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

This paper provides an update on a previous analysis (Vermunt et al. 2020) of microwave radar data taken in Florida in 2018 over a corn field. In this previous paper, the authors have identified a diurnal cycle in backscatter which may be related to changes in vegetation water content (VWC). However, validating this hypothesis requires sub-daily measurements of VWC changes which are notoriously hard to obtain. The authors thus present a technique to reconstruct daily changes in VWC from a combination of sapflow measurements and weather-station based estimates of evapotranspiration. They evaluate this technique against a set of sub-daily destructive VWC samples taken in another location. The technique is then applied to the 2018 Florida data and used to demonstrate that sub-daily changes in backscatter are consistent with the reconstructed diurnal variability in VWC (in addition to surface canopy water and soil moisture).

Considering what the authors aim to achieve, the study set-up and the available measurements are not 100% ideal. The absence of more reliable ET data (i.e. from a flux tower) is a bit unfortunate, as is the fact that only few days have all types of measurements available. Contrary to what may be thought from the title, the proposed technique is not able to entirely reconstruct VWC variability, rather it can be used to extrapolate sub-daily VWC behavior from a single measurement (made daily, for example in the morning). Still I believe this to be a very useful attempt, especially if one focuses on sub-daily variability alone, and it may guide future similar research. There is certainly an interest in reconstructing sub-daily VWC from fewer of the time-consuming destructive samples.

Response: Thanks 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.

I have a few comments below which I think need to be considered, followed by some more minor comments and suggestions.

Major comments —

(1) Figure 10. The presentation of this figure is a bit misleading. If I understood correctly, the regression only attempts to predict intra-day variability in backscatter (Eq. 3). The initial backscatter value for each day is not reconstructed, but taken from the measurement directly. This is why there is a perfect match between 'observed' and 'calculated' at the start of each day. This should be made much clearer so as to not give the impression that the substantial inter-day variability in Fig. 10 can be explained from the regression. In fact, the quality of the regression for intra-day variability remains to be demonstrated as the authors do not report it (neither do they report if the coefficients of the regression are statistically significant).

Response: We agree that this was not clear enough in the manuscript. The observations used to constrain the predictions of sub-daily σ^0-variability , σ^0_{t0}, are now accentuated with open markers in Figure 12 (former Figure 10), and a description is added to the caption. Table A4 has been included to provide the reader with additional details on the regression and the statistical significance of the coefficients. The following text has been added regarding Figure 12, in lines 362 - 364: "The P-values for SCW are always higher than those for VWC and soil moisture. Nonetheless, with the exception of the SCW coefficient in the case of HH-backscatter ($P > |t|$=0.286), all P values are < 0.05, indicating statistical significance."

(2) In view of this, it's hard to tell if the regression is actually reliable, especially since much of the sub-daily variability in backscatter doesn't seem to be well predicted in Figure 10 (but it's hard to evaluate). Showing a scatter plot of the measured vs predicted sub-daily variations would be more informative in that respect.

Response: The R^2 values in Table A4 show that 68-71% of the variance in backscatter is explained by the three predictors. In addition, the P-values (with the exception of SCW and HH-pol) indicate that the regression is reliable. See previous comment.

(3) One could also make it clear which points are the ones that are used as the "anchor points" at t_0, for instance by giving them a different symbol color or shape.

Response: In response to your suggestion, we have changed the observations at t_0 into open markers, and added a description in the caption.

(4) Also the data in Figure 9 d-e-f provides the opportunity to better illustrate the modeled diurnal impact on backscatter (and compare it against the data in panels a-c). The contributions of all variables are mixed up in Figure 10, so it's difficult to learn much from that figure alone.

Response: We do not think that the data in Fig. 11 d-e-f (former Fig. 9) would better illustrate the diurnal impact from VWC, SCW and soil moisture on backscatter than the data used in Fig. 12 (former Fig. 10). We can discern the same periods from Fig. 11 in Fig. 12, but for all days individually. Fig 9 (d-e) can be used to analyse periods in detail. Using Fig. 12 in its current form will point a reader to some other interesting features too, such as the representation of backscatter increase after rainfall (June 8, 10 and 12) and the impact of a poor VWC reconstruction (e.g. June 12) on the backscatter simulations. Therefore, we chose to not replace Fig. 12.

(5) Section 3.2.3 is a bit difficult to read because the purpose or context of some new methods that are explained there only becomes apparent or fully understandable later in the paper. Maybe there is potential to reorganize this section a bit and potentially already illustrate the different approaches with a figure (Figure 4 provides some of that but too late for the reader). In general, the methods (when they document a new approach) seem a bit excised from the rest of the text. It wouldn't hurt to give a bit more meat to it, for instance by providing a figure to explain the reconstruction method in 3.3. as well (for instance, Figure 4 does that well for CDF-matching).

Response: In response to this comment, as well as comments from the other two reviewers, we reorganized the methodology section. In the new *Data and Methods* section, we have moved the text related to rescaling ETo from former 3.2.3 to new *3.1.1: Adjustments of the methodology*. In this new section, we merged all adjustments of the methodology presented in section 2 to make it applicable to corn. The different approaches to estimate transpiration are highlighted in a new table: Table 1. Moreover, we added an extra panel to Fig. 5, which shows the effect of the three approaches on the transpiration estimate. We chose not to add Figures containing our data at this stage of the paper, because the data collection is described in section 3.1.2. Instead, we added a high-level summary of the steps taken to reconstruct diurnal VWC cycles in the last paragraph of section 3.1.1, which now reads: "In summary, we adapted and evaluated the sap flow methodology to estimate diurnal cycles of corn VWC through the following three steps.
① The diurnal cycle of transpiration was estimated from ETo and sap flow data, using three different ap-

proaches (Table 1).
② Sub-daily variations in VWC were estimated by calculating the cumulative difference between 15-minute basal sap flow and transpiration estimates (eq. 1).
③ The resulting estimates of diurnal VWC variations were compared against destructive measurements of VWC." Please see also the reply on comment (10) for details about the different approaches to estimate transpiration.

(6) Are there any downsides to CDF-matching? You force the T rates to follow the same distribution as the sap-flow rates. Is there any evidence that this is may or may not be true in papers comparing transpiration and sap flow measurements? I think it's fine to test this method, but the implications and plausibility should be better discussed. For instance, there is a physical rationale for having a long-term balance between sap flow and T rates that justifies the 24-hour (or more) sum approach.

Response: The long-term balance between sap flow and T rates still holds for CDF-matching. That is not different from the linear approaches. See from the example in Fig. 1 below (in response to comment 10) that the distribution of ETlinear3d and ETcdf3d is quite different, but the 3-day sums are 17.04 mm and 17.07 mm, respectively. Based on the plant hydraulics theory described in section 2, lines 72 –75, it makes more sense that sap flow follows transpiration with some time lag, with similar peaks and during a similar period of time (e.g. Fig. 4(e)). From this point of view, there is no physical rationale for the distribution of the linear approach, with sap flow having much higher peaks than transpiration, during a shorter period of time (e.g. Fig. 5(b)). In fact, earlier experiments suggest that the diurnal distribution of sap flow and transpiration are actually quite similar (Miner et al., 2017). This is something we tested through CDF-matching, and it turned out that CDF-matching gave the best fit between sampled and modelled diurnal VWC cycles.

Minor comments —

(7) Title: because the proposed method still requires some daily VWC measurements as constraints. I wonder if "Extrapolating continuous vegetation water content . . . " would be more appropriate and a better description of the paper's contribution. Alternatively, you could put the emphasis on sub-daily ("Reconstructing diurnal vegetation water content..."), which does not need daily VWC measurement as constraint if one focuses on anomalies.

Response: Agreed. Changed title to 'Extrapolating Continuous Vegetation Water Content to Understand Sub-daily Backscatter Variations'

(8) L49: unavoidable suggest to replace with acceptable

Response: Agreed. Changed to 'acceptable'.

(9) L83: Was a bit hard to get on first read. Maybe modify the sentence into: ". . . lag between transpiration and upper sap flow, compared to the lag with basal sap flow, . . . ".

Response: Agreed and modified.

(10) L145-155: It may be useful to provide an illustration of the time series (before and after correcting ET with these different processing options) as a supplementary figure. Right now, it is a bit difficult to visualize what is happening to the ET time series. By the way, even if P-M ET was a perfect method and produced close to truth ET time series, you'd still need to separate the plant transpiration part from the soil evaporation part. My point is that the "correction" actually also serves to do that operation.

Response: Figure 1 below (also added to supplementary materials) gives an example of the effect of the three rescaling methods. What stands out is that the cdf-matched rescaling provides significantly higher peaks, compared to the linear rescaling. But in this case, the 3-day sum of ET_{cdf-3d} is not that different from the 3-day sums of $ET_{linear-24h}$ and $ET_{linear-3d}$: 17.07 mm, 17.35 mm, and 17.04 mm, respectively. This is because below 0.12 mm/15min, cdf-matched ET is lower than linear rescaled ET. An extra panel (a) was added to Fig. 5 in the manuscript, which illustrates the effects of the three approaches to estimate transpiration from ETo and sap flow. Explanatory text was added to section 4.2, which reads: "Fig. 5(a) illustrates the effects of the three approaches to estimate transpiration from ETo and sap flow (Table 1). T-cdf and T-3d represent the CDF-matched and linear estimates of transpiration, for which 3 days of data were used: July 24-26. What stands out is that the CDF-matched rescaling (T-cdf) provides a significantly higher peak, compared to the linear rescaling (T-24h and T-3d). On the other hand, when ETo rates are 0.09 mm 15min^{-1} or lower, T-cdf was lower than the linear estimates. Both linear transpiration estimates were close in this particular case, which means that the ratio of the 24h sum of sap flow over ETo was close to the ratio of the 3-day sum of sap flow over ETo."

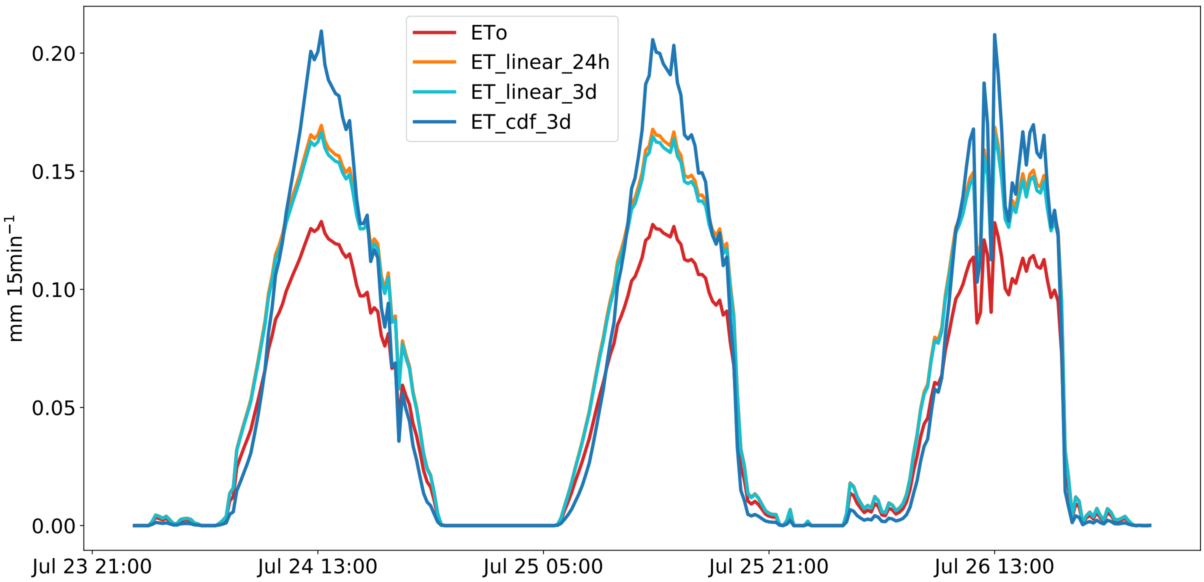

Figure 1: ETo and the three rescaling approaches for July 24-26.

(11) L153: I thought on first reading that CDF matching was done with the daily totals (not the sub-daily time steps). This may need to be mentioned here.

Response: Addressed this by changing the sentence to 'This matching was achieved by first ranking all 15-minute data from both data sets from low to high values, ...'

(12) L155: It could be useful to give a final high level summary of what happens here. For instance: "information on the diurnal shape of ET is entirely derived from Penman-Monteith, but the ET daily totals

are scaled so that T estimates that are consistent with sap flow over long periods of time".

Response: Added "This means that information on the diurnal shape of ETo is derived from the Penman-Monteith equation, and that these ETo estimates are then scaled so that the resulting transpiration estimates are consistent with sap flow over a given period of time." to updated lines 113-114.

(13) Equation 2: I think the notation is not appropriate (or at least it is very unclear to me). I think I understand what you did in the end, but the equation does not reflect it: Is "k=15 minutes" meaningful here? The lower position should indicate the starting point (i.e. k = t_0, or k = t_0+15 minutes), check for instance: http://www.columbia.edu/itc/sipa/math/summation.html In Fk and Tk, does k denote the start or the end of the 15 minute time period? Why multiply (Fk – Tk) by Δt, if Fk and Tk are already expressed in per 15 minute rates? (I assume Δt would equal 15 minutes, since t and t_0 are indicated to be expressed in minutes).

Response: Thanks you for pointing out this mistake. This is addressed in lines 80-82, which now read: "

$$VWC(t) = VWC(t_0) + \sum_{i=t_0}^{t} (F_i - T_i)\Delta t \quad (1)$$

, where $VWC(t)$ is the estimated VWC at time t, $VWC(t_0)$ is a reference VWC at t=0, F is basal sap flow, T is whole-crown transpiration, both in mass per unit of time, and Δt is the duration of a time step."

(14) L188: Why these 10 days in particular?

Response: In the reorganized methodology section, this is explained in lines 199-200, which read "The longest period for which we had all data available was from June 4 00:00 to June 13 10:15.", and in lines 241-242, which read "The period of consecutive days for the analysis was limited by the availability of sap flow data. A 10-day time series was found in mid-to-late season which contained continuous sap flow and weather data, L-band backscatter, and five sampling days."

(15) 192: "did not overlap". I don't understand what this means. Do you simply mean, if they are not equal to each other?

Response: Indeed. Re-phrased the sentence, which now reads: "In case there was a gap between forward and backward reconstructions,...", see line 245-246 of the revised manuscript.

(16) L200: So this expression allows for an investigation of the sub-daily dynamics and basically removes the potential inter-day differences (since all data is relative to t_0). Maybe this should be stated more explicitly

Response: This is clarified by adjusting the text around the expression, which now reads: "The separate effects of the three different moisture stores on sub-daily backscatter (σ^0) variations were quantified through multiple linear regression. The relation between sub-daily backscatter variations and changes in these dynamic moisture stores was described by:

$$\sigma^0(t) = \sigma^0_{t0} + a(\theta_t - \theta_{t0}) + b(VWC_t - VWC_{t0}) + c(SCW_t - SCW_{t0}) \quad (2)$$

,where t_0 is the first radar acquisition time of the day (01:00), and assuming linear relations between σ^0 and the individual moisture stores. The regression coefficients a $[dB/m^3m^{-3}]$, b $[dB/kgm^{-2}]$, and c $[dB/kgm^{-2}]$ were used to quantify the change in backscatter within a day as a result of change in moisture, and were derived for each polarization separately. ".

(17) L219: It is unclear what is meant by "the linear estimate". I guess this means the scaling to match the 24-hr totals. Maybe section 3.2.3 needs to be better structured. You could potentially make a quick list of the different methods which you are testing and comparing.

Response: We indeed referred to the scaling to match the 24-hr totals as 'the linear estimate'. In the reorganized methodology section, we included a table (Table 1), which gives a clear overview of the three methods we compared and tested, including their assumptions and equations.

(18) L227: "observed [on that day] from"

Response: Added 'on that day' for clarification

(19) Figure 4. It is assumed that ET estimates need correction to maintain some balance between transpiration and sap flow, but what about biases in sap flow measurements for high rates of flow? Are they possible and how big could they be?

Response: Biases in the presented sap flow measurements for high rates of flow are unlikely, because first of all, the sensor installation with shield and proper insulation limits thermal noise from radiation or other effects. Moreover, the Dynamax programme uses a built-in high flow-rate filter to prevent a distortion of the accumulated flow over those rates that are reasonable (Dynamax, 2007). Possible extraneous observations from a single sensor in 2018 are levelled out by averaging four sensors.

(20) L242: "An exception to this rule was July 25, when all available data for the CDF-matching were used." I don't understand why this is an exception, which sample was used as a constrain there then?

Response: This sentence was omitted in the revised manuscript.

(21) Figure 5. In each time series, it would be useful to show with a different symbol the one sample VWC that was used as constrain.

Response: Agreed. We changed the symbol for the measurements which were used to constrain the reconstructed lines in Figures 5, 7 and 8, and included explanations in the captions.

(22) Figure 5. This Figure shows well how the 24-hour method does not allow for a difference between the start and end-of-day VWC. Could be mentioned.

Response: This is addressed in lines 299-301, which read "The upper row clearly shows that the linear-

24h approach does not allow for a difference between the start and end-of-day VWC, while the inclusion of multiple days does."

(23) Figure 5. Unlike the other days, Aug 23 had a lot of dew, so it could be that the VWC measurements were biased up because of that (one can remove dew with paper towels only on the accessible parts of the plant). This would explain why the reconstruction has a hard time for that day.

Response: Thanks for the suggestion. However, we do not think this can explain why the reconstruction is poor on Aug 23. Our sampling protocol involves removing the whole plant from the field, separating the leaves and stems and removing all dew with paper towels. So, we are confident that there is no bias due to residual dew due to inaccessibility. Furthermore, residual dew would lead to an overestimation of VWC. This would then particularly hold for the measurement at 6:30, because at 10:00 all dew was dissipated. A lower VWC at 6:30 (or even 10:00), would still result in a different shape of the sampled diurnal VWC cycle compared to the estimated diurnal cycle.

(24) L264: "see fig 4d". It's hard to understand how this relates to what is being said. This could be better explained.

Response: This was an error. It should have been a reference to Figure 8(a) (former Fig 7(a)), which shows that ET on June 6 is markedly different to that on the other days. For clarity, the sentence is reformulated in lines 316 - 318 , and now reads: "Despite the advantage of CDF-matching, opposed to linear conversion, to better reflect diurnal extremes, the anomalous dynamics of June 5 and 6 are not captured sufficiently.".

(25) Figure 9. It would be useful to show some +/- 1 std deviation error bars (or envelopes) around the averaged data.

Response: Please see Figure 2 below for the mean +/- 1 standard deviation. We think the added value of the standard deviation is low, and including them in the Figure will negatively affect the readability of the Figure. Therefore, we decided against adding them to the Figure.

(26) L285 typo

Response: Done

(27) L298: Is it 3 times more if the units are dB ? (and same later)

Response: Rephrased sentences with this statement here, and later, based on a comment from another reviewer. This specific sentence (lines 356-359) now read: " This indicates that on this typical dry day, a diurnal variation in VWC leads to an almost four times higher change in VV-polarized backscatter [dB] than a diurnal change in soil moisture does. On the same day, the changes in HH- and cross-polarized backscatter [dB] were two times higher for the diurnal VWC variations than for the soil moisture drydown."

(28) L301: I don't understand why (where?) Fig. 10 would show that. Please indicate what you mean

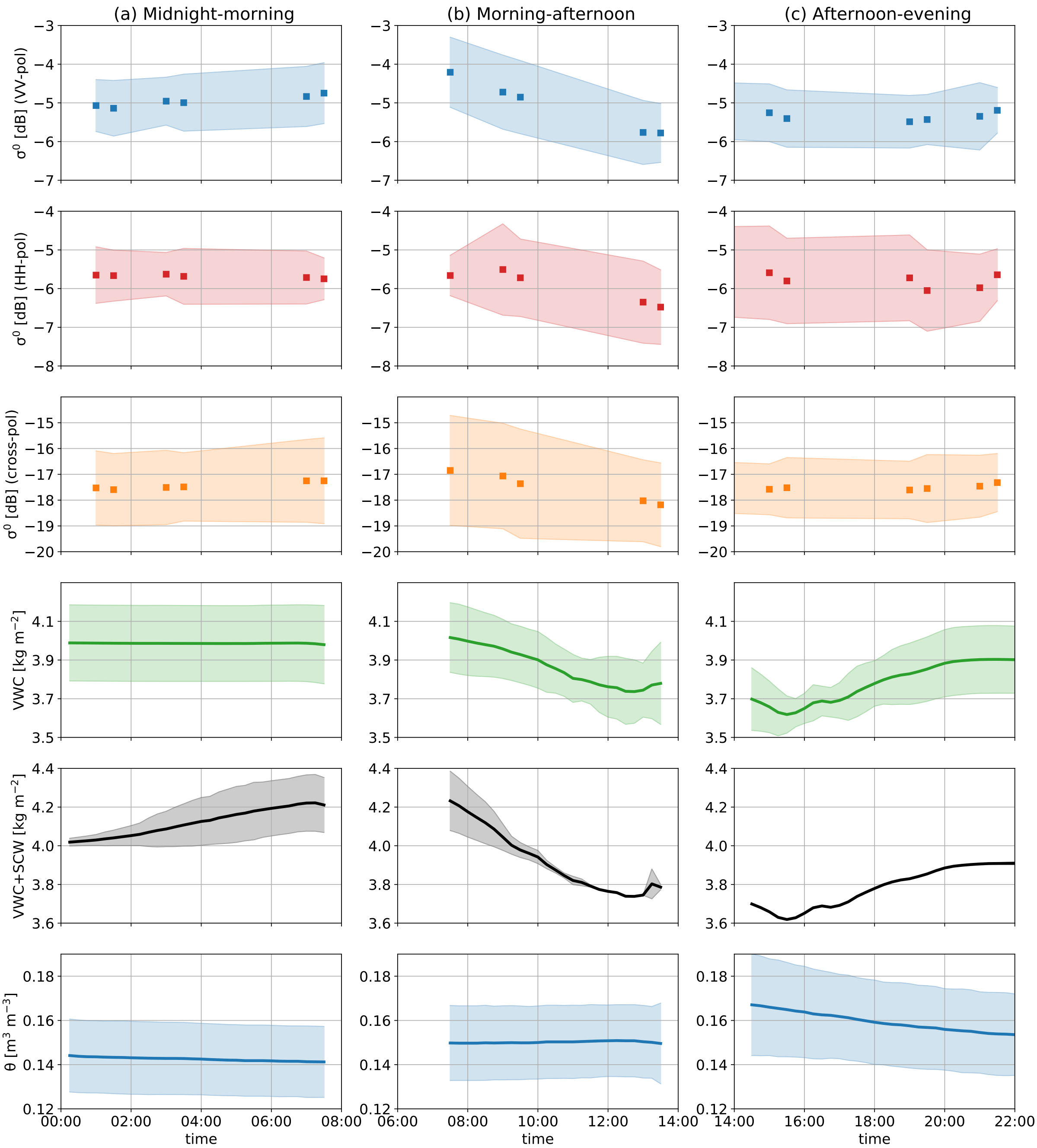

Figure 2: Mean and standard deviations of backscatter (VV, HH, cross-pol), VWC, SCW, and soil moisture for the periods described in Figure 11.

about Fig. 10 more clearly.

Response: When we look at VV-pol backscatter in Fig. 12 (former Fig. 10), we see that each night observed backscatter increases until the 7:30 acquisition (except from June 12). We have seen before that this increase can be attributed to dew formation, because VWC and soil moisture do not increase in these periods. Meanwhile, the calculated backscatter stays stable or only increases slightly. Similar patterns can be observed for cross-pol. This suggests that the regression results underestimate the effect of SCW on backscatter. Rephrased this sentence in lines 364 - 366, which now read " However, note from Fig. 12(a) and (c) that the observed nocturnal backscatter increase as a result of dew formation is barely visible in the calculated backscatter. This suggests that the regression underestimates the effect of dew on backscatter. ".

(29) Table1: Was the significance of the coefficients tested? Please report if they are statistically significant, their confidence interval, and what is the overall performance of the regression.

Response: In the revised version of the manuscript, we included Table A4, which shows the results of the multiple regression analysis (see also responses on comments (1) and (2)).

(30) L310: "of dew" => "that dew"

Response: Changed 'of' to 'that'

(31) L317: "This is comparable to estimated dew evaporation in this period, which was 0.09 kg m2". Can you explain where this estimate comes from?

Response: See Fig. 7. The black line in the bottom left figure represents dew on July 25. The peak at 6:00 is 0.09 kg m-2 (see y-axis on the right). At 8:15, all dew was evaporated. Added "(Fig. 7)" to line 385.

(32) Does the temperature of the canopy water or of the soil water have any possible impact on backscatter and if yes, could it explain some of the diurnal variability?

Response: For the range of temperatures observed, the primary driver of variations in dielectric constant (of soil or vegetation) is water content, with water chemistry and temperature being of secondary importance. Temperature becomes highly significant when the water in the plant or soil freezes as the water is bound rather than free, resulting in a sharp decrease in dielectric constant as the temperature goes below freezing (Schwank et al. (2021); El-Rayes and Ulaby (1987)). A preliminary analysis of the data provided by El-Rayes and Ulaby (1987) (Figure 11) suggests that VWC effects dominate. However, the figure assumes that the sample did not dry out during the period in which the temperature was changed, and has few data in the temperature range we observed. We are not aware of experimental datasets that consider both temperature and moisture variations in vegetation, so this is something that warrants attention as sub-daily microwave data become available. Based on the results of Schwank et al. (2021), temperature seems to primarily become significant when freezing occurs.

(33) L340-345: Yes I think most of the re-scaling approaches you presented here would still be potentially needed to get from measured ET to T.

Response: Agreed. However, we expect that when diurnal E variations could be excluded from the ET measurement as much as possible, one can get better estimates of diurnal variations in T, and potential errors after re-scaling with sap flow may be smaller.

(34) L350-355: This is based on the fitted coefficients but it's not clear if these are actually significant.

Response: See response on comment (1).

(35) L357: I agree that it is a credible interpretation of Figure 9, however, I think it would be more convincing if a physical model of backscatter was there to demonstrate that both effects are indeed of similar magnitude and can cancel each other. But I guess this would also mean adding a whole new section to the paper..

Response: We agree that the use of a physical model is potentially a convincing tool to demonstrate these opposite effects on backscatter. However, widely used physical models are generally developed and calibrated based on seasonally variant VWC only. As a consequence, the effect of sub-daily VWC variations on backscatter are not captured very well (and SCW is not included at all). We are certainly keen to adjust physical models in such a way that they can handle both internal VWC and surface canopy water on sub-daily timescales. However, this is sufficiently complex to warrant a separate manuscript by itself.

(36) The conclusion makes a good summary and some good points on why the research is relevant, good job! It would also be interesting to read the authors' perspective on what type of future work would be needed to achieve better comparability between in-situ microwave data and eco-hydrological observations. In particular, it seems that when it boils down to sub-daily variability only, the time lag between sap flow and the transpiration estimate will control most of the VWC cycle. If it's really the case, the authors may provide some recommendations on the needed temporal resolution (already touched on L335, but could deserve more space).

Response: Thanks. We addressed this 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."

References

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