The manuscript by Dralle et al. aims to account for moisture availability in snow dominated catchments due to snow-melting and sublimation processes by modifying the Wang-Erlandsson et al. (2016) root-zone storage capacity (Sr) framework. The modified framework aims to provide a more conservative Sr estimate in snow dominated catchments and is analyzed at a much finer-resolution of 1km for Southern Sierra Nevada, CA, USA.

The modification to the original framework addresses an important aspect: excess moisture availability in snow-dominated catchments, which can influence moisture availability in a warmer climate. The manuscript is generally well written and the open access approach is laudable.

R3.1: We thank the reviewer for the careful and thorough review of the manuscript.

However, I do have some major concerns:

While it is clear that the modified framework provides a more conservative Sr estimate (i.e., lower bound), the manuscript does not provide evidence that modifications also yield more accurate estimates. However, in several places in the manuscript (e.g., P6L129), it is implied that the new estimate is also more accurate. Ideally, I would suggest that the authors provide validation through e.g., hydrological modelling (with and without modified Sr) and validation against observation-based evaporation data or gauged runoff data. However, if providing such evidence is not within the scope of Technical notes, I would suggest that the authors make it clearer in the manuscript that there is no evidence at this point that the more conservative estimate is also more accurate.

R3.2: Thanks for this insightful comment. We agree with this sentiment. We propose modifying the manuscript with:

We caution that neither this dataset nor the original dataset has been validated against direct measurements of root-zone storage capacity. Although \citet{Wang_Erlandsson_2016} performed an implicit validation of S_R via hydrological modeling, we advocate for complementary *in situ* measurements of dynamic water storage in the critical zone, which will be required for true validation of emerging remote sensing datasets of subsurface water storage \citep[e.g.][ ]{Wang_Erlandsson_2016, Enzminger-2019, Swenson-2003}. Systematic validation of this form requires significant new fieldwork efforts that we leave for future work.

• The term “conservative” may be confusing, as a low Sr might be more conservative in certain applications (e.g., flood prediction) and less conservative in others (e.g., ecosystem service valuation of drought buffering capacity). Simply sticking to the terms like “lower-bound” or “minimum” would be less ambiguous.

R3.3: We agree. We propose to use “lower-bound” throughout the manuscript, as the reviewer suggests.

• Ignoring horizontal inter-pixel flows (leakage and runoff) following WangErlandsson et al. 's (2016) methodology (implemented globally at 0.5 degree resolution) at a 1km resolution for the present 'high elevation' study area can be problematic and non-conservative. Dralle et al., states (P2L46-49) that leakage and runoff are ignored, which "results in a conservative estimate of Sr". However, while this is true for high-elevation pixels, low-elevation pixels can expect an underestimation of Fin, and hence an overestimation of Sr. It is not clear to me if and how the authors address this, please clarify.

R3.4: This is a good point, and a limitation inherent to both our proposed modification and the original method. We propose to add additional text to the manuscript:

In the case that inter-pixel flow results in a net contribution to the root-zone, estimates of S_R in our (and the original) method may not represent true lower bounds. At present, however, there are few if any methods for reliably measuring such inter-pixel fluxes at large scales, let alone for determining whether vegetation have access to these fluxes.

• The authors exclude the interception evaporation term from Fout (L86), but uses total precipitation (rather than effective precipitation) for Fin. If interception evaporation is excluded, it would make sense to also exclude the non-effective precipitation, which does not interfere with sub-surface processes. While it makes Sr estimates lower, it might not be for the right reasons. Or do the authors by the phrase "interception is not included" mean that both interception and noneffective precipitation are removed? If that is the case, the sentence formulation needs to be less ambiguous, especially as the term "interception" comes directly after "transpiration" and "soil evaporation".

R3.5: We did not include interception in the original F_out term, but agree that this might decrease the lower bound estimate of S_R, possibly for the "wrong" reasons, as the reviewer suggests. We propose to include interception in F_out as the reviewer recommends. A preliminary analysis shows this does not significantly alter observed differences with the original method, as the increase is roughly the same between both methods when interception is included. We propose to add the following to clarify the role of interception:

Although interception is not strictly sourced from the subsurface, it nevertheless may decrease the effective precipitation that reaches the subsurface. Following \citet{Wang_Erlandsson_2016} we therefore leave the interception component of the ET flux from the PML_V2 dataset in our calculation of Fout.

• A suggestion for a better overview could be to introduce a table with two columns for "before" and "after" your modifications: i.e., the first column lists the WangErlandsson et al original equations, and the second column lists the modified version. You could list all differences in this table, incl. for example resolution, and definitions of Fout.

R3.6: Because we now include interception (R3.5), the difference between the methods boils down to the difference between inclusion (or not) of snowcover in Equation 3. We don't feel we need a table to illustrate this difference. Pixel resolution is a function of underlying choice of datasets, rather than methodology, which is the focus of this tech note.

• In general, it would be helpful if the authors could more systematically describe when and how the water balance is violated.

We propose to add the following:

The water balance is violated when F_out exceeds F_in over long time periods such that changes in storage may be considered negligible relative to cumulative sums of fluxes. This could arise, for example, due to errors in the underlying datasets, or unaccounted for input fluxes (e.g., irrigation subsidies or inter-pixel flow).

Specific comments

L28: '...plant-accessible water below the soil'. Does this include groundwater? Please be specific.

R3.6: We propose:

"We emphasize that an accurate representation of $S_R$ therefore should include not only moisture available within the soil, but also plant-accessible water below the soil, which may include unsaturated storage in weathered bedrock or groundwater."

L49: "Fin and Fout are set equal to precipitation (P) and evapotranspiration (ET), respectively". However, later at L86, it is stated that "interception is not included". This can be confusing as interception evaporation generally is considered part of ET. To minimize confusion, please consider defining the Fin and Fout clearly once and then consistently throughout.

R3.7: Thank you. Please see R3.5 for more information.

L53,57: 'n' for Eq. 1 and 2 are not mentioned for the Sr calculation. Is the simulation run for the whole term (2003-2017), or is it simulated annually?

R3.8: It is run for the whole term. We propose to further clarify in the methods:

"We restrict our analysis to the temporal intersection of these three datasets (the root zone moisture deficit is tracked continuously from the 2003 to the 2017 water year), reproject into WGS84 (EPSG:4326), and resample pixels using nearest-neighbor to a 32.34 arc-second pixel scale (approximately 1 km). "

L55: ‘root-zone storage deficit’. Suggest be consistent in terminology with WangErlandsson et al., 2016.

R3.9: Wang Erlandsson et al. use various terms throughout their paper, including ‘storage deficit’ and ‘soil moisture deficit’. We prefer storage deficit, as F_out may come from the soil or underlying weathered bedrock.

L83: Dralle et al. have used PML-v2 evaporation product, which does a lot of plant function type (PFT) parameterization in evaporation calculation, leading to biome-based assumptions. Though, we believe that at such a fine-resolution, it shouldn’t matter much. However, it would add robustness to the framework if a sensitivity analysis using a different evaporation product (e.g., FLUXCOM) can be done using the modified framework. (I would recommend this for normal articles, but acknowledge that I am not sure about the scope of "Technical notes" - maybe the editor can help provide some guidance here.)

R3.11: We provide an interactive Python notebook, which can be straightforwardly edited to try different datasets. We maintain that a full inter-comparison of ET data products is beyond the scope of a methods-oriented Technical Note.

L89-90. What is the rationale for C0 = 10 %? Is C0 resolution/scale/context dependent? What are your recommendations for users attempting to apply the modified algorithm on a dataset with different topography, climate, and resolution? Furthermore, the statement ‘C0 = 10% is also the resolution of the underlying snow cover dataset’ is unclear.

R3.12: Please see our third comment to Reviewer 1; this is the minimum non-zero value of snow cover.

L110: What does Smax represent, since it hasn’t been mentioned before? What is a low-energy location? Please be more descriptive. Fig 2. Evapotranspiration is referred to as Fin, instead of Fout. Please be consistent with the notations for ‘Root-zone water storage capacity’ (Sr or Sr[L] or Smax).

R3.13: Thanks, this is a typo we have fixed. We propose to remove “low-energy” (though we meant it in the sense of Budyko).