Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1
https://doi.org/10.5194/hess-2022-273-RC1, 2022


# Comment on hess-2022-273

Jessica Lundquist (Referee)

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Referee comment on "Canopy structure, topography, and weather are equally important drivers of small-scale snow cover dynamics in sub-alpine forests" by Giulia Mazzotti et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-273-RC1, 2022

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Overview: Mazzotti et al. explore the interplay between topographic shading, forest cover, and three-years of varying meteorology on how snow evolves near Davos, Switzerland. The paper is well-written and contributes to understanding of how and why forest-snow process vary between years and different locations. I recommend publication following minor revisions. These fall in two categories. The first, listed under major comments, involves putting the results in a larger context. The second, listed under minor comments, involves improving clarity (by rewriting very long and complicated sentences). The authors are welcome to reach out to me if they have any questions about any of my comments: Jessica Lundquist, University of Washington, jdlund@uw.edu

**Major comments**:

- This is a very thorough paper with a lot of years of analysis. Given the large focus on different components of the energy balance, I recommend that you comment on your relative confidence in your spatial calculations of SW, LW, turbulent fluxes. You keep saying sensible heat flux is a dominant term, but I think you are less confident about that than about your radiation calculations.
- How transferable are your results to other regions of the globe? Further south, further north, or warmer or colder? How transferable to other modeling work? The following comments indicate some thoughts on how to more broadly interpret your work here.

The following are details related to overarching question number 2:

* Obviously, I'm biased by being me, but I'd be really curious to see you put your results in context of Figures 6, 7, and 8 from Lundquist et al. 2013 (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/wrcr.20504 ) . I was trying to get at what you've done here with a much simpler model, and I'm curious how much of your results confirm what the simpler model shows, and how much your results show that a more complicated model really is needed. (Motivation: we have a lot of forest and water managers in the U.S. who would like to use simpler models if possible — what are the trade-offs.)

* You say that you have to consider all three factors (canopy structure, topography, and meteorology), but could you consider topography as a local modification to meteorology? At your study site, does it do anything other than alter your solar exposure? For example, you state that Currier et al. 2022 could be further refined by accounting for topography, but the DHSVM model used in that paper does adjust all of the local meteorological inputs based on topography (including shading by surrounding slopes). (I'm less familiar with Broxton's paper, but I'm pretty sure the topographic effects are included in Currier et al. 2022.)

* You definitely don't need to add another citation to your paper, but you might be interested to look at Lundquist and Flint 2006 (https://journals.ametsoc.org/view/journals/hydr/7/6/jhm539_1.xml ) which talks about topographic shading being more important when melt occurs earlier in the year. Look at Figures 12-15, which talk about the interplay of warming, latitude, and terrain on net radiation. Your multi-year analysis takes this basic concept and expands to the impact of forest cover. I think that looking at these conceptual figures and discussion might help you put your results in a larger context.

* I'd also like to see a bit of discussion about the scale at which topography would become more important than canopy structure in controlling accumulation. For example, if you have a north-facing slope in a rain shadow, and an adjacent south-facing slope that experienced orographic enhancement (or vice versa). You have a unique situation in that your slopes experience the same precipitation, but differences have been observed even at very fine scales (e.g., see Minder et al. 2008: https://www.atmos.albany.edu/facstaff/jminder/research/minder_et_al_cases_published.pdf and Anders et al. 2007).

**Minor comments**:

There a large number of very long and complicated sentences. Some shortening for clarity would be helpful.

For example, this line around line 40 is super awkward to read "In view of ongoing changes in both, snow cover regimes due to increasing temperatures (Mote et al., 2018; Marty et al., 2017; Notarnicola, 2020; Bormann et al., 2018), and forest structure following manmade and natural disturbances (Bebi et al., 2017; Seidl et al., 2017; Goeking and Tarboton, 2020), it is also urgent and pertinent to adequate forest and water resources management strategies - particularly in regions where downstream water supply is dependent on snow resources from forested headwaters"

Not sure what you mean by this sentence: "However, it remains unclear whether landscape heterogeneity entails a variable response of snow cover dynamics to environmental change." - line 42

I really like Figure 3.

Line 247: Regarding higher peak SWE not implying longer snow duration — note that these two are strongly linked across most of the western U.S., but these are also in snow packs with greater than 1 meter of SWE (and hence longer total melt out periods). With regards to greater context above, it might be nice to add some discussion to how different regions may or may not see similar results to what you show here.

Additional sentences that would benefit from clarification are noted on the attached annotated PDF.

Please also note the supplement to this comment:
https://hess.copernicus.org/preprints/hess-2022-273/hess-2022-273-RC1-supplement.pdf