Interactive comment on "Reconstructing Continuous Vegetation Water Content To Understand Sub-daily Backscatter Variations" *by* Paul C. Vermunt et al.

Reviewer comments in black
Reply to comments in blue

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

Response: Thanks to Andrew Feldman for the careful consideration of the manuscript and the constructive comments. Below we have addressed the comments in blue. The line numbers in our replies refer to the revised manuscript.

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.

Response: Thanks for the suggestions. We have reorganized the methods section based on your suggestions and those from the other two reviewers. We have structured the new Data and Methods section by separating the two objectives (section 3.1 and 3.2), and their corresponding data description. The adjustments of the methodology and corresponding data requirements are now grouped together and described before the details about data collection. We made a clearer distinction between a description of the original method applied to trees (section 2) and the specific modifications, assumptions and data requirements when this method is applied to corn (section 3.1). Please find answers on specific comments below.

Line specific comments

(1) 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.

Response: Agreed. We added a definition of VWC in lines 21-22.

(2) 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.

Response: We acknowledge that the suggested automated psychrometer and water potential measurements are very interesting, and potentially useful to add to the instrumentation in the field. Although water potential is the most useful descriptor for water status and from the plant hydraulic perspective, it does not have a one-on-one relation with dielectric constant and thus backscatter. The best possible measurements for validation of radar observations are therefore direct measurements of plant water content rather than water potential. Indeed, destructive sampling is the most direct and common method for measuring VWC for validation in agricultural terrains. For woody constituents in trees, dendrometers have been used to infer water content non-destructively after detrending, and similarly, reflectometry (TDR and FDR) and capacitance-style sensors have been used to derive water content indirectly by measuring dielectric permittivity (Konings et al. (2021)). However, as far as we know, these sensors were never used for non-woody tissue. We have addressed this in lines 54-58, which now read: "For woody constituents in trees, dendrometers have been used to infer water content non-destructively after detrending, and similarly, reflectometry (TDR and FDR) and capacitance-style sensors have been used to derive water content indirectly by measuring dielectric permittivity (Konings et al. (2021)). Moreover, a water balance-style approach using sap flow sensors have been used by the tree physiology community to estimate diurnal changes in tree stem water storage (Goldstein et al. (1998); Meinzer et al. (2004); Cermak et al. (2007); Phillips et al. (2008); Köcher et al. (2013))."

(3) 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.

Response: Lines 52-58 of the original manuscript were rewritten to make the objectives and used methodology more explicit, and now read: "The objectives of this study were to test the potential of a non-destructive sap flow technique for estimating sub-daily VWC variations in a herbaceous plant, and to use these estimates to better understand what controls sub-daily variations of L-band backscatter. Specifically, we adapted a methodology developed by the tree physiology community, described in section 2, to estimate 15-minute changes in corn VWC using sap flow sensors and a weather station. An extensive data set from a field campaign in the Netherlands in 2019 was used to evaluate the adapted method against diurnal cycles of VWC obtained by destructive sampling. Finally, the technique was applied to reconstruct sub-daily VWC variability of multiple consecutive days from another field campaign in Florida in 2018. In this campaign, high temporal resolution tower-based polarimetric L-band backscatter was collected. The reconstructed VWC was used, together with simultaneously collected soil moisture, surface canopy water (SCW), to gain better understanding of what controls sub-daily backscatter behaviour."

(4) 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.

Response: In the revised manuscript, we have made it clearer that the mentioned lines relate to previous research on trees, by (1) changing the subtitle of section 2 to 'Estimating diurnal variations in tree water content using sap flow probes', (2) adding 'In these studies on trees, ...' to the original line 71, and (3) moving the last paragraph of this section, which was related to the adaptations we made for a corn crop, to the reorganized methodology section. We think that mentioning the assumptions made by tree physiologists in section 2 is crucial to understand the adjustments required to apply the methodology to corn plants (section 3.1.1). We hope that the re-organization of the methodology section makes this clearer.

(5) 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.

Response: In response to this comment, we removed the 'minimize day-to-day weather variations' part. However, since it can be confusing that we used two data sets, we think it is still insightful to explain why the VWC data sets were different in the two campaigns. The reason that we used the 2019 VWC data set for evaluating the sap flow technique, was because we did not collect full diurnal cycles of destructively sampled VWC in 2018. The original lines 109-113 are adjusted and now read: "In contrast to the 2019 data set, VWC samples were not collected to capture the full diurnal cycle. Instead, these samples were obtained four times per week. Three days at 6:00, and one of these days also at 18:00, originally to capture differences between morning and evening passes for a sun-synchronous satellite such as SMAP (Entekhabi et al. (2010)). Moreover, the presented VWC data for 2018 are averages of eight plants instead of six. The samples were used to constrain the reconstructed VWC variations." The new line numbers are 234-240.

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

Response: Please find in lines 119-120 (original manuscript) that the sensors are 'wrapped around a corn stem, about 20 cm above the ground, ...'. Added 'near the base of the stem' for clarity to original line 118 (now line 154), which now reads "Sap flow was monitored near the base of the stem using stem-flow gauges produced by Dynamax Inc. (Houston, TX, USA)." .

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

Response: An equation is now shown in Table 1.

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

Response: Added 'spread throughout the day' to the sentence. Radar acquisition times are shown in the table below.

Table 1: Radar acquisition times (EDT)

01:00	07:00	13:00	19:00
01:30	07:30	13:30	19:30
03:00	09:00	15:00	21:00
03:30	09:30	15:30	21:30

(9) 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).

Response: We estimated transpiration from reference evapotranspiration (ETo) and sap flow data. ETo was calculated based on the sequence of equations presented in Zotarelli et al. (2010). ETo is the ET from a hypothetical, optimally growing grass reference crop, and was introduced to study the evaporative demand of the atmosphere independent of crop characteristics and management practices. Site-specific inputs are (1) weather station data (air T, solar radiation, wind speed), (2) field location (latitude, elevation above sea level), and (3) Day of Year. Hence, for the calculation of ETo, we did not have to make choices for crop-specific parameters like roughness height etc.

(10) 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.

Response: Indeed, our main objective here was to compare the temporal dynamics of the reconstructed VWC and measured VWC. We agree that for this purpose, we wouldn't need to calculate the RMSE for the five different samples. However, another sub-question we had was 'at which time of day should we sample if we would want to have the most realistic diurnal cycle of VWC?' To address this question, we needed to calculate the RMSE between samples and reconstructions, using all five samples once to constrain the reconstruction.

(11) Line 200: Is it true that the VWCt0 reference is needed to get the day to day dynamics right while the

VWCt (= 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 VWCt0 term would mostly get lumped into the y intercept term. VWCt0 are mostly a magnitude scaling and won't change the relationship between the VWCt (= sap flux − transpiration) term and backscatter within a day. The VWCt0 is essentially picking up on the biomass and total water storage changes day to day. VWCt 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 VWCt0 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.

Response: Indeed, VWCt0 won't change the relationship between VWCt and backscatter within a day. VWCt0 is indeed used for magnitude scaling. If we would not use VWCt0, the reconstructed diurnal variations would be the same. In equation 6 (former eq. 3), VWCt0 is not the destructive sample, but simply the reconstructed VWC at t=0, which is the first radar acquisition of the day (01:00). VWCt - VWCt0 simply describes the difference between VWC at time t and VWC at time t=0 (01:00). So the magnitude scaling of VWC does not affect the derivation of the b-parameter in eq. 6.

The reason it is valuable to use our destructive samples for scaling in Figures 8 and 9, is to be able to evaluate whether the diurnal cycles are more or less reasonable. Due to the destructive samples, we for example know that the reconstructions on June 9 and/or 10 would lack refilling in the evening if we wouldn't use the weighted average between forward and backward reconstruction, since significant gaps arise between reconstructions and samples (see Fig. 8). If a multi-day period is analyzed, such as in Figures 8 and 9, we advise to still use regular destructive samples for scaling, particularly for a fast-growing crop like corn.

(12) 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.

Response: Addressed this in the Discussion, lines 374 - 375, which now read "While the indirect estimation of transpiration could be considered a drawback of the method, Fig. 6 has shown that the diurnal VWC cycle was represented generally well."

(13) 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)?

Response: Indeed, this shows that the assumption that all withdrawn water is exactly replaced within 24 hours does not hold for most days. There can be several explanations. From our predawn destructive samples (depicted in Fig. 2, 8 and 9), we for example know when there are increasing VWC trends as a result of growth, and decreasing VWC trends as a result of senescence. In these situations, the 24-hour assumption does not hold. Similarly, the effect of a dry period can result in decreasing VWC trends, related to hydraulic capacitance of the plant. In this case, the 24-hour assumption does not hold either.

(14) 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.

Response: Thanks for the suggestion. We have added Figure 1 below to the manuscript, showing zoomed-in diurnal variations of June 7, 9 and 11.

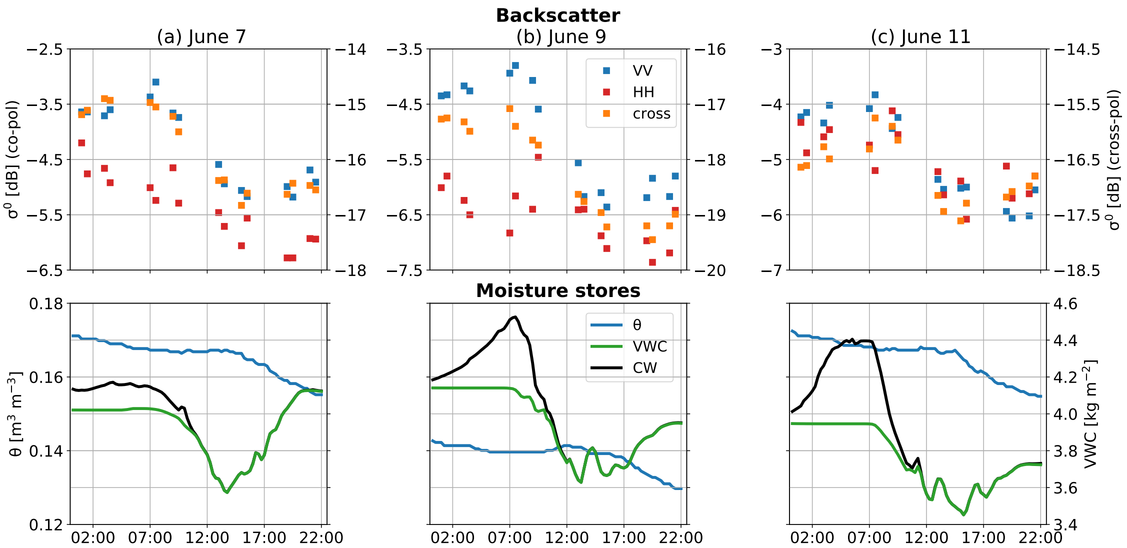

Figure 1: Zoomed-in diurnal variations of backscatter and moisture for June 7, 9 and 11 (2018).

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

Response: Note from Table 2 that the differences in sensitivity to VWC between VV and cross-pol are small. On June 9 for example, VWC changed with 0.5 kg m^{-2}, which would translate to a change of 1.5 dB (VV), and 1.2 dB (cross), if soil moisture and SCW would be stable. With the (inputs of the) regression not being perfect (see discussion), one should not put too much weight on this difference. The modelling study in Vermunt et al. (2020) showed that both VV and cross-pol backscatter were dominated by the vegetation/volume scattering contribution in this period. Given that most of the water is in the stems (Vermunt et al. (2020)), which are vertical structures, it is not surprising that vertically polarized backscatter is sensitive to dynamics in the vegetation.

(16) Line 300: Dew is receiving increased interested 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!).

Response: Maximum diurnal dew estimates ranged from 0.04 to 0.46 kg m-2 in this period (Figure 9), with an average of 0.24 kg m-2 (Figure 11). Note that these are estimated quantities (Vermunt et al. (2020), and are similar to findings from Kabela et al. (2009). In lines 353-359, we describe how coefficients for soil moisture, VWC and dew translate to changes in backscatter for a typical dry day during the campaign of 2018 (June 9). It is true that we exhibit less confidence in the regression coefficients for dew, compared to those for VWC and soil moisture. First, this is because when we visually inspect Fig. 12, we see that nocturnal increase (as a result of dew) is barely visible, while variations due to VWC and soil moisture are represented quite well. This suggest that the 'c' coefficient underestimates the effect of dew on backscatter. Second, Table A4 shows that, for all polarizations, the P-values for SCW are higher than those for VWC and soil moisture. Nonetheless, with the exception of HH ($P>|t|$=0.286), all P-values for SCW are < 0.05, indicating statistical significance. So yes, we are confident that the regression is largely reliable, but we think that the effect of dew is underestimated. We think this could be improved if the estimates of SCW (and VWC) improve. Besides, SCW is not considered in backscatter models yet, so the relationship between dew and backscatter is not well known. We agree that this is a topic that requires urgent attention, and we are working towards including SCW in EM models. Addressed in lines 443-448, which now read: " However, it seems that the SCW coefficients (c) for VV and cross-pol in Table 2 underestimate the effect of dew on backscatter, as the nocturnal increases in calculated σ^0_{VV} and σ^0_{cross} in Fig. 12 are lower than observed. This could partly be addressed by improved SCW estimates, for example through inclusion of more leaf wetness sensors distributed in the canopy (Vermunt et al. (2020)). Moreover, additional research is needed to provide more insight into the scattering mechanisms under the presence of SCW, for example by considering SCW in physical backscattering models. "

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

Response: Agreed. Changed sentence to "Our reconstruction results confirm that it is possible to estimate 15-min variations in corn VWC with only sap flow sensors and a weather station."

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.

Response: Thanks for the suggestion. Addressed this, together with the suggestion in comment (20) in lines 470-476, which now read: "As radar observations are increasingly used to study plant water status, the presented sap flow method is a promising way to validate sub-daily satellite observations with just meteorological data and sap flow sensors, without laborious sub-daily destructive sampling. The method is expected to be most robust when the temporal resolution of the sap flow and ET observations are significantly smaller than the phase difference between the two, which depends on the species. The number of sensors required to capture VWC variations at footprint scale is expected to depend on the footprint size, and the spatial heterogeneity of vegetation type and factors influencing moisture supply and demand. Potentially, global database networks for sap flow measurements, i.e. *Sapfluxnet* [1], and flux tower measurements, e.g. *Fluxnet* [2] and *Ameriflux* [3] can play an important role here."

[1]http://sapfluxnet.creaf.cat
[2]https://fluxnet.org/
[3]https://ameriflux.lbl.gov/

(19) 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.

Response: The use of multiple days for rescaling transpiration already allows for day-to-day variations. This means that total storage over a day can be estimated. However, Fig. 9(d) shows that the diurnal VWC reconstructions are discontinuous between consecutive days. The gaps indicate that the method is not perfect, and we recommend to test the suggested adjustments (lines 404-412) to improve the method before using it for the purpose of evaluating day-to-day variations in VWC.

(20) 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.

See response on comment (18).

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