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

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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.

Thank you for the thoughtful and supportive review. We address your comments and suggestions below.

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.

We agree that this assumption should be stated and implications discussed. It is important to note that the air temperature influence on LW is by far the most dominant component of the Stefan-Boltzmann equation (effective air temperature is to the power of 4).

We have added the following sentence to the section where we first introduce the longwave radiation perturbation. Lines 265-266: "The *in-situ* atmospheric emissivity is assumed to be constant for the perturbed temperature scenarios."

We also discuss the implications of our assumption in the Discussion section where we discuss sources of uncertainty, and conclude that our results are somewhat conservative (Lines 557-560):

"Furthermore, by not perturbing the measured atmospheric emissivity used in the warmer scenarios, we may underestimate the longwave contribution to snowmelt. Atmospheric emissivity varies as a function of column-integrated temperature, specific humidity, and cloud structure above a site [Flerchinger et al., 2009]."

Thank you for the suggestion of the Flerchinger et al., (2009) citation.

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?

We now better describe this analysis with the following sentences on Lines 299 – 303:

"For this analysis, the model domain was divided into three elevation bands: 1500 to 2250 m asl, 2250 to 2800 m asl, and >2800 m asl, and percentiles of daily snowmelt were computed for all grid cells in each elevation band. The analysis was conducted separately for each of the three water years and seven scenarios."

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?

To clarify, we don't characterize the dry year as being particularly sensitive to warming. Rather, the wetter year is more resilient to warming than the other two years (an average and a moderately drier year). We explain this difference by the wetter year having substantially lower seasonal (AMJ) average air temperature; however, we do not explicitly analyze synoptic storm temperatures. While not shown, the series of spring storm events in 2010 (wetter year) were unseasonably cold. As simulated, these storms in spring 2010 brought substantial snowfall to low elevations even in the warmest scenario. Thus the phase change was resilient to simulated warming.

We provide a brief discussion of this on Lines 444-446: "The year with the most snowfall, characterized by late snowfall events and cold spring (AMJ) air temperatures, was slightly more resilient (-9.3% $°C^{-1}$) to warming than the drier or average snow years." These seasonal air temperature metrics are provided in Table 3.

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.

Thank you for pointing this out. This sentence has been reworded to clarify that we refer to "a majority of the simulations". In the drier year, the Reviewer is correct that only the top two elevation bands (>2250 m) had a majority of simulations in which the 99^{th} percentile values were above the 1:1 line. Lines 412-415 (changes in **bold**):

"**For a majority of the simulations,** extreme melt rates (99^{th} 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 elevations **>2250 m asl**."

This is now an accurate statement supported by the data.

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).

We agree that the Discussion would benefit from reorganization. We have taken the Reviewer's suggestion to divide the Discussion Section into three sub-sections:

4.1. Snowmelt response to simulated warming

4.2. Hydrologic Implications

4.3. Sources of uncertainty and caveats

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.

We now clarify this point on Lines 576-580 (changes in **bold**):

"**The simulated reductions in snowmelt volume due to increased sublimation are very small compared to reductions caused by the warming induced shift from snow to rain. However,** by not considering blowing snow and subsequent sublimation losses (i.e., overestimating alpine snowpack), we may further underestimate snowpack sensitivity to warming."

Technical Corrections

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

Table 1 and Figure 1 have been updated such that each of the 29 stations / snow courses has a unique identifying number.