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Discussions

Interactive comment on "Technical note: Accounting for snow in the estimation of root-zone water storage capacity from precipitation and evapotranspiration fluxes" *by* David N. Dralle et al.

Anonymous Referee #2

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I reviewed the technical note "Accounting for snow in the estimation of root water storage capacity from precipitation and evapotranspiration fluxes". The paper is well written and easy to follow but lacks a significant scientific contribution in its current form. I would first like to commend the authors for the open-source nature of the analysis and dataset. The paper addresses an important need, namely to quantify root zone water storage in places receiving snow, but has several limitations that concern me with its application. As I understand it, the main contribution of the paper is to improve the Wang-Eralandsson et al. (2016) approach in places with snow. To do this the authors make three big assumptions. First that vegetation does not transpire when snow is on the ground. Second, that snow doesn't modify the timing or intensity of precipitation at

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Discussion paper

a daily time step. And third, that runoff losses are minimal (or occur only at soil saturation). We know that these assumptions are inaccurate, as I detail below. Therefore, I challenge the authors to better quantify improvement using additional observations and an uncertainty analysis. The concern that a method can be overly simple and therefore, gives erroneous results at the time and spaces scales applied (i.e. daily and 4 km) should be dispelled by the authors. To this end, I try to give constructive ways forward in three major comments and also some minor points to help improve the readability of the paper.

Major comment 1: The paper uses the justification of making conservative estimates of storage for many of the decisions. However, it is unclear to me that deeper storage estimates are in fact more conservative. One can think of many situations where shallower storage steps would be more conservative, e.g. flood prediction. Perhaps a range of possible storages would be more consistent with the information available. It doesn't matter how conservative or consistent the method is if it misinterprets important processes. To rephrase this concern, there are three major assumptions that the authors make in their formulation that need to be explored and discussed more. The first is that trees don't transpire with snow cover. That is clearly not the case in the Sierra Nevada and more care needs to be taken in justifying this assumption. Evidence of substantial transpiration prior to snow disappearance in the Sierra Nevada are shown in the following papers.

Royce, E.B., Barbour, M.G., 2001. Mediterranean climate effects. I. Conifer water use across a Sierra Nevada ecotone. Am. J. Bot. 88 (5), 911–918. https://doi.org/10.2307/2657044.

Kelly, A.E., Goulden, M.L., 2016. A montane Mediterranean climate supports year-round photosynthesis and high forest biomass. Tree Physiol. 36 (4), 459–468.

Cooper, A.E., Kirchner, J.W., Wolf, S., Lombardozzi, D.L., Sullivan, B., Tyler, S.W. and A.A. Harpold . Snowmelt causes different limitations on transpiration in a Sierra Nevada conifer forest. Agricultural and Forest Meteorology. 291. https://doi.org/10.1016/j.agrformet.2020.108089

The second assumption is that snow doesn't modify the timing of intensity of water inputs. I find this assumption to be troubling, particularly because it is not discussed in any detail. While one can understand that this method might capture the overall/annual fluxes of water the daily dynamics are critical to how water is partitioned in the root zone (see next point). Here are some recent citations on how snow modifies water input timing and intensity:

Yan, H., Sun, N., Wigmosta, M., Skaggs, R., Hou, Z., & Leung, R. (2018). NextâĂŘgeneration intensityâĂŘdurationâĂŘfrequency curves for hydrologic design in snowâĂŘdominated environments. Water Resources Research, 54, 1093– 1108. https://doi.org/10.1002/2017WR021290

Harpold, A. A., & Kohler, M. (2017). Potential for changing extreme snowmelt and rainfall events in the mountains of the western United States. Journal of Geophysical Research: Atmospheres, 122, 13,219– 13,228. https://doi.org/10.1002/2017JD027704

My third concern is the assumption of zero outflow during snow covered periods, which is easily shown to be false with observational data. Additionally, these high elevation snow-covered zones are known to be runoff generation areas for the large downstream rivers. Consider some discussion of vertical drainage processes. It should be noted the method makes an implicit assumption that runoff is generated when the storage is full (i.e. saturation excess). Here is some relevant modeling work: Tague, C., and Peng, H. (2013), The sensitivity of forest water use to the timing of precipitation and snowmelt recharge in the California Sierra: Implications for a warming climate, J. Geophys. Res. Biogeosci., 118, 875– 887, doi:10.1002/jgrg.20073.

Hammond, J. C., Harpold, A. A., Weiss, S., & Kampf, S. K. (2019). Partitioning snowmelt and rainfall in the critical zone: effects of climate type and soil properties. Hydrology and Earth System Sciences, 23(9), 3553-3570.

The previous application of this method was in drier locations where issues of lateral and vertical subsidy of water are less important. In order to remedy these concerns, the authors need to compare their findings against observational data or somehow quantify improvement (not just difference with the previous method). The authors should consider using a recent paper (Rungee et al., 2019) in Hydrological Processes that has data for validation in the Sierra Nevada. It is the author's responsibility to show that this method reproduces some parts of reality and does not introduce artifacts via these three assumptions.

Rungee, J, Bales, R, Goulden, M. Evapotranspiration response to multiyear dry periods in the semiarid western United States. Hydrological Processes. 2019; 33: 182– 194. https://doi.org/10.1002/hyp.13322

Major comment 2: I am also concerned about the lack of uncertainty analysis given the reliance on modeled precipitation and evapotranspiration products. In particular, the authors should consider using multiple precipitation and evapotranspiration products in order to assess how much storage is sensitive to the products versus the method itself. For example, the previous work suggest that evapotranspiration uncertainty is more important than precipitation. However, these analyses were not done in wetter places like the Sierra Nevada. Precipitation products are inherently uncertain and need to be considered in the interpretation of the storage results. An easy alternative would be to use multiple precipitation products. Evapotranspiration is more challenging since the products are less validated, however, there are sufficient products to consider this or put arbitrary error terms on the existing data set. My concern is that this method portrays a certain level of sensitivity that will be specific to the climatology of the Sierra Nevada as well as the errors and uncertainties in the products themselves. Given that snowmelt strongly modifies the intensity of terrestrial water input, it's unclear to me how a non-explicit treatment of snow melt is sufficient for a paper that is trying to include snow in subsurface water storage. Again, I think some type of sensitivity analysis is needed to justify that this modulation of precipitation intensity is not a driver of soil saturation and excess runoff (related to major concern 1).

Major comment 3: Consistency and differences with the Wang- Erlandsson et al. (2016) approach. I am a bit confused about the attempt to modify this previous work but also changing the methods. In particular, the previous work includes interception in the outgoing water flux term. Additionally, the previous work corrects for loss terms by adding back in the difference between long-term precipitation and evapotranspiration. Again the justification of a conservative estimate is used but that is unclear in terms of what that means or whether these decisions support that.

Minor comments: âĂć I am curious about the argument on line 5: If warming scenarios show decreased snowpack and increased rain as snow, wouldn't this effectively make the Wang-Erlandsson et al. (2016) more viable over time? It seems like this proposed method is most important now and would likely diminish in its ability to represent what is happening as more precipitation shifts from snow to rain. âĂć Lines 27-29: It would be helpful to understand how other estimates rely on only soil moisture and what sets this method apart in partitioning plant-accessible water. I'm not clear on how this marks a distinction. âĂć Selection of fSCA threshold– Line 90: âĂŤI don't fully understand the selection criteria for the threshold below which snow contribution to the pixel isn't assumed to be meaningful. 10% seems sort of arbitrary aside from being the uncertainty for MOD10âĂŤwhat if that 10% is deep and holds significant water? Would be great to see more justification/explanation of how to derive this threshold since it exerts powerful control over Eq. 3. How sensitive is the result on your threshold? o Uncertainty based on the selection of a thresholdâĂŤHow much impact does the selection of a threshold have and if its user defined, how much would that swing the significance of the results as presented in Figure 1?; 4) Assumption about groundwater losses/contributionsâĂŤhow much does this assumption alter the bound if it looks fairly reasonable to assume that there are significant losses/contributions on a daily timestep in this particular region? ïĆğ Enzminger, T. L., Small, E. E., & Borsa, A. A. (2019). Subsurface water dominates Sierra Nevada seasonal hydrologic storage. Geophysical Research Letters, 46, 11993– 12001. https://doi-org.unr.idm.oclc.org/10.1029/2019GL084589 âĂć Definition of winter months –line 115: Justification of the time-period selected (January- April) for analysis. Why not expand to include Oct – June since that would be more reflective of a true winter especially with the focus on higher elevation sites? âĂć Line 135: I'm still having a hard time grasping why this method is more important in a warming climate with more precipitation as rain. It seems like those are the exact conditions where Wang-Erlandsson is valid and where you have minimal differences (30 mm) in Figure 1. As we shift to rain dominated systems with less snow, it seems like it would reduce the importance of this contribution, not enhance it. âĂć Line 110: Smax not previously introduced

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