see comment 1). Same for Figure 5.”

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Author response: We intend to revamp these two figures to better depict the response of snow to energy budget calculations (see response #1).

4) "Better explain choice/sensitivity to LW algorithm: I would like to see the authors do a better job explaining the longwave radiation models and results to the reader. Can you run a sensitivity analysis with the top 2-3 best models to see if it matters much for your results about the most important heat source being LW. I think making the case a little more strongly for LW will benefit the paper because this is a strong and important finding. Along these lines, add more comparison to previous energy budget and longwave calculations. I would like to know how your net longwave radiation compares to previous measurements in maritime conditions (they seem very high)."

Author response: Understanding that our longwave radiation result was a strong and important finding we share this reviewer's concern as to the calculation of longwave radiation and the sensitivity to its formulation and given the infrequency of cloud-free days, the cloud correction algorithm. With that in mind we ran a comparative analysis on a multitude of longwave calculation formulations much in the same as Flerchinger et al., (2009) and reported the root-mean squared errors of each algorithm [Table S2 and Table S3]. A full description of the study is given [pg. 7, lines 20-32]. Additionally, the authors agree in the value of the reviewer's suggestion as to how our longwave calculations findings compare to previous measurements in maritime conditions. Addition of this will benefit the paper and provide necessary context.

5) "Unclear how equation 1 is calculated: It is unclear what time step that interception efficiency is calculated, as the text prior seems to refer to the daily efficiency when snowfall is >3 cm. Figure 3 shows it as a per event ratio. You need to be clear how this is calculated (i.e. Figure 3 does not seem to match equation 1). I like the per event basis."

Author response: Eq. 1 is calculated on a per event basis. This ambiguity will be clarified in the text to reflect both the per event basis of Eq. 1 and how an event is

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constrained.

Minor comments: 1) “How do estimates of latent heat compare with typical sublimation estimates?”

Author response: We did not make any comparisons with other published sublimation results and will include some discussion to address this comment. Adding some comparative analysis will give the reader a better understanding of the underlying processes of our study area.

2) “Add y-axis labels to figure 4.”

Author response: We will fix this minor issue with Figure 4.

3) “May be I missed it but, why did high elevations not intercept snow (e.g. Figure 3)? This is an interesting finding.”

Author response: We suggest that the low interception capacities at the High-Forest site is based on canopy density and note that this removal inefficiency is a first order process on seasonal snow accumulation [pg. 12, lines: 23-24].

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