Author Responses to Reviewer 1 (Alan Rhoades) for "Technical note: Precipitation phase partitioning at landscape-to-regional scales" by Lynn et al.

Reviewer comments are provided in normal text.
Responses are given in blue
Revised text given in italics ***(bold for emphasis)***

Summary Lynn et al. in "Technical note: Precipitation phase partitioning at landscape-to-regional scales" unveil a new rain-snow partitioning algorithm, the North American Freezing Level Tracker (NAFLT), and assess trends in California (and western US-wide) snowfall percentages in Winter (Dec-Feb), Spring (Mar-Apr), and Cool Season (Oct-Apr) over the last~70 years. To build the NAFLT, the authors utilize the NCEP/NCAR reanalysis (2.5-degree resolution) along with the PRISM (4km) reanalysis products. The authors find a more notable decline in rain-snow partitioning in spring (-2/decade to -4%/decade) than winter (-1%/decade to -2%/decade). Overall, I think the paper by Lynn et al. is well within the scope of the Journal of Hydrology and Earth System Sciences and a valuable contribution to the scientific community. The figures and results are well-posed and, importantly, the findings have both scientific and societal impact as rain-snow partitioning in mountains (particularly a regular, "healthy" seasonal snowfall total) is a critical assumption in water supply management of western US states.

Most of my comments and revision suggestions are regarding the need to fine-tune the narrative of the manuscript and further discuss/evaluate methodological uncertainties. I would suggest that the editor assign minor revisions to this manuscript.

We appreciate the positive comments and constructive comments to improve upon the manuscript provided by Dr. Rhoades. Each comment is addressed below. We look forward to submitting a revised manuscript to HESS.

Review Comments and Suggested Revisions:

Page 1 Line 11 – Change to, "...into rain and snow, particularly snow as it maximizes available water in spring-to-summer."

Thank you for the suggestion. We edited the text following the suggestion, but went for a broader 'warm season' as reservoir deliveries occur from spring through fall:
", particularly snow as it maximizes available water for warm season use"

Line 21 – You might want to cite Huss et al., 2017 here...Huss, M., Bookhagen, B.,Huggel, C., Jacobsen, D., Bradley, R., Clague, J., Vuille, M., Buytaert, W., Cayan,D., Greenwood, G., Mark, B., Milner, A., Weingartner, R. and Winder, M. (2017), Toward mountains without permanent snow and ice. Earth's Future, 5: 418-435.doi:10.1002/2016EF000514

Excellent suggestion, reference has been added.

Line 23-24 – Change to, "...and, in particular, frozen (snow) components was a foundational assumption of climate stationarity in the development of water management infrastructure and practices..."

Thank you for the suggestion to improve this sentence. We made a slight change to the suggested revision to account for the phase partitioning being an assumption as well as the concept of climate stationarity in water management. In other words, precipitation comes as rain and (mostly) snow, and we assume this will not change, so this guides our management strategies.

New text:
"The partitioning of precipitation into liquid (rain) and, ***in particular***, frozen (snow) components ***along with climatic stationarity were foundational assumptions in the development of*** water management infrastructure ***and practices*** in…"

Line 35 – Change to, "...Some examples include an upslope shift in winter snow levels..."

Change made, thank you for the suggested change in phrasing:
"…an ***upslope shift*** in…"

Line 37 – What do you mean by "decreased snowpack water storage efficiency"? Does this have to do with cold content decreases and snow ripening occurring more frequently throughout the snow accumulation season? Please clarify.

Thanks for pointing out our initially confusing text. Your interpretation is valid but not our original intent. We added a brief bit of text better describing the metric used by Das et al. (2009). The ratio of SWE to P declining implies less precipitation is being stored in the snowpack by early spring (e.g., April 1 SWE) and thus the snowpack as a reservoir is less efficient.

New text:
"decreased snowpack water storage efficiency ***as measured by ratios of cool season snow water equivalent to precipitation***"

Page 2 Line 13 – Might want to point to a study (or several) that discuss the dataset/metric inadequacies that water managers/decision makers face when using climate information. For example...Jagannathan, K., A.D. Jones, and I. Ray, 0: The making of a metric: Co-producing decision-relevant climate science. Bull. Amer. Meteor. Soc., 0, https://doi.org/10.1175/BAMS-D-19-0296.1

Great suggestion to include this concept. We added a sentence highlighting this issue:
"These are among many inadequacies regarding datasets or climate metrics faced by water managers (e.g., Jagannathan et al., 2020)."

Line 19 – Change to, "...scales and, therefore, could be an informative diagnostic for both model development and water resource management in snow dependent regions..."

Good suggestion to add impact to model development and change 'important' to 'informative'. We made the changes (though we changed the order on model development since the paper is focused on management):
"Because the method uses publicly available gridded data sets, the indicator is scalable to regional-to-continental scales and ***therefore could be an informative diagnostic*** for water resources management ***and model development in snowmelt dependent regions***."

Line 31 – Change to, "...higher with decreasing latitude where median annual precipitation greatest in the Northern Sierra Nevada..."

Thanks for requesting clarity regarding where the wettest regions are in a latitudinal sense. We re-wrote this sentence as two:
"The elevation distribution of the analysis zones shifts higher with decreasing latitude**. *Median*** annual precipitation is the greatest in the ***higher latitude*** Northern Sierra Nevada and Southern Cascade regions."

Figure 1 caption – Change to, "Estimated historical (1950-1969) percentages of..." In my opinion, the dataset resolution part is TMI in the figure and should just be stated in the methods.

We removed the horizontal resolution part from the caption.
New text:
"*Estimated historical (1950-1969) percentages…*"

Page 3 Line 3 – Just to clarify, DWR uses the proprietary 800m PRISM product, but did not give you access for this analysis?
Yes, DWR uses the 800 m PRISM, and we did initially consider doing the analysis at 800 m. However after discussions, we felt that doing the analysis at the 4 km scale was reasonable from both a physical perspective (see below) but more so since many agencies or groups may not have the resources to pay for the 800 m PRISM products and wanted to show that the method works for the 4 km product.

It would be interesting to know how much of a different answer one would get for rain-snow partitioning if you were to use the 800m vs 4km (i.e., 5x coarsening) PRISM product (particularly in the Southern Cascades)? Similarly, performing a sensitivity analysis of another 5x coarsening (~20km) of the 4km PRISM product could be informative for climate modelers too.
In our preliminary analyses, the results did not appear sensitive to the 800 m vs. 4 km resolution. This is likely because potential differences at finer spatial scales were smoothed out by the

elevation-bin size. Spatial differences between the two PRISM products resulting from the interpolation scheme may also not be physical, since no additional data at finer scales is being included in PRISM (remembering that mountain observations are very sparse to begin with). Further, these spatial differences likely also are canceled out when aggregating to the watershed scales that matter most for water management. We would expect fine scale differences to appear when doing site-specific comparisons (and not aggregating to watershed scales), especially in areas of very complex terrain or large elevation gradients. However, challenges would emerge to test the robustness of these differences in areas where no observations are nearby to ensure that they are physical and not a product of PRISM. This is a limitation with all gridded data products.

We added a note in the limitations section that differences between the PRISM products likely cancel out at the scales of interest here but that site-specific comparisons should show differences:

"Differences between PRISM products at the 4 km and 800 m scales likely cancel out both from the elevation binning procedure and from the aggregation of data to the watershed scales used by water management. However, we would expect site-specific comparisons to yield differences."

The coarsening experiment is a good suggestion, and worth investigating further in subsequent work. We added a sentence to the concluding remarks to highlight this:

"The main advantage of the described approach is that the NAFLT can be periodically updated as higher resolution gridded data products become available (e.g., TerraClimate; Abatzoglou et al., 2018). ***It could also be expanded in scope to evaluate global rain-snow partitioning in global or regional climate models by aggregating to the spatial resolutions used in these models."***

Given that these are diagnostic estimates of rain-snow partitioning, could the authors use the Sierra Nevada Snow Reanalysis (SNSR) from Margulis' group at UCLA - https://margulis-group.github.io/data/ - to explore how different of answer one might get using the author's method vs other methods? This could also include (at least qualitatively) a comparison between more physics-based rain-snow partitioning estimates/trends in the literature versus NAFLT.

This is a great suggestion, and something we are actively working on. One limitation is the robustness of the SNSR at elevations below 1500 m: "The reanalysis dataset presented herein covers 20 watersheds and is applied to elevations above 1500 m, which represents the nominal snow line (Bales et al. 2006; Guan et al. 2013)" (quoted from Margulis et al. 2016). While beyond the scope of this study, as this is intended as a technical note to describe a general methodology with the hope/intent to inspire work exactly as the reviewer noted, we are also exploring other SWE reanalyses and remote sensing products. Our approach does not technically resolve SWE, but rather snowfall liquid water equivalent. Hence comparisons with SWE products would be flawed by not considering ablation processes. That all said, we added a line to close the limitations section describing how snow reanalyses could come in handy for addressing limitations in the approach described.

Added sentence:
"To begin to address these limitations, other high temporal (daily) and spatial resolution (>6 km) snow datasets can be utilized, such as snow reanalyses that incorporate satellite and/or in situ data (e.g., Margulis et al., 2016; Zeng et al. 2018)."

Added citations:
Margulis, S. A., Cortés, G., Girotto, M., and Durand, M.: A Landsat-era Sierra Nevada snow reanalysis (1985–2015), J. Hydrometeor, 17(4), 1203-1221, 2016.

Zeng, X., Broxton, P., and Dawson, N.: Snowpack change from 1982 to 2016 over conterminous United States. Geophys. Res. Lett., 45, 12,940– 12,947. https://doi.org/10.1029/2018GL079621, 2018.

Line 20-25 – Might be helpful to cite Jennings et al., 2018 when discussing the "hydrometeor energy balance theory" of snowflakes persisting in above freezing temperatures. Jennings, K.S., Winchell, T.S., Livneh, B. et al. Spatial variation of the rain–snow temperature threshold across the Northern Hemisphere. Nat Commun 9, 1148 (2018) https://doi.org/10.1038/s41467-018-03629-7 As you expand NAFLT for use beyond the Sierra Nevada (i.e., a more maritime mountain), it might be important to build in (or at least assess the sensitivity of adding in) specific humidity/relative humidity into the rain-snow partitioning algorithm.

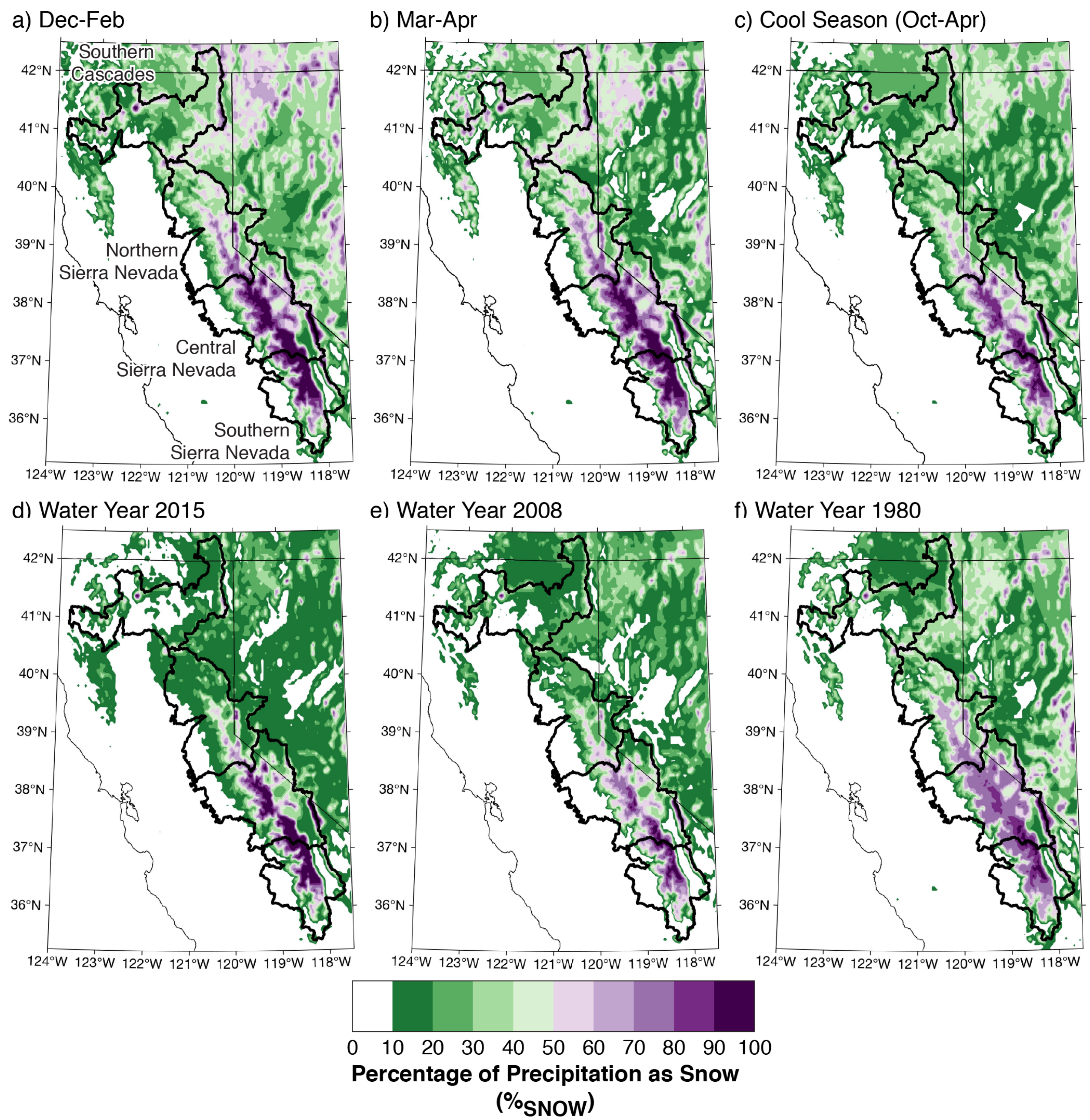
This is a great suggestion, we added the citation and also added a line to the limitations section about including RH or wet bulb temperature (among other variables) as potential ways to further improve the method:
"Further, comparisons with approaches that include relative humidity or wet bulb temperatures are recommended to further improve the methodology, as these have been shown to improve the quality of rain-snow partitioning (Harpold et al., 2017, Wang et al., 2019)."

Added citations:
Harpold, A. A., Rajagopal, S., Crews, J. B., Winchell, T., and Schumer, R.: Relative humidity has uneven effects on shifts from snow to rain over the western US, Geophys. Res. Lett., 44(19), 9742-9750, doi:10.1002/2017GL075046, 2017.

Wang, Y. -H., Broxton, P., Fang, Y., Behrangi, A., Barlage, M., Zeng, X., and Niu, G. -Y.: A wet-bulb temperature-based rain-snow partitioning scheme improves snowpack prediction over the drier Western United States, Geophys. Res. Lett., 46, 13825– 13835, https://doi.org/10.1029/2019GL085722, 2019.

Page 4 Line 1-2, Figure 1 – It might be useful to also plot a median snow water year(e.g., 2007-2008)? Also, why not use 1982-1983 for the max snowpack year (DWR's max SWE year - http://cdec.water.ca.gov/snowapp/swcchart.action)?

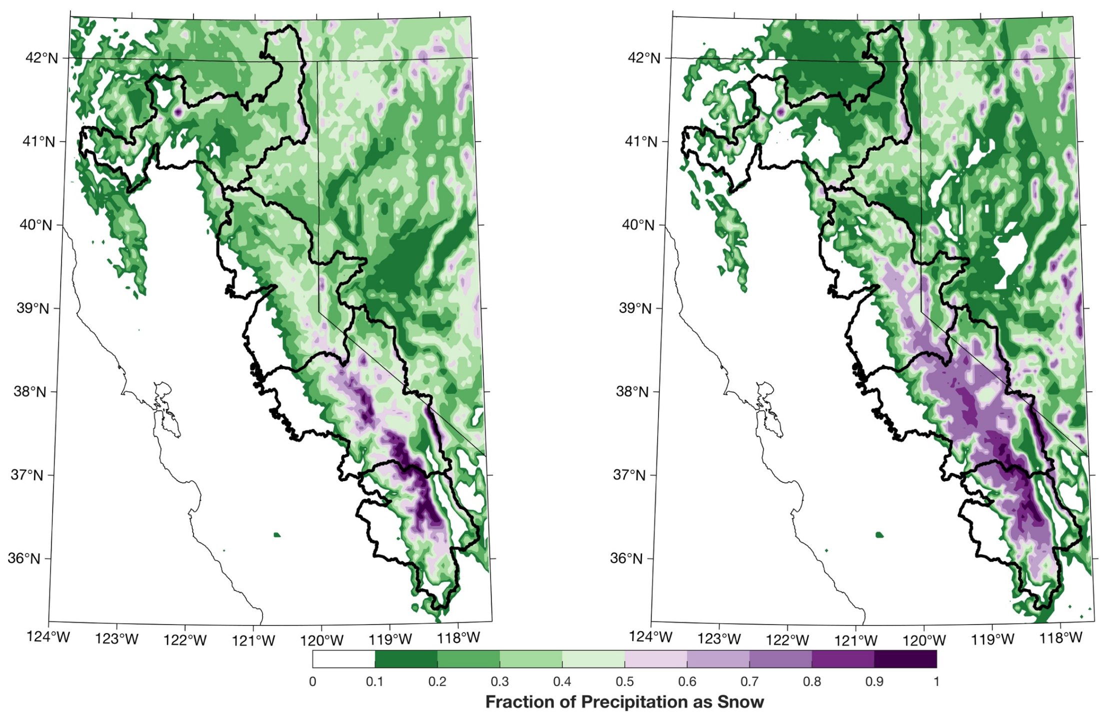
We see the reviewers point, and have changed the lower panels of Figure 1 to better show examples of interannual variability. We used the suggestion for 2008 as the median year (b) and have a low $\%_{SNOW}$ year (2015; panel (a)) and a high $\%_{SNOW}$ year (1980; panel (c)). Our new Figure 1 is as follows:



"Figure 1: Estimated historical (1950-1969) percentages of precipitation as snow for (a) winter (Dec-Feb), (b) spring (Mar-Apr), and (c) for the full cool season (Oct-Apr). Examples of $\%_{SNOW}$

averaged over the cool season (October-April) of water years (d) 2015, ***(e) 2008***, and (f) 1980. Thick black contours denote California Department of Water Resources analysis zones."

To address the reviewer's point, we did generate a plot of WY1983. However, it appears less snowy than 1980. This is likely a result of the signal of several warmer-than-normal storms during 1983 (recall there were some substantial flood events) and provides an example showing how $\%_{SNOW}$ and SWE are not directly linked. If one is measuring in terms of SWE, additional water added to the snowpack through rain could result in a bigger SWE year than a year that had all snow but less overall precipitation.



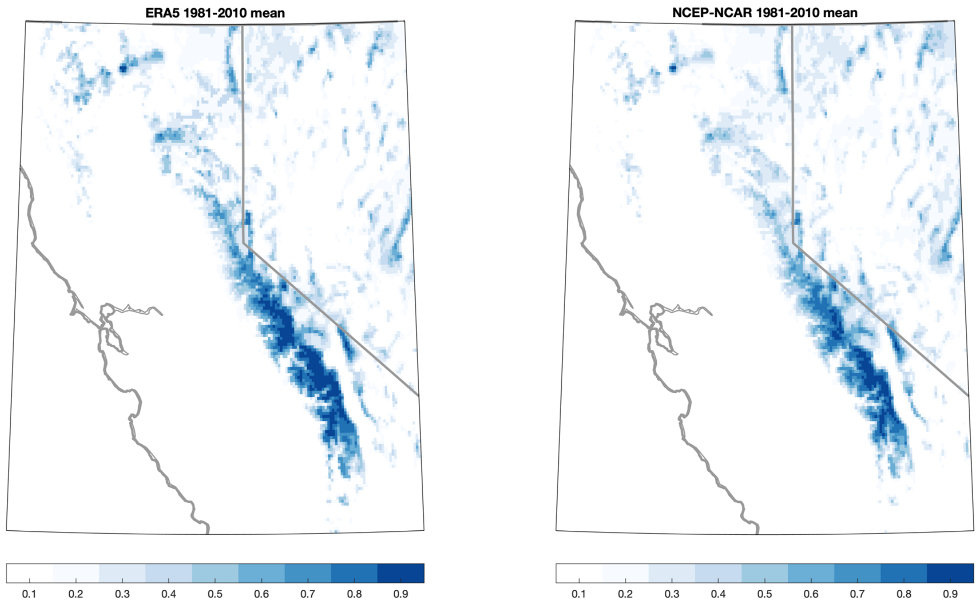
Fraction of precipitation as snow during water year 1983 (left) versus water year 1980 (right).

Line 4-8 - This is beyond the scope of this current study (and seems to be discussed more in Hatchet et al., 2017 and in the "Primary Limitations" section of this article), but given that NCEP/NCAR reanalysis is fairly coarse (2.5-degree resolution) do the authors have a sense of the magnitude of uncertainty baked into rain-snow partitioning estimates in the NAFLT (i.e., confidence intervals)? For example, the freezing isotherm may be influenced by aggregation of sharp gradients in topography in NCEP/NCAR (i.e., resolution dependence) and the precipitation estimates may lack extreme precipitation events (i.e., statistical relationship assumptions in PRISM and/or coarse grid averaging in NCEP/NCAR) and/or may be lower bound estimates of orographic enhancement of storms. The use of the new ECMWF generated ERA5 reanalysis product (i.e., global, 1950-present, hourly/monthly,~30km, up to~137 vertical levels) might be a path forward to explore/address any uncertainties in NAFLT too (https://confluence.ecmwf.int/display/CKB/ERA5%3A+data+documentation). At the very least, I think a brief discussion in the manuscript on the potential sources (or even magnitudes and confidence intervals) of uncertainty within the NAFLT rain-snow estimates might be useful and informative to users.

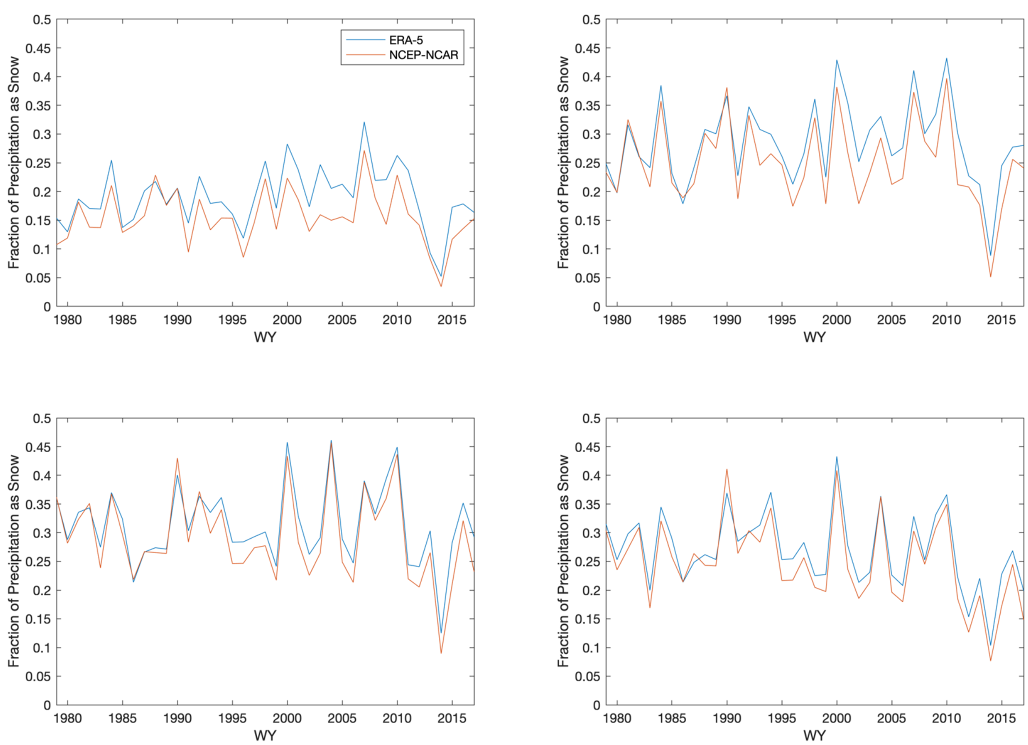
These are excellent points and similar concerns with NCEP/NCAR were also brought up by the other reviewer. Following both reviewer's suggestions, we repeated the analysis with ERA-5 for the four aggregated DWR watersheds to provide some estimates of how well NCEP/NCAR performs. We found encouraging results (figure below, added as a supplementary figure), with ERA-5 and NCEP/NCAR being very well-correlated over the overlapping time period (correlations exceeding 0.9). ERA-5 was a bit colder (more %SNOW), which is likely related to a number of improvements in the ERA-5 model compared to NCEP/NCAR (data assimilation, spatial/vertical resolution, terrain, physical process representation). We added a paragraph to the limitations section highlighting our use of an older model (which was state-of-the-art at the time the NAFLT was developed in ~2008) and showing that it still performs relatively well. All in all, this comparison suggests that the method we are showing is valid and can be a way to evaluate precipitation partitioning in models.

"The NCEP/NCAR reanalysis, which the NAFLT uses to identify freezing levels and partition precipitation is an older generation model. Recent advances in atmospheric reanalysis products such as ERA-5 (Hersbach et al., 2020) may provide additional benefits given their advances in data assimilation procedures, greater resolution in time and space (both horizontal and vertical), improved representation of terrain gradients and physical processes, and direct production of 0°C height as standard products. Comparisons of the NCEP/NCAR approach to ERA-5 showed that NCEP/NCAR performs reasonably well in terms of the mean spatial distribution (Supplementary Figure 3), with high correlations ($0.9<R<0.99$) of interannual $\%_{SNOW}$ variability between the models (Supplementary Figure 4). ERA-5 was consistently colder, with approximately 5% more $\%_{SNOW}$, supporting the notion that NCEP/NCAR is a conservative estimate of the freezing level. However, because it begins in 1979, ERA-5 precludes longer analyses periods. The method for partitioning precipitation described herein shows promise despite its use of an older reanlaysis model. As the NAFLT is updated, we anticipate including all available global reanalyses to provide an ensemble perspective of historic freezing level and precipitation partitioning."

New Supplementary Figures that will be added to the revised manuscript:



Supplementary Figure 3: Comparison of 1981-2010 mean water year fraction of precipitation falling as snow (multiply by 100 to yield $\%_{SNOW}$) for northern California and western Nevada produced using ERA-5 (left) with NCEP-NCAR (right).



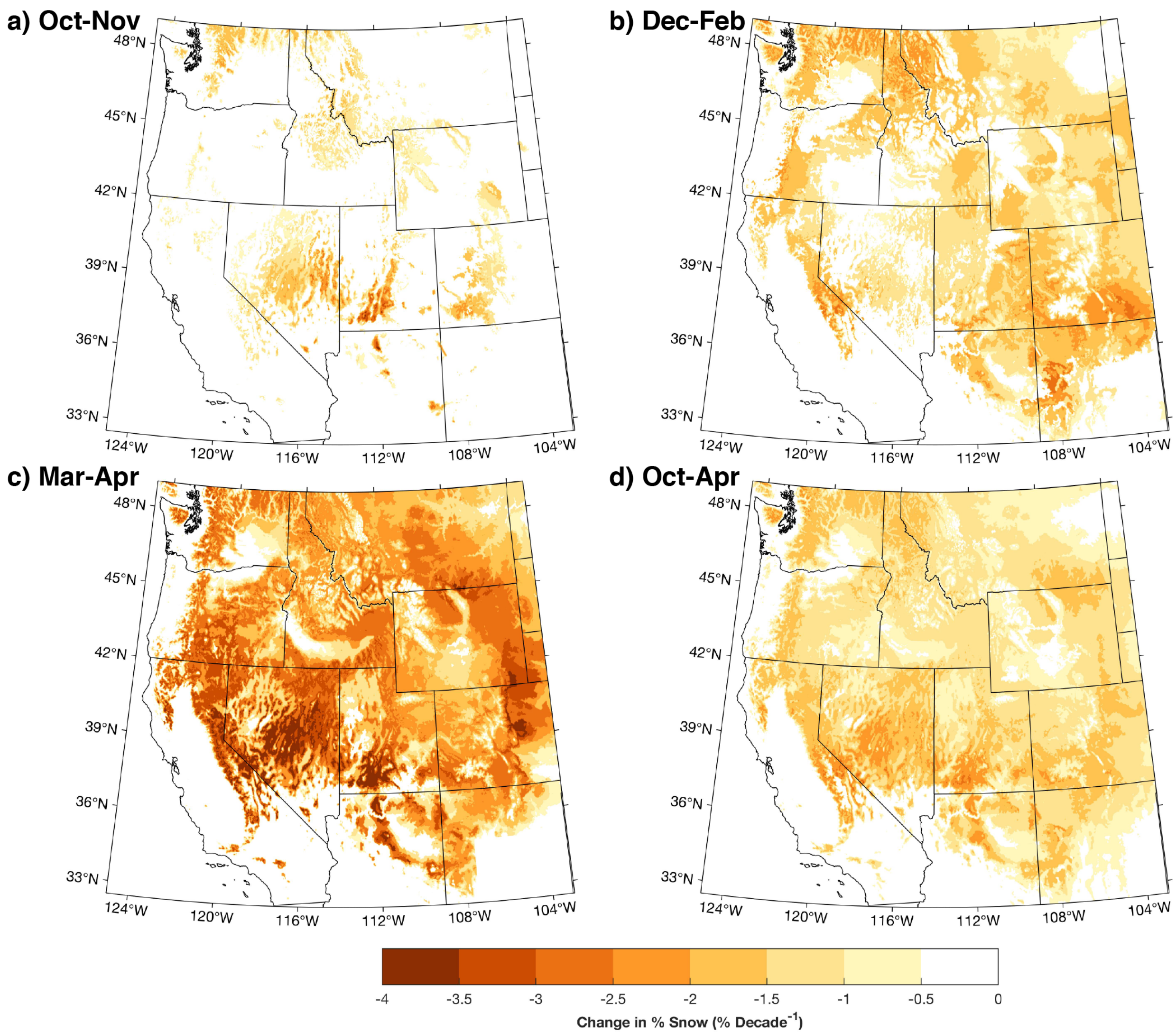
Supplementary Figure 4: Comparison of fraction of precipitation falling as snow for ERA-5 (blue line) and NCEP-NCAR (red line) for the period 1979-2018 for the four DWR analysis zones, ordered clockwise from upper left: Southern Cascades, Northern Sierra Nevada, Central Sierra Nevada, and Southern Sierra Nevada.

Figure 2 – Is there any value in looking at trends in Oct-Nov too? I am curious if there is an asymmetric or symmetric response in rain-snow partitioning between the "shoulder" months of the Cool Season.

Fall trends were not nearly as strong in California as other seasons, and west-wide there were only a few locations of stronger signals (leeside of the WA Cascades, central Great Basin, southern Utah, higher elevations in the Rockies) so initially we omitted these results.

Looking more closely, these trends are interesting since they do affect the highest elevations (CO Rockies, Wind Rivers, NW Montana ranges) We will leave the main manuscript figures showing California as they are, but will include fall in the west-wide Figure 5:

New Figure 5:



Line 21-30 – Is there any added value in evaluating sliding (rather than fixed) decadal

trend analysis? Or, more specifically (may be a follow-up study), isolate trends based on certain climate variability indices? For example, the ENSO Longitude Index (ELI)...Patricola, C.M., O'Brien, J.P., Risser, M.D. et al. Maximizing ENSO as a source of western US hydroclimate predictability. Clim Dyn 54, 351–372 (2020). https://doi.org/10.1007/s00382-019-05004-8
This is a great follow-up study suggestion, and the exact direct we'd like to go (in addition to improving the calculation of the metric). For example, Abatzoglou 2011 did find that trends in the PNA had contributed to a hastening of freezing level increases and declines in precipitation as snow; additional exploration of how modes of variability influence freezing levels would certainly add value.

While beyond the scope of this methods paper to evaluate modes of variability, we have added a note that this would be a fruitful area of further research:

"Further examination of how freezing levels are influenced by large scale modes of climate variability are also recommended. For example, Abatzoglou (2011) found trends in the Pacific-North American pattern contributed to increases in freezing levels and declines in precipitation falling as snow. Evaluating freezing level and precipitation phase relationships to isolated modes of climate variability may provide useful guidance for hydroclimate predictability at lead times relevant for water management (e.g., Patricola et al. 2020)."

Line 21-30 – Figure 3 – Do the authors want to discuss potential physical mechanisms regarding the much larger Spring declines in rain-snow partitioning on the leeside (i.e.,-4%/decade) compared with windward (i.e., -1-2%\decade) of the Sierra Nevada, particularly in the northern-to-central HUC watersheds? Topography is mentioned but given that there is an asymmetric response between even abutting windward and leeward HUC watersheds (and this is more seen in the Spring rather than Winter), are there potential physical mechanisms that should be discussed? For example, are these changes due to less Spring storms overall or are there the same number of Spring storms, but they are warmer and thus more readily produce rain? Another difference could be that the leeward HUC regions mix trends in the Sierra Nevada with the White Mountains and mask storm-type changes in rain-snow partitioning (e.g., large-scale vs convective and/or inland AR penetration).

We appreciate the suggestion to add some discussion on the windward/leeward and spring trends. There are likely dynamic explanations for these trends, however without substantial effort that goes beyond the scope of a methods paper, we would be left speculating. We have included additional text that the method described can help identify curious spatial behaviors that warrant additional research to provide a physical explanation:
"The apparent asymmetric warming of the leeside of the Sierra Nevada compared to the windward side (Fig. 2) warrants additional investigation to elucidate physical mechanisms generating this asymmetry. One candidate mechanism, which may be broadly applicable for the spring season, was demonstrated by Gonzales et al. (2019) who found robust warming of landfalling March atmospheric rivers. The watershed-scale signal may also be a by-product of the greater land area at higher elevation in leeside watersheds. A benefit of the spatially distributed nature of the DWR approach is that it facilitates the identification of spatial behaviours that may not be readily apparent in station observations."

Citation added:
Gonzales, K. R., Swain, D. L., Nardi, K. M., Barnes, E. A., and Diffenbaugh, N. S.: Recent warming of landfalling atmospheric rivers along the west coast of the United States, J. Geophys. Res. Atmos., 124, 6810– 6826, https://doi.org/10.1029/2018JD029860, 2019.

Line 28 – Change to, "...remain upslope of the 0 degree C elevation..."
Change made, thank you for the suggestion.

Page 5 Line 5-6, Figure 4 – In addition to watershed area (i.e., proxy for volume of snowpack lost), it might be good to note or discuss other downstream impacts too (i.e., the acre-foot storage of reservoirs, importance of tributaries for surface water, endangered species habitat, etc.). For example, even smaller declines (at least from a water resource management perspective) above Lake Shasta might matter more than more marked declines in watersheds that do not have a reservoir downstream of them (or the reservoir storage capacity is much smaller).
Thank you for bringing up the need to include these discussion points. We added a sentence to briefly point out these impacts, as our metric could be much more (or less) useful for basins that are more (or less) susceptible to precipitation phase changes.

New text to get the idea in there:
"In watersheds with minimal or no reservoir storage, changes from snow to rain may have more impactful changes on flood hazard and habitat, especially during warm season low flows, thus requiring more creative or costly solutions."

Line 30-31 – Might want to cite a healthy number of future climate modeling studies of the western US here.

Good suggestion, we added several studies to this sentence.

"…(Klos et al., 2014; Huang et al., 2018; Rhoades et al., 2018a; Sun et al., 2018)."

We also added a sentence further up to better connect with other Sierra Nevada-specific modeling and projection studies:

"Snowpack declines are robustly projected to continue into the 21st century (Rhoades et al., 2018a) and be further exacerbated during droughts (Berg and Hall, 2017) and extreme wet years (Huang et al., 2018)."

Added citations:

Berg, N., and Hall, A.: Anthropogenic warming impacts on California snowpack during drought, Geophys. Res. Lett., 44, 2511– 2518, doi:10.1002/2016GL072104, 2017.

Huang, X., Hall, A. D., and Berg, N.: Anthropogenic warming impacts on today's Sierra Nevada snowpack and flood risk. Geophys Res Lett, 45, 6215– 6222, https://doi.org/10.1029/2018GL077432, 2018.

Rhoades, A. M., Ullrich, P. A., & Zarzycki, C. M. Projecting 21st century snowpack trends in Western USA mountains using variable-resolution CESM. Clim. Dyn., 50(1), 261– 288. https://doi.org/10.1007/s00382-017-3606-0, 2018b.

Sun, F., Berg, N., Hall, A., Schwartz, M., and Walton, D.: Understanding end-of-century snowpack changes over California's Sierra Nevada. Geophys. Res. Lett., 46, 933– 943. https://doi.org/10.1029/2018GL080362, 2019.

Page 6 Line 1-2 – Although a bit tangential to the work in this study, it could be useful to cite some other water supply strategies that can help to offset decreases in mountain snowpack (e.g., recycled water, stormwater catchment, etc.). Some of these supply-side strategies have, historically, been undervalued, but now that co-benefits are being assessed the $/acre-foot start to make more sense and could help to offset the projected low-to-no snow future California might face...“Economic evaluation of stormwater capture and its multiple benefits in California” - https://journals.plos.org/plosone/article?id=10.1371/journal.pone.0230549"...current economic analyses of storm water capture do not adequately examine differences in stormwater project types and do not evaluate co-benefits provided by the projects. As a result, urban stormwater capture is undervalued as a water supply option. To advance economic analyses of stormwater capture, we determined the levelized cost of water in U.S. dollar per acre-foot of water supply (AF; 1 AF = 1233.5 m3) for 50 proposed stormwater capture projects in California, characterizing the projects by water source, process, and water supply yield." “The cost of alternative urban water supply and efficiency options in California.” - https://iopscience.iop.org/article/10.1088/2515-7620/ab22ca"...this analysis evaluates the costs of four groups of alternatives for urban supply and demand based on data and analysis in the California context: stormwater capture; water recycling and reuse; brackish and seawater desalination; and a range of water conservation and efficiency measures. We also describe some important co-benefits or avoided costs, such as reducing water withdrawals from surface water bodies or polluted runoff in coastal waterways...."

We appreciate the suggestions to dive a little deeper into this and have added to the discussion section (in ***bold italics***) that already gained additional insight from a previous reviewer comment (*italics*):

“*In watersheds with minimal or no reservoir storage, changes from snow to rain may have more impactful changes on flood hazard and habitat, especially during low warm season flows, thus requiring more creative or costly solutions.* ***Other non-traditional strategies to offset projected decreases in mountain snowpack and achieve water supply reliability exist, such as storm water recapture, water recycling, and water markets. However, these will require economic assessments to determine feasibility (Cooley et al., 2019).”***

Added reference:

Cooley, H., Phurisamban, R. and Gleick, P., 2019. The cost of alternative urban water supply and efficiency options in California. Environmental Research Communications, 1(4), p.042001.

Line 9-10 – I am still on the fence about the argument that "model-based estimates> gridded statistical estimates" for precipitation/snowfall in mountains. There is a lot of nuance that needs to be discussed with this "movement" (which seems primarily "all-in" on WRF). For example, I think some of the assumptions/limitations of micro-physics/macrophysics schemes and boundary layer schemes in climate models need to be discussed (particularly in the context of mountains). I know this is an on-going debate (and my $0.02 is one of many), but I would ease the definitive statement regarding "skill" made here.

Agreed, we revised this sentence to ease the definition about skill and be more qualitative ("more realistic").

New text:
"Indeed, some high-resolution model simulations show more realistic precipitation amounts in mountains than some observational networks (Lundquist et al., 2020; Wrzesien et al., 2019)."

Line 29 – Change to, "...is that NAFLT can be periodically updated, as datasets become available, with higher resolution gridded data products (citations) and expanded in scope to evaluate global rain-snow partitioning."

Thank you for the suggestion, we revised the text as follows:
"The main advantage of the described approach is that ***the NAFLT can be periodically updated as*** higher resolution gridded data products ***become available*** (e.g., TerraClimate; Abatzoglou et al., 2018). ***It could also be expanded in scope to evaluate global rain-snow partitioning in global or regional climate models by aggregating to the spatial resolutions used in these models."***