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Comment on hess-2021-459

Andrew Feldman (Referee)

Referee comment on "Extrapolating continuous vegetation water content to understand sub-daily backscatter variations" by Paul C. Vermunt et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-459-RC1>, 2021

Vermunt et al. use non-destructive sap flow measurements to estimate the diurnal cycle of vegetation water content and then relate it to microwave radar backscatter. This paper is of high relevance to ongoing microwave vegetation measurements and answering large-scale ecosystems questions. I am in support of this work given the low amount of ground measurements and available techniques and consequently high uncertainty in microwave vegetation retrievals. It creatively uses a known application in plant physiology and ecohydrology for microwave remote sensing validation. I think the study is well done and is a great contribution. I ask that the authors consider some comments here before publication.

I do not wish to remain anonymous. -Andrew Feldman

Major comment

I think the methodology needs a clearer section or paragraph that explicitly outlines the method used here, its advantages and disadvantages, assumptions, and how the method can be modified in scenarios of different vegetation types (tree instead of corn). This could be a modification of Section 2. The sections afterward can expand on this as they currently do with section 3 and onward. While reading the methods, I felt as if I was finding out more components required for the method as it went along. It also seems like some steps are optional or can change for different types of vegetation (see my comments

below). Be clearer earlier that sap flow sensors, destructive sampling, and weather stations are needed and that this approach is somewhat specific for corn or other herbaceous vegetation types. Lines 54-55 motivate the method as a standard approach used in previous studies, but this method seems different because a sap flow sensor could not be placed in the crown and transpiration needs to be modeled. Furthermore, destructive sampling needs to be used to constrain the VWC estimates (though I am not sure this is always needed; see below). If a different vegetation type other than corn is used, the method can become more reliable because one can use two sap flow sensors and not have to model transpiration (other than relying on an additional assumption about small leaf capacitance). Since this is in part a methods paper, a more organized overview of the method can make the method more reproduceable or easier to modify.

Line specific comments

Line 21: Here or further down, an explicit definition of how vegetation water content is traditionally defined is needed. "Water content" can be confusing because it could be a total water volume (as is the case traditionally with VWC) or could mean a ratio to the dry or total volume (as for soil moisture or soil water content). Therefore, a definition of kg/square meter or other used here would be helpful.

Line 47-49: This is an excellent introduction. The main thing I feel that is missing is I am wondering if the authors could be more descriptive here of the other VWC in-situ sample options, how prevalent they are, and why they didn't choose them. A few things I am wondering: is the destructive sampling method the most common for radar validation? What specific destructive methods are used (oven drying leaves, branches, etc.)? Why not measure leaf/stem water potentials using automated psychrometers (Guo et al., 2019) since those sensors can provide rapid measurements (then mention why that does not directly provide water volume)? Have others used psychrometers or water potential measurements for radar validation? Another approach used in radiometry for VOD was to use water potential measurements and biomass to estimate VWC (Momen et al., 2017). Similarly, VOD was related to diurnal variations of leaf water potentials (Holtzman et al., 2021). I don't think the authors need to provide a large description of this (or cite these papers for that matter). I think it may provide more context and perhaps strengthen the motivation to choose the sap flow method by contrasting with other known options.

Line 52: A more specific research question/objective could be helpful here. This objective

has been broadly pursued before. The authors are specifically testing whether a non-destructive sap flow technique can measure VWC and thus be used to validate radar diurnal VWC measurements, which is a great endeavor that should be explicitly stated.

Lines 71-78: I became confused here because I thought in line 71 that this approach is applied here. Then I found out that it wasn't in line 79. Please only mention the assumptions applicable here to a single sensor and estimated transpiration. Then give more detail about the assumptions. You could argue that this approach circumvents the first assumption which could be flawed; the first assumption I think suggests that capacitance is negligible in the leaves and is larger lower in the canopy (trunks and lower parts of branches). This may not always be true for succulents and large trees. With the second assumption and full approach here, I wonder whether day to day variations can still be measured with this approach if a storage term is estimated and stem flow measurements are consistent.

Line 109-113: This paragraph appears to give some extraneous information. It might be helpful to only mention the measurements relevant to this study. The authors are not trying to minimize day-to-day weather variations here.

Line 118: For clarity, one sensor is placed at the base of the plant for each plant (as suggested by lines 79-86)?

Line 144-146: Consider showing an equation of this here.

Line 169: What time of day are these samples from?

Line 180: Since modeling transpiration can be viewed as the largest uncertainty of the method, I would add more details about how P-M equation was used and choices made for certain parameters (like roughness height and others).

Line 184: Using the data to constrain and validate here becomes somewhat circular. I think the method is generally fine. However, I would note that I don't think this step is entirely necessary – I think the authors can simply try to compare the temporal dynamics of the reconstructed VWC and measured VWC and not worry about correcting the bias too much.

Line 200: Is it true that the VWC_{t0} reference is needed to get the day to day dynamics right while the VWC_t (= sap flux – transpiration) term is all that is needed to explain the backscatter diurnal variations within a day? If so, I would be more explicit about this. This can be seen where, in eq. 3, the constant VWC_{t0} term would mostly get lumped into the y intercept term. VWC_{t0} are mostly a magnitude scaling and won't change the relationship between the VWC_t (= sap flux – transpiration) term and backscatter within a day. The VWC_{t0} is essentially picking up on the biomass and total water storage changes day to day. VWC_t is effectively the storage anomaly which is all that is needed to evaluate the backscatter anomaly. The consequence is that if one is only interested in the subdaily variations, the destructively sampled VWC_{t0} reference used to scale the VWC is not necessarily needed and is an extraneous step (this can be seen with using a panel regression in place of eq. 3 where the eq. 3 regression is effectively applied separately to each day's diurnal variations). If true, I think this idea should be mentioned. Perhaps the extra step to use destructive VWC sampling each day is to evaluate day to day changes in VWC. The point is that I think one can test the time dynamics of backscatter at large spatial scales using only sap flux and transpiration estimation without needing labor intensive, destructive methods to constrain the magnitude of VWC. If I am wrong, consider clarifying the issue in the text.

Line 235: I was worried about using P-M equation and CDF matching to sap flow to estimate transpiration because transpiration is very hard to estimate/measure. However, Fig. 5 shows this generally works well. It is stated somewhat indirectly, but I would emphasize clearly here or elsewhere that Fig. 5 shows that while modeling transpiration is a major drawback of the method, it works generally well in representing the VWC diurnal

cycle.

Line 245-247: Does this mean there is evidence that full rehydration does not take place overnight every day and that capacitance is large enough to have some storage deficit carry-over from day to day? And that the assumption to use sum of sap flux over the day does not hold (lines 144-146)?

Line 266: I think Fig. 8 is somewhat of a disservice to the authors and their nice results. The approach is well set up for sub-daily sampling, but ad-hoc modifications like CDF matching and VWC scaling are needed to represent day to day variations. I would say that the method is strong and well-developed for evaluating sub-daily VWC variations and a bit weaker for evaluating daily variations. In Fig. 8, my eyes are drawn more to the daily than diurnal changes which is not the focus of the paper and section heading. Consider showing diurnal variations individually for a few days (by segmenting individual days) to emphasize the results if possible.

Line 292: Is this unexpected that VV is more sensitive than cross-pol to vegetation?

Line 300: Dew is receiving increased interest in its impact on diurnal observations of microwave emission and backscatter. Can the authors contextualize the dew results in the table a bit more? It is hard to tell if "c" is a large or small contribution to the signal compared to "b" without knowing typical dew variations in kg/m². Maybe a variance-explained or normalized slope metric can help readers determine how much dew and internal water content relatively influence each backscatter signal. Only comparing the absolute slopes here does not fully show the relative contribution to the signals. It seems the authors are exhibiting less confidence in the dew results (i.e., lines 371-377) and it is not clear why (while the result in lines 366-370 are very interesting!).

Line 308: Arguably, the destructive samples may be optional here, especially for sub-daily variations, which strengthens the results.

Line 400: I think it is worth mentioning the sapfluxnet project and how one can use those data (along with station or flux tower data) to validate time dynamics of VWC seen by passive and active satellites at large scales.

I recommend commenting on whether such an approach here can be used to evaluate day to day variations in VWC. Are there ways in which the transpiration = sap flow scaling in the early morning can be relaxed such that total storage over a day can be computed and evaluated day to day (line 147-149 start to get at this)? I know this becomes uncertain due to phase lags between transpiration and sap flux caused by the capacitance that the method is trying to measure. But I think this study is a nice step towards that and the authors recommendations for how that can be done or recommendations against it could be helpful for the active and passive microwave vegetation community. If the authors feel that is off topic, feel free to ignore.

I also recommend commenting somewhere on how one could use such a method to validate satellite observations. What would be required in this case to estimate a diurnal VWC cycle at the footprint scale? It seems like a few meteorological measurements and sap flux sensors would suffice to at least understand the diurnal cycle.

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