We thank Dr. Conway for their insightful review. Our responses can be found in blue throughout the following text. Please note, tables and figures specific to this response document are given with the prefix R (for example, Table R1 in the comment below). Tables and figures in the manuscript are referred to by numbers only.

Review of paper hess-2019-82 "**The sensitivity of modeled snow accumulation and melt to precipitation phase methods across a climatic gradient"** by Keith S. Jennings and Noah P. Molotch

Jono Conway

This paper presents a systematic evaluation of the impact of precipitation phase partitioning on modelled snowfall and snowpack evolution. Multi-year datasets from 11 stations across 5 locations in the western United States are used to drive simulations with a sophisticated snowpack physics model. The effect of parameter and algorithm choice are assessed for a range of commonly used parameterisations. The authors relate the modelled sensitivity to average climate characteristics. Snowfall and maximum accumulated snow in warmer maritime locations with high precipitation and winter temperatures between 0 and 4 degrees Celsius are found to be most sensitive to precipitation partitioning, while snowfall in colder inland locations are found to be less sensitive.

The manuscript is well written and with good figures and a clear systematic structure. It addresses a topic of high interest and relevance internationally. However, there are some areas that should be addressed before the paper could be accepted for publication.

Major comments

While the paper is framed as a comparison of methods used to partition precipitation, the results mainly reflect the range of Ta thresholds used (0 to 3 C) rather than the choice of parameterisation. This is in part due to the use the range metric on results that are generally are bounded by the two extreme Ta thresholds. The abstract and conclusions should reflect this (i.e. being explicit about choice of parameter values and/or parameterisation rather than using the ambiguous term "method". If the authors wish to make general statements, then using "precipitation partitioning" would be more appropriate. It is well established the Ta alone is a poor predictor of precipitation phase, so to really compare methods, those that perform poorly against observed SWE (e.g. Ta0 and Ta3) should be removed from the analysis. This would highlight the differences induced by using different parameterisations that have a sound physical basis. If the Ta0 and Ta3 options are to be retained, then further justification for their inclusion should be given in the methods section. The dependence of the results (especially the range of Ta with a large range in modelled snow) on the range of Ta thresholds used should also be discussed. Perhaps the use of a standard deviation or similar metric rather than a range metric would put the focus on the choice of parameterisation. Further specific comments address this issue.

We should note here that at YOS-DAN, NWT-C1, and NWT-SDL, the T_{d1} threshold produced greater annual snowfall fractions than T_{a3}. Thus, although T_{a3} produced higher snowfall fractions at the remaining sites, this effect was by no means universal. Additionally, as mentioned in the Introduction (p. 3 lines 17-18) and further highlighted in our response to Dr. Schaefli, a 3°C T_a threshold is appropriate in upland continental areas of the western US (e.g., NWT) where snowfall is more common at warmer temperatures than in other locales. Our concern is that many land surface and hydrologic models use spatially uniform air temperature thresholds to partition precipitation phase, so we argue that it is essential to incorporate thresholds that cover the range of observed rain-snow partitioning air temperatures for our study sites (1°C to 3°C). And, despite decades of evidence showing its inefficacy, the T_{a0} threshold and T_{ar0} range are still commonly employed to partition precipitation phase. For example, the widely used VIC macroscale hydrologic model assigns precipitation phase with a default -0.5°C to +0.5°C temperature range, centered on 0°C (https://github.com/UW-Hydro/VIC/; accessed 2019-05-20). We therefore included this method, if only to provide more evidence that it underpredicts snowfall, snow accumulation, and snow cover duration.

In order to further address the need to use the whole range of air temperature thresholds, we have added new text to the Methods (Sect. 3.2):

"T_a thresholds were chosen to represent the spatial variability of rain-snow partitioning in the western United States, where values of approximately 1°C are common near the Pacific Coast, increasing towards 3°C in the Rocky Mountains (Jennings et al., 2018). Additionally, despite significant literature showing its poor performance (e.g., Jennings et al., 2018; Marks et al., 2013), we included a 0°C T_a threshold in the analysis because it is still widely used in observational and model-based hydrologic studies."

In an early draft of this manuscript, we analyzed standard deviations in addition to the ranges presented in the submitted version. The story remained the same: warm maritime sites were greatly impacted by precipitation phase method choice, while cold sites were not. This is illustrated in Figure R1 below:

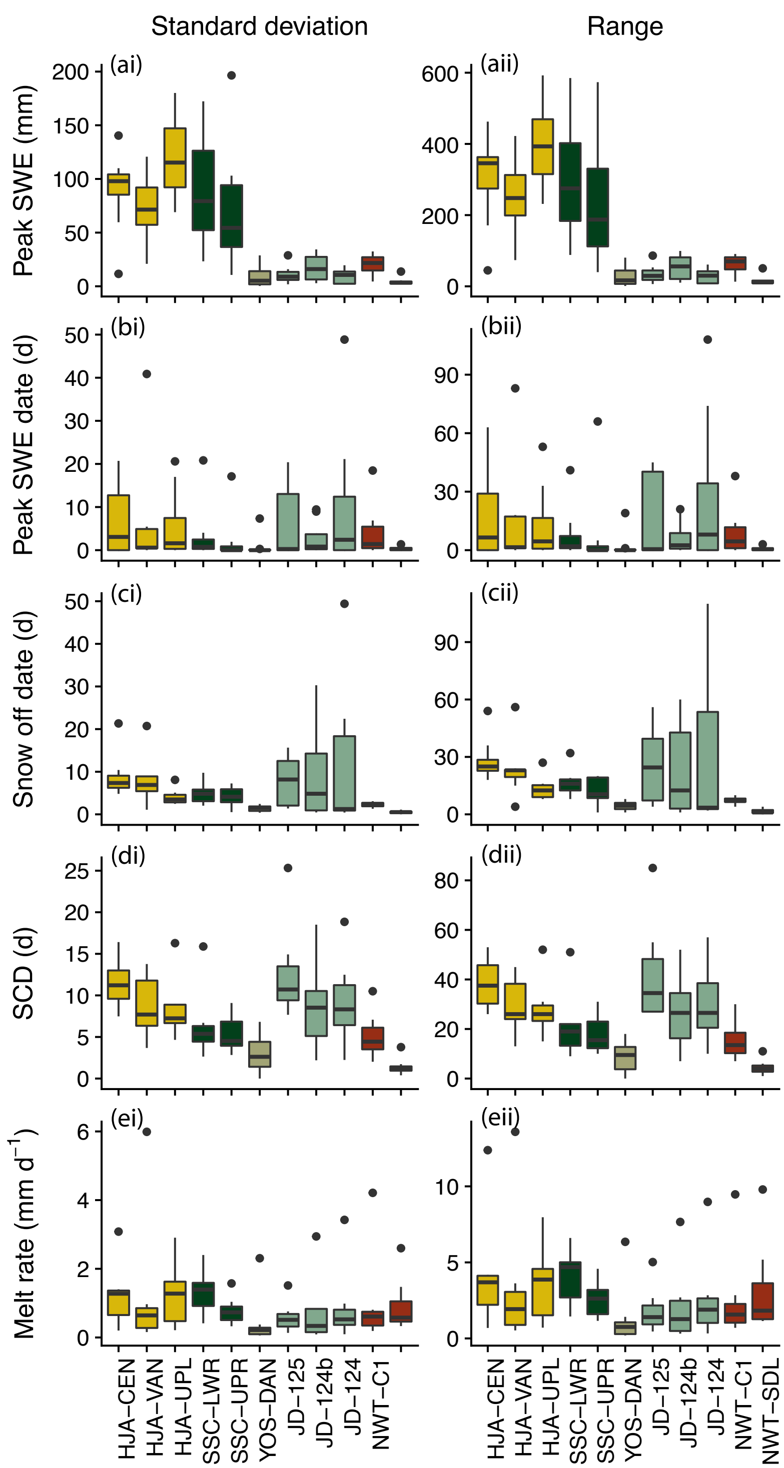

Figure R1. The annual standard deviation (left column) and range (right) in simulated peak SWE (ai,aii), peak SWE date (bi,bii), snow-off date (ci,cii), snow cover duration (di,dii), and melt rate (ei,eii) due to precipitation phase method selection at the study stations.

Regarding the semantics of "method" versus "precipitation partitioning" versus "parameterization," "precipitation phase method" is commonly used to describe modeling and empirical approaches to discriminating between rain and snow (e.g., Harder and Pomeroy, 2014; Harpold et al., 2017). We will leave as is.

Timing and magnitude of SWE ranges seem mainly related to snowfall and accumulation, whereas as range of melt rate does not have high sensitivity or clear relation to climate. This should be clearer in the abstract and conclusions.

We added to text to the abstract noting this finding:

"Average ranges in snowmelt rate were typically less than 4 mm d^{-1} *and exhibited little relationship to seasonal climate."*

And to the conclusion:

"In contrast to the marked differences in peak SWE, melt onset, and snow cover duration between the warm and cold stations, ranges in snowmelt rate exhibited little relationship to seasonal climate."

The abstract and conclusions need to highlight the novel aspects of the results presented here and provide clearer recommendations for future research. While the analysis is comprehensive, the result is not entirely new and, in my opinion, there are other results in the paper that could (and should) be highlighted in addition to the main result that the relative differences are largest in maritime snowpack. For example, the fact that using threshold or ranges for Ta (for the same 50% crossover) do not produce large differences in the snowpack, or that partitioning choice has little effect on snowmelt rate and the effects are dominated by snowfall. At present, the authors recommendations for future researchers are unclear.

For the ranges, we added text to Discussion Sect. 5.1:

"In the course of this work we found negligible differences between T_{a0} *and* T_{r0} *as well as between* T_{a1} *and* T_{r1} *in terms of annual snowfall fraction (Fig. 5) and model performance (Fig. 3). This suggests the ranges and the mixed-phase precipitation they produced provided little further information on precipitation phase at the hourly model time scale relative to the thresholds. However, it should be noted there is relatively little*

quantitative data on the frequency and solid-liquid proportions of mixed-phase events (e.g., Yuter et al., 2006). Work from the Torino region of Italy showed mixed-phase events are relatively few compared to all-rain and all-snow events (Avanzi et al., 2014), while research in a maritime climate indicated mixed-phase events can be quite frequent (Wayand et al., 2016). Thus, future work would benefit from further explorations of the frequency of mixed-phase events and model representations thereof at multiple time scales."

For snowmelt rate, it is not that the effect is small (Table 5 shows relative differences between 11.5% and 235.5%), it is that the metric showed no relationship to seasonal climate. Please see our response to the comment above for the extra material we added on snowmelt rate.

For novelty/implications, we changed the final line of the abstract to:

"This study shows care should be taken when selecting a precipitation phase method as the variability introduced to snow accumulation and melt will likely propagate into simulated streamflow and land surface albedo, particularly at the warmer fringes of the seasonal snow zone."

Regarding future directions, suggestions were given in the original manuscript (p. 8 lines 15-16, p. 20 lines 25-27, p. 21 lines 1-3, p. 22 lines 16-19, p. 23 lines 13-16). Given the multiple lines devoted to this topic and the further additions noted in this response, we find no further recommendations are needed.

The use of multiple linear regression is probably not appropriate here, but if retained should be presented and discussed more fully.

Please see our response to the specific comment below on this topic.

Specific comments (page-line)

1-15 please be clear the study modelled non-vegetated snowpacks only.

We have reconfigured the abstract to note these were point simulations with no canopy cover.

4-3 Given that they form a key part of the results, please include average values for Tw and Td in Table 1.

Added to Table 1

7-26 The large bias in LWin is concerning – perhaps the influence of vegetation on the measurements whereas LWin is modelled for non-vegetated location? This should be discussed when presenting the validation results in Figure A2.

We added more text expanding upon the bias in Sect. 3.1:

"*At the HJA stations, we bias-corrected the LW_{in} estimate based on one year of LW_{in} observations from HJA-VAN that showed a -56.9 W m^{-2} wintertime bias, which may have been related to site vegetation conditions. This was significantly larger in magnitude than the bias found in the Unsworth and Monteith (1975) estimate by Flerchinger et al. (2009), suggesting its performance is more spatially variable than previously noted. This finding also underscores the need for enhanced monitoring of the radiation budget at snow modeling sites (Lapo et al., 2015; Raleigh et al., 2015, 2016).*"

17-8 "80.1% of the variance in annual snowfall fraction standard deviation" – the figure caption and methods describes this as the "range in annual snowfall fraction" – please clarify which it is and correct.

Yes, good catch. We have changed the text to "*…annual snowfall fraction range*" to match the figure.

18-1 Figure 6 and 7 – given that the range in snowfall fraction is driven primarily by the two extreme air temperature threshold methods (Ta0 and Ta3) these results are presumably quite sensitive to the choice of the Ta thresholds? Please discuss and if possible show the sensitivity of the results to the choice of threshold.

Yes, Figure 6 is designed to illustrate the effect of threshold/method choice on daily snowfall fraction. The data presented are standard deviations, which minimizes the effect of the extreme T_a thresholds. However, we were curious how removing T_{a0}, T_{ar0}, and T_{a3} would affect the analysis and we found it made little difference (Figure R2 is nearly identical to Figure 6 in the submitted manuscript). Except for 1 outlier at -19.5% all differences in the standard deviations for the T_a and RH bins are between -7% and +5%, with a mean difference of -1.3% (computed by subtracting the SD for the analysis with all methods included from the analysis with T_{a0}, T_{ar0}, and T_{a3} removed).

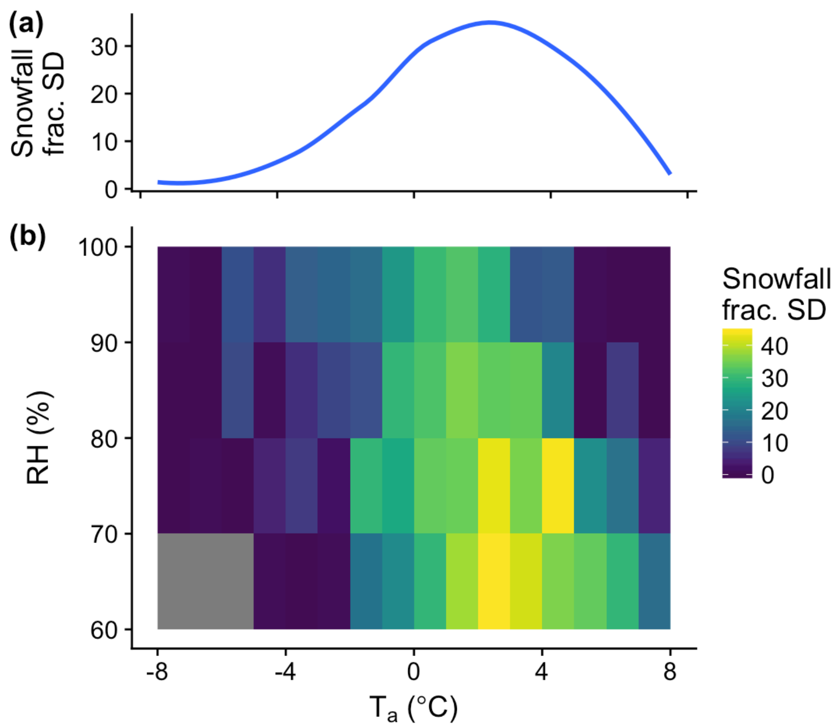

Figure R2. Same as Figure 6 in submitted manuscript but with T_{a0}, T_{ar0}, and T_{a3} removed from the analysis.

Given this analysis held up to the removal of the three least physically representative thresholds, we find the inclusions of Figures 6 and 7 along with the associated text to be appropriate.

18-6 Looking at the figure, it seems that a multiple linear regression may not be appropriate. There seems to be two groupings – highly sensitive warm and wet locations, less sensitive drier locations that span both warm and cold locations. Also, given that the equation is not presented nor used further, and the issues discussed with extrapolating the equations, please consider removing the regression. If it is retained, please present the equation and display contours of predicted values on Figure 8 so that the reader can visualise the predicted relationships.

We have decided use a loess function to create a smooth surface presented behind the station data (new Fig. 10 shown below). This, we believe, more clearly shows the clustering of low peak SWE ranges at the colder and/or low precipitation sites and high peak SWE ranges at the maritime sites without introducing the statistical pitfalls of multiple linear regression. We also edited the text to remove the linear regression statements.

"We next evaluated how sensitivity in peak SWE was related to seasonal climate. In this case, warmer T_a and increased PPT were both associated with greater ranges in the peak SWE simulated by the different precipitation phase methods (Fig. 10). This meant the maritime sites HJA and SSC had the greatest sensitivity to precipitation phase method due to their relatively warm T_a and high PPT values. Conversely, moderate PPT values and lower T_a led to minimal sensitivity at the cold continental NWT stations and the cold maritime YOS-DAN station. Again, the effect of T_a on sensitivity was manifest in the data. In high snowfall years at NWT-SDL, Dec–May PPT approached that of the low Dec–May PPT years at HJA and SSC. However, despite the increased PPT at NWT-SDL, the range in peak SWE predicted by the different precipitation phase methods remained low."

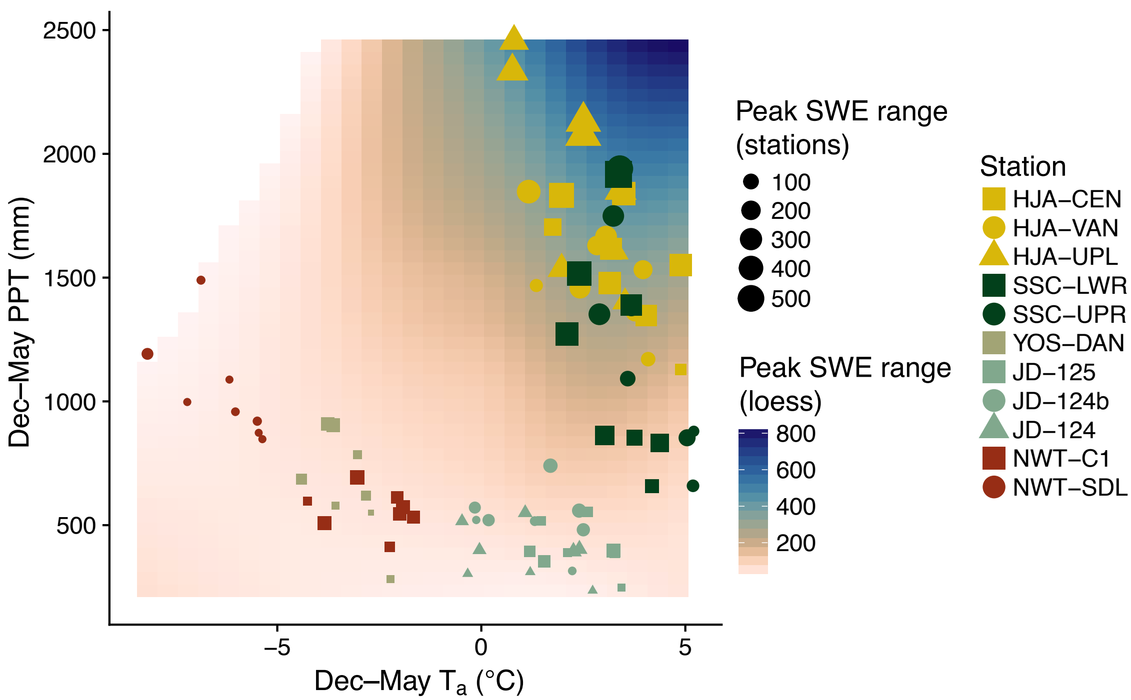

Figure 10. Range in annual peak SWE as simulated by the different precipitation phase methods at the 11 study stations. Each point represents one simulation year at a given station and larger points correspond to larger differences in maximum minus minimum peak SWE. The background shading corresponds to ranges in peak SWE predicted by a loess function fit to the station data. **[Please note, this figure has changed from 8 in the submitted manuscript to 10 in the revised version because we moved the validation figures from the appendix to the results as per the recommendation below.]**

19-10 The validation results presented in the appendix should be included in the results or methods section, especially as they form part of the discussion, rather than simply an intermediate methodological step.

We have moved this from the Appendix to be the first results section:

We have also added material in the Methods detailing how validation was performed (appended to the end of Sect. 3.1):

"To validate model output, we compared simulated SWE and snow depth to observations at our study stations. SWE was observed at all HJA stations, SSC-UPR, YOS-DAN, and both NWT stations, while snow depth was observed at all HJA stations, both SSC stations, and all JD stations. All SWE data were derived from automated snow pillow measurements except for NWT-SDL, which was acquired through manual snow pit observations (Williams, 2016). Similarly, automated ultrasonic snow depth sensors produced all snow depth data. Comparisons were made at the daily time scale when either simulated or observed SWE or snow depth were > 0 mm. This was done to prevent artificial enhancement of objective function values during periods when snow cover was absent."

We have also edited discussion Sect. 5.1 to reflect these changes and to incorporate feedback from the two subsequent comments.

"In this work we showed that the selection of a precipitation phase method produces varying degrees of variability in modeled snow accumulation and melt at our study stations. The different methods also expressed variable performance relative to observations of SWE and snow depth, with the binary regression models, Reg_{Bi} and Reg_{Tri}, as well as the T_{a1} threshold producing the lowest biases (Fig. 3). Previous observational work has shown that, in general, methods incorporating humidity information outperform T_a-only methods when it comes to predicting precipitation phase (Harder and Pomeroy, 2013; Jennings et al., 2018; Marks et al., 2013; Ye et al., 2013). The Reg_{Bi} method, which predicts phase as a function of T_a and RH, exceeded all other methods in partitioning rain and snow in a Northern Hemisphere precipitation phase method comparison (Jennings et al., 2018). Our study showed that Reg_{Bi} also typically produced simulations of SWE and snow depth that had low biases relative to observations (Fig. 3) and led to snow cover evolution metrics that were neither extremely high nor low compared to the other methods examined in this work. This finding is complemented by the performance of other humidity-based metrics, which produced average SWE and snow depth biases between -19.2 mm and 25.1 mm, and -64.1 mm and

45.0 mm, respectively.

This is in contrast to the T_a *thresholds and ranges, which produced the largest magnitude biases. Notably, the four worst performers were the* T_{a0}, T_{ar0}, T_{a2}, *and* T_{a3} *methods, with the former two underpredicting snow accumulation and the latter two overpredicting. Across our study sites, the only* T_a *methods that performed well relative to observations were the* T_{a1} *threshold and* T_{ar1} *range. These modeling results confirm again that including humidity information, whether it be in the form of a binary logistic regression model.* T_w, *or* T_d, *offers advantages over a* T_a*-only method. It is important to note again that we chose methods that covered the range in rain-snow partitioning* T_a *values across our study domain or that included humidity information. The only methods not falling into this category were* T_{a0} *and* T_{ar0}, *which were chosen because they are still employed as default methods in some models and studies. Although there are some small geographic regions where such a threshold or range may be appropriate (Jennings et al., 2018), they are unsuitable for many locations and should not be used for large-scale studies.*

In the course of this work we found negligible differences between T_{a0} *and* T_{r0} *as well as between* T_{a1} *and* T_{r1} *in terms of annual snowfall fraction (Fig. 5) and model performance (Fig. 3). This suggests the ranges and the mixed-phase precipitation they produced provided little further information on precipitation phase at the hourly model time scale relative to the thresholds. However, it should be noted there is relatively little quantitative data on the frequency and solid-liquid proportions of mixed-phase events (e.g., Yuter et al., 2006). Work from the Torino region of Italy showed mixed-phase events are relatively few compared to all-rain and all-snow events (Avanzi et al., 2014), while research in a maritime climate indicated mixed-phase events can be quite frequent (Wayand et al., 2016). Future work would therefore benefit from further explorations of the frequency of mixed-phase events and model representations thereof at multiple time scales.*

Despite the analyses presented in this work, it is important to note that uncertainties in forcing data, model structure and parameters, as well as a lack of precipitation phase observations prevent this research from being able to unequivocally identify a "best" precipitation phase method for snow modeling. However, as noted above, including humidity information improves the prediction of precipitation phase relative to observations and generally increases model performance. Our primary aim in this research was to quantify how snow simulations were affected by the choice of *precipitation phase method across a climatic gradient. We did not create optimized model setups at each site, but rather kept model setup consistent in order to compare the sensitivity of phase partitioning without introducing other uncertainties. Thus, the low r^2 and higher bias values at HJA-VAN, NWT-SDL, and JD-124 (Fig. 4) could likely be improved with model tuning, but we did not pursue such an approach."*

20-3 "In that context, one can consider the RegBi model as a baseline given its top rank in a Northern Hemisphere precipitation phase method comparison". Please describe and discuss the results presented here (figure A1) that seem to show similar performance for a range of methods that incorporate humidity information. The discussion as it is not balanced and does not accurately reflect the results presented. Please revise.

Please see edited discussion Sect. 5.1 above.

20-11 "a referendum." This does not seem an appropriate term – please revise. You could either give an expert view based on the results presented here, or cite others work.

Please see edited discussion Sect. 5.1 above.

20-24 "Therefore, our use of a single model may overestimate or underestimate the sensitivity of snow cover evolution to precipitation phase method at certain sites and points in time." This statement is very broad - more effort is needed to quantify and discuss the uncertainty of the model simulations.

This statement is broad because model intercomparisons say little about the effect of precipitation phase method selection. For example, SnowMIP2 used different precipitation phase methods at different sites (Rutter et al., 2009). Thus it is still unknown how model selection and phase partitioning methods interact (i.e., would a temperature index model be more affected than a physics-based model?). We stand by our statement but have clarified with some extra text:

"Given this variable performance and differences in snow model structure and physics, it is possible that some models may be more or less sensitive to the choice of a precipitation phase method. Our use of a single model may overestimate or underestimate the sensitivity of snow accumulation and melt to precipitation phase method selection. Future research should therefore focus on how model choice affects the sensitivity of simulated

snow cover evolution to precipitation phase method."

21-23 "These large variations in snow cover evolution were likely due to the combined effect of reduced frozen mass entering the snowpack and subsequent changes to the snowpack energy balance". More detailed results are needed to support this statement. For example, the change in snowfall mass and albedo could be shown to illustrate the importance of the direct and indirect effects on snowpack mass balance.

There are 10 citations in the previous lines detailing how rain vs. snow affects the snowpack energy balance. We include this as a discussion because a full treatment of the energy balance data is outside of the scope of this already fairly long manuscript.

22-3 "In this context, the precipitation phase methods that produced more rainfall affected snow cover evolution not just through reduced frozen mass but also through changes to the snowpack energy budget." These results are not shown here (they could be?) so this statement is speculation. Please revise.

See response to comment above. We also added a qualifier to the sentence and followed it with a future research line:

"In this context, the precipitation phase methods that produced more rainfall likely affected snow cover evolution not just through reduced frozen mass but also through changes to the snowpack energy budget. Further observational and modeling research is warranted to evaluate how rain versus snow affects snowpack energetics."

22-25 "winter and spring average Ta values (0°C–4°C) that lead to the greatest uncertainty in rain- snow partitioning," I would argue that the uncertainty is not in the actual rain-snow partitioning, but rather due to the use of an inappropriate parameterisation (only Ta) which requires a wide range of parameter tuning. Please revise.

As we showed in our response to a comment above, the range of uncertainty stays the same when removing T_{a0}, T_{ar0}, and T_{a3}. Furthermore, the difficulty of predicting precipitation phase and the resulting uncertainty at temperatures slightly above freezing is a well known phenomenon in both hydrology (Ding et al., 2014; Harpold et al., 2017; Jennings et al., 2018) and atmospheric science (Ralph et al., 2005; Stewart et al., 2015).

23-1 Please mention that no clear relationship was found for snowmelt rate in the conclusions – this is still a key result and an important caveat to the earlier statement that "precipitation phase method introduced significant variability into simulated snow accumulation and melt".

Added per recommendation on an earlier comment.

23-30 How was the r2 calculated here? the average r2 of hourly SWE/snowdepth or something else? Please include in the text and figure caption.

Added to Methods Sect. 3.1 as noted above.

24-1 Given the poor performance of some methods (Ta0 ,Ta3, Tr0) should they be excluded from the analysis? If not, further discussion is needed.

Please see earlier comments on the T_{a0}, T_{ar0} and T_{a3} methods.

24-6 "at all stations." Given that SWE and snowdepth are only presented for some sites in Figure A2, I presume not all sites contribute to averages here? Please list the sites that contribute to each of the SWE and snowdepth validation statistics in the text or caption.

Please see new results text above.

Figure A2 – why is the snowdepth bias 0 for the JD sites?

That is an artifact of the data at JD. The low overall snow depth produced low absolute biases. We added this text to the figure caption (please note, this figure and section has been moved to Results 4.1 per recommendation on an earlier comment):

"Note: in panel (c) the low mean biases for JD snow depth are due to small observed snow depth values at the site. Mean relative biases at these stations were 35.4% (JD-125), 3.8% (JD-124b), and 35.7% (JD-124)."

Editorial comments:

10-6 "daily Ta and RH" do you mean "daily average Ta and RH"?

Yes, line changed to "daily average T_a and RH"

16-5 "not computed because for" -> "not computed for"

Redundant "because" has been removed.

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