Review of **Technical note: Accounting for snow in the estimation of root-zone water storage capacity from precipitation and evapotranspiration fluxes** by Dralle et al.

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.

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.

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.

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.

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.
P2.L31. Shift from snow to rain under a warming climate → This sounds like a statement that needs a reference.
P3.L54. I am not sure if I follow, aren't in and out always opposite of sign?
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.
P3.L78. Distributed timeseries hydrological → distributed timeseries of hydrological

P3.L78. Evapotransipration → evapotranspiration
P4.L90. C0 = 10%...snow cover dataset. → what do you mean? How can a percentage be a resolution?
P4.L103. I would suggest to introduce your study site in the methods section.
P4.87-89. How did you deal with cloud cover?
P6.L128-129. Globally...forested areas → reference?
Fig.2. Maybe also add precipitation here, to have D, Fin and Fout all together.