The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-123-RC2, 2017

Interactive comment on "Snow melt response to simulated warming across a large elevation gradient, southern Sierra Nevada, California" *by* Keith N. Musselman et al.

B. Henn (Referee)

bhenn@ucsd.edu

Received and published: 8 September 2017

General Comments

This study examines the effect of projected climate warming on snow accumulation and melt rates along an elevation gradient and between wet and dry years. The authors use detailed snow and meteorology observations from Sequoia National Park to validate and drive physically based snow model simulations of historic and projected snowpack under warming. The simulations show that historic conditions are reproduced for three years of data with acceptable agreement to observations. They then show sensitivity of snow volumes and melt rates with elevation and wet/dry years, finding that snow volumes decline with warming at about 10%/degC overall with greater losses in the mid elevation coniferous forests, and that melt rates decline in areas of snow loss as melt now occurs earlier in the year. They also argue that extreme high melt rates increase under warming based on one of the three simulated years in which mid-winter melt events occurred more extensively.

The study reinforces previous work that shifts from snow to rain under warming will result in lower snowpacks in mid-elevation areas, and that the lower, more ephemeral snowpacks will melt more slowly and earlier in the winter. The elevation gradient and three years of varying precipitation are helpful in visualizing those effects across those important dimensions of the mountain hydroclimate, making the study a useful contribution. The authors' claim that extreme melt rates (defined as high-quantile rates under the historic scenario) may increase under warming is novel and has flood risk implications. This claim may need to be phrased more carefully as the majority of the results presented show melt rates decreasing under almost all warming scenarios, and it also seems possible that the increases in melt rates reported are sensitive to the assumptions of the warming perturbations in this particular study. The authors should perhaps more clearly state some of these caveats.

The paper is well written and clearly organized and has high-quality figures.

Specific Comments

L254-262: The section on how longwave radiation was calculated under the perturbation scenarios is clear in terms of how it was done, but would benefit from more conceptual explanation about why this method is appropriate. For example, this method assumes that both RH and emissivity of the atmosphere do not change under the warming scenarios. While there is conceptual evidence to support (fairly) similar RH in a warmer climate, emissivity has a dependence on temperature (Flerchinger, G. N., W. Xaio, D. Marks, T. J. Sauer, and Q. Yu (2009), Water Resour. Res., doi:10.1029/2008WR007394.) Given that the authors cite midwinter melt rates as a key finding later in the study, and longwave has been implicated in driving midwinter melt (Lundquist, J. D., Dickerson-Lange, S. E., Lutz, J. a., & Cristea, N. C. (2013). Water Resources Research, http://doi.org/10.1002/wrcr.20504), justifying the perturbation assumptions around longwave and turbulent energy fluxes might benefit the paper.

L295-297: The explanation of the quantile analysis of melt rates could perhaps be better. If I am understanding correctly, the 99th percentile (for example) melt rates (over the whole spatial domain and year?) are calculated for the nominal case, and then this is repeated for the warming cases and the melt rates are compared?

L376-389: Somewhere in the paper, discussion about why drier years were more susceptible snowpack loss to warming than wetter years might be warranted. Given that precipitation is fixed, were snow accumulations reduced more in the dry year because those storms were warmer (rain-snow level closer to mean domain elevation) than in wet years? i.e., is this a general finding or something specific to the storms in those years?

L405-408: Perhaps I am misunderstanding Figure 9, but the statement that

"Extreme melt rates (99th percentiles; downward-facing triangles in Fig. 9) actually increase (inferred from markers plotting above the 1:1 line) at elevations > 2800 m asl in all years (top panels) and in the drier year at all elevations (left panels)"

doesn't quite seem to follow the data - most of the warming scenarios showed did not show increasing 99th percentile rates for the dry year at low elevations, nor did several of the scenarios for each year the high elevation zone. It is interesting that some extreme melt rates did increase, but focusing only on those scenarios that increased might overstate the robustness of this finding.

L425: The discussion section could benefit from better organization and potentially using subsections to divide it among topics. The ordering of this section (discussing results, then flood and soil moisture implications, then caveats and other issues) seemed a bit meandering for me as a reader (implications for streamflow and soil moisture, processes which are not tested in this paper, seem like they should go last, for example).

L545: The mechanisms behind the reduction in snowpack with warming seem like they deserve greater discussion here. If I am understanding the experiment, there are only two mechanisms by which warming reduced meltwater volumes: precipitation falling as rain instead of snow, and increased sublimation. How important are these relative to each other in reducing meltwater volumes? Is increased sublimation significant at all or is it entirely the shift to rain? Some discussion of these mechanisms behind the results might be helpful to placing them in physical context.

Technical Corrections

Figure 1: Having repeated numbering of different types of stations is confusing - consider different ways of numbering sites.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2017-123, 2017.