To: Natural Hazards and Earth System Sciences
Reviewer 2 (Adrienne Marshall)
Re: Author Responses to Reviews of Manuscript (nhess-2021-193)
Date: 12-Nov-2021

Dear Dr. Marshall,
Thank you for your positive feedback and constructive comments. We are addressing and incorporating your suggestions and look forward to submitting a revised manuscript. Our initial responses to your comments are in **bold** with revised text in red.
Sincerely,
Benjamin Hatchett

I. General comments

This manuscript introduces a novel visualization method for illustrating snow drought evolution using phase diagrams. The authors illustrate the use of this method through a series of case studies, and evaluate the utility of the method for in situ snow observation data as well as gridded data products. They also document a web-based tool to allow users to view these snow drought phase diagrams for locations and time periods of interest. The manuscript is well-written and appropriate for NHESS, and I like the idea of using phase diagrams for snow drought identification. I have quite a few specific comments, but none of them require fundamental changes to the manuscript. The most important specific comments below are to:

Comment

Consider adding a 1:1 line to the phase diagrams and additional or alternative designations for warm snow drought

We like the idea and we will add the 1:1 line to the revised figures as well as adding discussion about the idea of alternative designations for snow droughts with below median precipitation regimes that also indicate a 'flavor' of warm snow drought.

Comment

Reconsider the implications of data presented in Figure 6 (on basin-averaged gridded data vs point scale in situ data)

We agree with this point. Please see responses later in specific comments.

II. Specific Comments by Line (L) number

Comment

Line 19 – I think the idea that a snow-to-rain transition leads to less runoff efficiency is somewhat contested; I might also consider referencing Barnhart et al. (2020) and nuancing this statement a bit.

Barnhart, Theodore B., Christina L. Tague, and Noah P. Molotch. "The Counteracting Effects of Snowmelt Rate and Timing on Runoff." Water Resources Research 56, no. 8 (2020): e2019WR026634.

https://doi.org/10.1029/2019WR026634.

Thank you for the reference and the suggestion to alter our text to address this issue. We plan to revise the text to highlight the alternative explanations for how a transition from snow to rain will alter mountain hydrology.

Comment

Line 21 – maybe specify “in snow-dominated regions”.

Change made, thank you.

Comment

Table 1 – I don’t think Table 1 is referenced in the text;

Thank you for pointing out our omission to the table citation; this has been fixed in the text.

Comment

could you specify the source of the “snow climate” designation? I don’t disagree with any of these designations, but think it might be helpful for readers. Or, describe what you mean by each when you reference these climates in the text (Line 59).

Yes, and thank you for the suggestion. We added a citation to the source of these classifications (Mock and Birkeland (2000) as our designations pertain largely to continentality (distance from the ocean).

Added citation:

Mock, C. J., and Birkeland, K. W. (2000). Snow Avalanche Climatology of the Western United States Mountain Ranges, Bulletin of the American Meteorological Society, 81(10), 2367-2392. Retrieved Nov 12, 2021, from https://journals.ametsoc.org/view/journals/bams/81/10/1520-0477_2000_081_2367

Comment

Line 121 – So the “dry but snowy” conditions could essentially either indicate anomalously cold storm events for the season, or a seasonal shift in precipitation timing (towards colder), right? Consider whether this might be a helpful rephrasing of what you already have here.

Great suggestion to add additional nuance to the discussion here. We’ve added the following text:

”In addition, a shift in precipitation timing into the colder months of the season could also drive a leftward shift towards the ”dry but snowy regime” (Gershunov et al., 2019).”

Added citation:

Gershunov, A., Shulgina, T., Clemesha, R.E.S. et al. Precipitation regime change in Western North America: The role of Atmospheric Rivers. Sci Rep 9, 9944 (2019). https://doi.org/10.1038/s41598-019-46169-w

Comment

Figure 2a – Do you think it would be appropriate to add a 1:1 line? I found myself wanting to know for a given precipitation percentile if the snowpack for that date was above or below the snowpack percentile. I know that warm snow droughts are typically defined as having approximately normal precip with below-normal SWE, but I also wonder about identifying cases where SWE is proportionally lower than precipitation – e.g., if precipitation was 30% of normal, and SWE was 5% of normal, wouldn't we still want to think of this as a warm snow drought at least to some extent? Maybe it needs a different designation? This would fall below a 1:1 line, but not in the lower right quadrant.

We like this suggestion and will explore this as we revise the manuscript. We will add a 1:1 line to the revised figures. It makes lots of sense for the low left quadrant (and maybe upper right). We will look through the data to try and find a useful example like the 30% and 5% examples brought up by the reviewer. This may lead to sub-categories that could have interesting hydrological implications.

Comment

Line 137 - "seasonally induced shifts in solar insolation" It makes sense to me that this causes melt, but why would it cause a decrease in the percentile of SWE, given that these solar insolation shifts happen at roughly the same time every year (unless there were anomalous clear skies)?

The idea we intended to explain is that normally this is still the accumulation season and apologize for not being clear in our explanation. We interpret the dry conditions and solar radiation to allow the snow-albedo feedback (which is presumably more effective at melting/ripening snow with the increasing sun angle and duration of daylight) to accelerate declines in snow percentile. Climatologically, SWE should be accumulating and even constant SWE this time of year would result in declining SWE percentiles with time. This is likely too much nuance, so ultimately we removed the text about solar radiation to avoid confusion.

Comment

Line 199 – Suggest specifying that "comparable snow drought conditions" refers to SWE percentiles only.

Good point, we added:

"(with respect to snowpack percentiles)"

Comment

Line 201 – I have not seen the Native name for Ti'Swaq' used before in scientific/snow hydrology literature and I appreciate it! Might this be appropriate for any other place names you use?

Definitely, and we will alter other names in the text for locations we can find Indigenous place names, such as Da Ow (Lake Tahoe).

Comment

Line 226 – Yes, although depending on how you define hydroclimate, Marshall et al. (2019) showed projected decreases in interannual variability of peak SWE in many locations.

Thank you for pointing out the need to be more explicit in how we define hydroclimate (our intent was more focused on precipitation here) and to add the point about decreasing peak SWE variability. We have revised the text as follows:

"Enhanced interannual precipitation variability and decreased peak SWE variability are expectations of a warming climate (Boer et al. 2009, Pendergrass et al. 2017, Marshall et al. 2019)."

Comment

Figure 6 – It seems like the two basin-averaged snow drought phase diagrams are much more similar to each other than either is to its respective SNOTEL site. It seems the text interprets this figure as saying that basin-averaged SWE diagrams are useful when no in situ observation exists; while I don't disagree that this may be the case, I'd be more inclined to comment on either the limitations of SNOTEL sites for representing basin characteristics or the impacts of spatial averaging on how we interpret these data.

Good suggestions, and thank you for bringing this point up. While our initial interpretation intended to demonstrate the basin-average SWE diagrams will work in lieu of in-situ observations (i.e., they appear reasonable), the two components of your comment are important to include in our discussion. From my understanding, SNOTEL stations are placed with the intention to be representative of the snow-dominated portion of the watershed, but they are limited by site access as well as other constraints such as wilderness designations, and thus may not be fully representative of the basin characteristics. To your second point, the spatial averaging could have a notable impact on the behavior of the phase diagram owing to basin hypsometry, accumulation patterns (and whether these are well-represented by the spatially-distributed snowpack model), and other factors such as land surface and vegetation characteristics.

We will revise the manuscript to include this discussion, as this may help users better interpret differences between phase diagrams created using in-situ observations, gridded products, or other spatially-distributed blended observational products utilizing remotely sensed data.

Comment

Line 276, 279 – You refer to making the phase diagrams "meaningful" in complex terrain – could this get a little more specific and denote "meaningful to what?"

Clarify this: Substitute "extract maximum information value for application of interest" What scale or process are we interested in?

Yes, we appreciate the suggestion to be more specific as well as the suggested revision. Our intent was to get at meaningful scales or aspects of the watershed for management or science, i.e., do we need to examine the basin as a whole, or focus on a particular elevation band or subwatershed?

Using the suggested text, we added details on what we mean by meaningful. New text as follows:

"The challenge is how to aggregate spatial information to extract maximum information value for the application of interest (e.g., water management, avalanche forecasting, ecosystem processes) regarding the state and evolution of snowpack conditions at relevant scales (e.g., the full watershed, a sub-basin, or within specific elevation bands) in complex terrain."

Following this line of thought, we also revised the second use of "meaningful" later in the paragraph: "Creating meaningful phase diagrams *for varied management and scientific applications* using spatially distributed information is the primary goal of our ongoing research.

Comment

Figure 8 – I like this figure for giving a time series version, but it feels a little disappointing at this point in the manuscript to resimplify to percentile-based snow droughts. Could this figure be expanded (either with additional panels or additional colors, as in Figure 7) to differentiate between warm and dry snow droughts (as you suggest in Line 323)?

You bring up a good point about the awkwardness of re-simplifying to percentile-based snow droughts at this location in the manuscript. Instead of making the figure more complex by expanding it-we like its simplicity-we have opted to re-organize the manuscript slightly and now will use this figure as a jump off point to lead into the phase diagram idea. Our revised manuscript will start out with this figure to demonstrate a simple percentile-based approach to motivate the idea and need for snow drought across the western US (while also touching on region-wide trends in snowpack decline). This figure helps us build up the idea that different types of snow drought (e.g., warm vs. dry) have varying impacts on natural resources and ultimately to motivate the need to look at snowpack behaviors through the lens of warm or dry snow drought as well as including the time component.

Comment

Line 320 – I suggest using relative p-values (e.g., $p < 0.001$), rather than giving a precise number; it risks placing more importance on the numerical p-value than is warranted. It's interesting that the April trend is smaller but more consistent (significant) than the December trend; you could point out that this is probably because April trends integrate warm temperatures over the course of the water year, while December trends are more impacted by stochastic precipitation and warm/cold events.

Yes, your point about placing too much importance on the value is well-taken. We will revise to report relative p-values in the figure.

We will also add a line of text following your thoughts about the April vs. December trends; this is a good point to bring up and, in line with your previous comment, also helps motivate the phase-diagram approach to evaluate why a given season ended up with less-than-normal peak SWE.

Comment

Line 370 – Percent of average also gets weird at the end of the season, either in big snow years (percent of average can be huge or infinite, if average is zero), or in years with earlier than average melt date (percent of average can be very small for a few days, but this may exaggerate the apparent low snow conditions).

Yes, and seeing some of the huge numbers reported in the media or elsewhere was part of the motivating factor to move away from percent of average.

Comment

Line 400 – I appreciated this discussion of the differences between percent of medians and percentiles.

Thank you! We found this example to be eye-opening with respect to how different these values can be for different locations.

Comment

Line 416 – You mention climate change here; it may also be appropriate to provide a brief mention somewhere of the impact of the period of record on these percentiles. Should the period of record used to calculate percentiles be allowed to extend over non-stationary climates? When comparing multiple sites, how should users account for potential differences in the period of record used for percentile calculations?

These are important and relevant points that introduce challenges to our analysis (multiple site issues) especially as the climate changes (non-stationarity issue). We will address both of these in the revised manuscript, but here are our initial responses. While comprehensively handling these issues (i.e., solving them) is beyond the scope of our work, bringing up these issues are important for highlighting the limitations of our approach.

To the first point, on non-stationarity, it seems salient to use a fixed historic period based on water management assumptions from which to calculate percentiles. This is difficult in practice given that water rights allocations and water management frameworks were developed between the late 1800s and mid-1900s (culminating in the mid-20th century-era of dam building) given the only snowpack information for this period includes the monthly snow courses. Other work by one of the authors (Hatchett et al. 2015, 2018) in the western Great Basin (eastern Sierra Nevada) suggests that the period 1971-2000 is on par with the wettest period in the last 3,900 years. In addition, for the same region, Sterle et al. (2019) highlighted the period spanning 2010-2017 included years that were on par with the average wettest and driest climates at millennial timescales. In the Upper Colorado Basin, despite persistent drought in recent decades, several extremely wet and snowy years have been observed (e.g., 1993, 2011, 2019). It thus seems reasonable to include data for these years to capture the range of natural variability, however observed snowpack decline (e.g., Mote et al. 2018; Siirila-Woodburn et al. 2021) is likely shifting snowpack distributions towards lower values. We are thus left with a difficult choice as to when should additional years in the percentile calculation no longer be added to ensure we don't appear to be out of snow drought when in reality, to paraphrase the late Kelly Redmond, "not enough water is avail-

able to meet needs (human or ecosystem)". The reviewer has brought up a difficult but critical question that we will grapple with as we revise the manuscript.

Hatchett, B. J., Boyle, D. P., Putnam, A. E., and Bassett, S. D. (2015), Placing the 2012–2015 California-Nevada drought into a paleoclimatic context: Insights from Walker Lake, California-Nevada, USA, Geophys. Res. Lett., 42, 8632–8640, doi:10.1002/2015GL065841

Hatchett, B.J., Boyle, D.P., Garner, C.B., Kaplan, M.L., Bassett, S.D., and Putnam, A.E. (2021), Sensitivity of a western Great Basin terminal lake to winter northeast Pacific storm track activity and moisture transport, From Saline to Freshwater: The Diversity of Western Lakes in Space and Time, Scott W. Starratt, Michael R. Rosen. Geological Society of America Special Papers 536, https://doi.org/10.1130/2018.2536(05)

As a start to this response, we have added text to address this comment (normal font for context): "They can also be applied to investigate how climate change may permanently alter phase diagram trajectories and/or residence times of WY snowpack conditions in particular quadrants of the phase diagram. *However, as warming shifts the distribution of early, late, and spring peak snowpack towards lower values, it is worth considering holding the period from which percentiles are calculated constant over a management-relevant time period. This would reflect the non-stationarity of the climate as well as the historic conditions from which water management made assumptions about snowpack and water availability (Siirila-Woodburn et al. 2021).*"

Regarding the differing periods of records for stations, we will add text to the phase diagram figures such that readers are made aware of the period of record used. How users account for these differences is a perennial challenge in applied/service climatology, as knowing long-term context allows users to differentiate between a station with more data that captures a range of years when comparing with a station with a different period of record that may not have as large a range or the same distribution. One way to account for this might be to allow the user to select the start year of the period of record in the web-tool in order to allow direct comparisons of the same years. The code used to generate the figures in this manuscript includes such functionality. We added a note in the methods that only sites with at least 20 years of data should be used, and added a note in the limitations that to facilitate comparisons between stations, similar periods of records should be used:

New text in methods: "We recommend only using stations with at least 20 years of data."

New text in limitations: Our snow drought phase diagram visualization approach is not without limitations. "First, we used the full period of record available for stations to calculate percentiles. In cases where stations being compared have sufficient data (i.e., at least 20 years) but differing periods of record, selecting commonly overlapping periods from which to calculate percentiles may avoid biases created by a station whose full record captures anomalous conditions (e.g., a notable wet, dry, warm, or cold set of years) compared to a station with a shorter period of record."

Comment

Line 420 – Could you say a little more specifically what concerns were highlighted in the papers you reference here?

Certainly, thank you for the suggestion to be more specific. New text highlights a few specific concerns including runoff forecast timing, reservoir storage, and decreased water quality and quantity. New text:

The goal of these diagrams and the web-based tool is to alleviate some management concerns outlined in Hossain et al. (2015) and Sterle et al. (2019), including runoff forecast timing errors, lack of upstream reservoir storage, and reductions in water supply reliability and water quality. These concerns can be start to be addressed through illuminating water supply uncertainties across a range of hydroclimate conditions, enhancing the flexibility of subseasonal-to-seasonal water management practices, and improving coupled atmospheric and hydrologic forecast systems (Siirila-Woodburn et al. 2021).

Comment

Conclusions – Could you say anything more about how the snow drought phase diagrams might support scientific innovation, rather than only focus on the management/end user audiences?

Yes and thank you for pointing out the opportunity to broaden the application of the phase diagrams. We added a few examples to the conclusions:

"Last, snow drought phase diagrams can support innovative ways in thinking about how weather and hydroclimate variability influence the mountain environment. For example, phase diagrams help us re-frame snow drought as a time-dependent process rather than a point-in-time concept, helping to contextualize hydrologic outcomes such as runoff efficiency. Phase diagrams also can be blended with other indicators relevant for ecosystem function (e.g., Contosta et al. 2019) to explore how snow drought impacts forest health or limnnology as well as changing wildfire behavior Alizadeh et al. (2021)."

Added citations:

Contosta, A. R., Casson, N. J., Garlick, S., Nelson, S. J., Ayres, M. P., Burakowski, E. A., Campbell, J., Creed, I., Eimers, C., Evans, C., Fernandez, I., Fuss, C., Huntington, T., Patel, K., Sanders-DeMott, R., Son, K., Templer, P., and Thornbrugh, C.. 2019. Northern forest winters have lost cold, snowy conditions that are important for ecosystems and human communities. Ecological Applications 29(7):e01974. 10.1002/eap.1974

Alizadeh, M.R. Abatzoglou, J.T., Luce, C.H., Adamowski, J.F., Farid, A., Sadegh, M., (2021), Warming enabled upslope advance in western US forest fires, Proceedings of the National Academy of Sciences, 118 (22) e2009717118; DOI: 10.1073/pnas.2009717118

I checked out the snow drought phase diagram tool – it was easy to use and is a great addition to the manuscript, in my opinion. My only critical comment on it was that I found that the color scale was not very intuitive (could you use the same color scale as Figure 2a?)

Thank you for checking out the tool! We agree and are working with the web-developers to implement the same colormaps used in this manuscript to make for more intuitive, and color-safe, graphics on the web-tool. We appreciate you bringing this up, it helps the developers to learn about this feedback as well. They also appreciate the positive feedback about usability.

III. Technical corrections by line (L) number

Line 26 – This sentence might read more easily if you started with "Reductions in snowpack negatively impact … in addition to …" Just a style suggestion; it was hard to see where it was going.

We revised following this suggestion and agree the text reads better following this approach.

Line 41 – typo, "hydroclimate conditions to varied .."

Thank you, fixed.

Line 71 - Should "Seaber" be inside parentheses?

Thank you, fixed.

Line 88 – I don't think 'drought-busting storms' should be in quotes unless you have a citation (even though I get your intention). I don't know if the meaning of "mixture effects" in this line is clear.

Agreed, we added a citation to the quoted storm type (Dettinger, 2013). In the second sentence, we revised 'mixture effects' to be more specific to the time-varying behaviors of temperature and precipitation that ultimately control snowpack behavior. New text:

Thus, phase diagrams can help diagnose snow drought onset, termination, duration, type, and severity as well as explore timing and characteristics of large 'drought-busting storms' such as atmospheric rivers (Dettinger, 2013). By implicitly including the time-varying effects of precipitation and temperature, phase diagrams provide a unique perspective over time series plots in tracking snowpack conditions.

Added citation:

Dettinger, M. D. (2013). Atmospheric Rivers as Drought Busters on the U.S. West Coast, Journal of Hydrometeorology, 14(6), 1721-1732. Retrieved Nov 12, 2021, from https://journals.ametsoc.org/view/jou
d-13-02_1.xml

Line 181 – A few times in this paragraph, you say snow or precipitation percentiles "improved" instead of "increased." While I agree with you, I think this inserts an unnecessary value judgement, and "increased" would probably be more clear.

Agreed, thank you for pointing this out. All instances of 'improved' have been revised to 'increased'.

Line 233-234: Using “active” to describe the weather feels a little imprecise; could this language be made more specific?

Yes, we have replaced ’active’ with the more descriptive (but still succinct) ’stormy’.

Figure 7 – The color map here is clever and effective, but could you reverse the bar so that the lower numbers are on the left?

Certainly! Thank you for the insightful suggestion to improve visual clarity. The revised manuscript will include the reversed colorbar.