R1 comments:

The manuscript of Dralle et al. deals with a new method to estimate root zone storage capacities. They based their work on the methods of Wang-Erlandsson et al., who did not account for snow. The new method adds a correction factor for snow, which leads to more conservative estimates of the root zone storage capacity in snowy areas.

I like the method and believe the manuscript is clearly written. I would like to make the authors a compliment about their open science approach. Sharing the notebook that creates the plots and links to the data is in my view an excellent example of open science, and unfortunately still rare. Nevertheless, I also have some issues, that the authors may want to address.

Thanks for your careful review and support of the efforts behind reproducible science!

First, I find the discussion rather short and believe it would be good if the authors reflect a bit more thoroughly on the advantages, but especially also the disadvantages of the method. For example, the root zone estimates strongly depend on the used products for evaporation and precipitation, and the accompanying uncertainties.

Thank you for this prompt; we propose to add discussion:

"Drawbacks associated with the general approach are presented in detail in \citet{Wang_Erlandsson_2016}. In particular, the results are highly sensitive to the quality of the underlying remote-sensing datasets; by making our code publicly available, we hope that as improved datasets become available they can be readily incorporated into new estimates of S_R. As noted in a similar effort by \citet{dralle_plants_2020}, we caution against using evapotranspiration datasets which rely on a soil water balance that as a model parameter rely on pre-determined values of S_R (e.g., from existing soils databases), as this would bias the inferred S_R.

Because the method relies on a mass balance approach, estimates of S_R will inherently be larger in locations where rates of plant-water use are high during extended dry periods periods; for example, in the Mediterranean-type climate of California, where the long dry summer coincides with the growing season. Consequently, S_R estimates will be less representative of total water storage capacity in wetter climates because root-zone storage deficits are frequently replenished, and therefore never reach large values. In other words, this method is only capable of documenting the root-zone storage capacity that is \textit{accessed} by plants, rather than the \textit{accessible} plant-available water storing capacity that may exist through the *whole rooting zone; the former provides a minimum estimate of the latter. In energy-limited environments, or places where seasonal precipitation and energy delivery are in phase, the method is prone to significant underestimation of plant-accessible water."*

In addition, I have some questions regarding C0. First, why is it chosen at 10%? This seems a bit arbitrary for me. More importantly, I wonder why the authors did not use the percentage snow cover as a correction factor by itself. I have nothing against the conservative method of the authors to just switch the correction on and off, but why not multiply Fout with 1-C? In other words, when 20% of a cell is covered in snow, then 80% of the cell can still contribute to evaporation, which is less conservative, but maybe closer to reality.

There is a practical reason for choosing 10%; this is the minimum non-zero value of snow-cover in the underlying data. We propose to add this comment to the manuscript:

"(snow cover is assumed negligible at less than $C_0=10\%$ pixel coverage; in this case, $C_0=10\%$ is also the minimum non-zero value of the underlying snow cover dataset)"

The reviewer makes a very observant point about the potential to scale Fout by 1-C, instead of using a C-threshold as a binary on/off switch for the entire pixel. We did consider this idea in formulating the approach, however, we ultimately decided not to attempt to account for sub-pixel processes, as we were unsure how the collective spectral signal of a pixel (including both snow- and non-snowy areas) that results in an ET value should be partitioned across the pixel. We hope that other future implementations of this approach improve estimates by incorporating these sub-pixel effects.

I also wonder if the necessary correction is not an artefact of the chosen evaporation product. The used soil evaporation and transpiration from the Penman-Monteith-Leuning Evapotranspiration V2 do not reach zero during winter and still reach values of 1-2 mm/d in Figure 2. However, one would expect that with snow and temperatures around zero, the transpiration and soil evaporation are zero as well. Especially as the chosen product also includes a band that accounts for snow and ice evaporation. And with zero transpiration/soil evaporation, the correction of the authors is actually unnecessary. So do you believe that with a different product, that already corrects in a better way for snowy days, this correction is actually needed? I might be worth looking at another product that uses a better correction for snow.

In California's snowy areas, including the Sierra Nevada (where we chose to highlight the method), it has been documented that ET can be far from negligible during times with significant ground snowpack and near-freezing temperatures. For example, Goulden and Kelley (2016; doi:10.1093/treephys/tpv131) documented relatively warm (above freezing) daytime upper canopy temperatures and significant ET (via flux tower measurements) during Dec-Feb in the Sierra Nevada that coincided with deep snowpack. During spring snowmelt, daytime temperatures are even warmer, resulting in relatively high ET, while snowpack persists. Thus, the ET product we use appears to be consistent with direct observations of tree water use during periods with snowpack. We agree with the reviewer that our proposed method would be unnecessary with a dataset that does not allow for ET during times with snow cover, but do not agree that such a dataset is necessarily better or accurate.

The updated manuscript will include these additional citations:

“Mountainous snow-rain transition zones can support high rates of ET and coincide with forested areas \citep{goulden2012evapotranspiration,hahm2014bedrock}, underscoring the importance of accurate estimates of S_R for prediction of forest sensitivity to climate variability in the future.”

Furthermore, I still have some minor comments in the list below. I hope the authors find my comments useful and I look forward to a revised version of the manuscript.

Minor comments

P1.L21-P2L28. I fully agree here, just note that the opposite is also true: estimates of soil water storage are made for the full soil column, whereas the volume of water that roots actually use may be smaller.

We agree.

P2.L31. Shift from snow to rain under a warming climate → This sounds like a statement that needs a reference.

Thank you for pointing this out. We propose to reference Knowles et al [2006] who demonstrate this ongoing shift in precipitation phase:

Knowles, Noah, Michael D. Dettinger, and Daniel R. Cayan. "Trends in snowfall versus rainfall in the western United States." Journal of Climate 19.18 (2006): 4545-4559.

P3.L54. I am not sure if I follow, aren’t in and out always opposite of sign?

We are pointing out that when tracking a storage *deficit via* change in deficit = dt*(F_{out} - F_{in}), the signs on the outgoing/incoming fluxes are the opposite from a typical storage tracking mass balance, with change in storage = dt*(F_{in} - F_{out}). We propose to add the following parenthetical to help clarify this point:

"(outgoing fluxes minus incoming fluxes for deficit calculations, as opposed to incoming fluxes minus outgoing fluxes for storage)"

P3.L60. Due to....is zero. → I think you need to clarify this, I misunderstood first. I guess you mean that precipitation is taken as zero in the method of Wang-Erlandsson et al., whereas in reality snow melt still enters the storage. Stated like this, it looks more like an overestimation.

We agree that the wording was a bit confusing. We propose to re-write as:

The potential inaccuracies introduced by this original method that we explore here are that, during periods when snowpack is present within the pixel, F_{in} may be non-zero due to melting snow entering the rooting zone, for example, or F_{out} from the root zone may be overestimated (due to attribution of sublimation/evaporation from the snow surface to a flux from the subsurface).

P3.L78. Distributed timeseries hydrological → distributed timeseries of hydrological

Thanks

P3.L78. Evapotranspiration → evapotranspiration

Thanks

P4.L90. C0 = 10%...snow cover dataset. → what do you mean? How can a percentage be a resolution?

Thanks for requesting clarification, we propose to change from 'resolution' to "the minimum non-zero value".

P4.L103. I would suggest to introduce your study site in the methods section.

We propose to move the short description of the study area to the end of the methods.

P4.87-89. How did you deal with cloud cover?

The underlying PML dataset employs multiple techniques for dealing with cloud cover, including interpolation and the use of historical data for gap-filling during cloudy days.

We will note in the revised manuscript that this ET dataset is corrected for clouds. For more details, see:

Zhang, Yongqiang, et al. "Coupled estimation of 500 m and 8-day resolution global evapotranspiration and gross primary production in 2002–2017." *Remote Sensing of Environment* 222 (2019): 165-182.

P6.L128-129. Globally...forested areas → reference?

Thanks - we propose to re-write as: “Mountainous snow-rain transition zones can support high rates of ET and coincide with forested areas \cite{gouldenEvapotranspiration along an elevation gradient in California's Sierra Nevada; Hahm2014PNAS}.”

Fig.2. Maybe also add precipitation here, to have D, Fin and Fout all together.

Good suggestion, we propose to add this flux (light blue, dashed lines) to the top plot in figure 2: