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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

Anonymous Referee #2

Received and published: 16 January 2017

I reviewed the paper "Forest impacts on snow accumulation and ablation across an elevation gradient in a temperate montane environment" by Roth and Nolin for publication in HESS. I found the paper to be a well written explanation of a novel and robust dataset. My comments for major revisions focuses on better linking accumulation and ablation processes with energy budget estimates. Overall, some of the discussion needs to explain to the reader the cumulative importance of the results, particularly in terms of warming temperature and other climatic variability. I outline these as several major and minor comments:

MAJOR COMMENTS:

1. Link energy budget to snowpack observations: The authors' present two very inter-

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esting datasets, namely snow physical properties and micrometerology/energy budget. However, not enough effort is made to explain snowpack characteristics with energy budget. Some suggestions are below (some of these are relevant to comment 2 and 3).

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 (see comment 3).

b. What about 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? See comment 2.

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 modulates those effects and their corresponding ’sensitivity’. A laymight 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? 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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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). Here are a coiouple of relevant citations.

- Lapo, K. E., L. M. Hinkelman, E. Sumargo, M. Hughes, and J. D. Lundquist (2017), A critical evaluation of modeled solar irradiance over California for hydrologic and land surface modeling, Journal of Geophysical Research: Atmospheres, n/a–n/a, doi:10.1002/2016JD025527.

- Sicart, J. E., J. W. Pomeroy, R. L. H. Essery, and D. Bewley (2006), Incoming long-wave radiation to melting snow: observations, sensitivity and estimation in Northern environments, Hydrological Processes, 20(17), 3697–3708, doi:10.1002/hyp.6383.

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.

MINOR COMMENTS:

1. How do estimates of latent heat compare with typical sublimation estimates

2. Add y-axis labels to 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.

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