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## **Comment on hess-2021-459**

Anonymous Referee #3

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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-RC3>, 2021

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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.

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

Major comments ---

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).

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.

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.

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.

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).

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.

Minor comments ---

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.

L49: « unavoidable » suggest to replace with « acceptable »

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, ...".

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.

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.

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".

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  $k = t_0 + 15 \text{ minutes}$ ), check for instance:

<http://www.columbia.edu/itc/sipa/math/summation.html>

- In  $F_k$  and  $T_k$ , does  $k$  denote the start or the end of the 15 minute time period?
- Why multiply  $(F_k - T_k)$  by  $\Delta t$ , if  $F_k$  and  $T_k$  are already expressed in per 15 minute rates? (I assume  $\Delta t$  would equal 15 minutes, since  $t$  and  $t_0$  are indicated to be expressed in minutes).

L188: Why these 10 days in particular?

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

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

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.

L227: "observed [on that day] from"

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?

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?

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.

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.

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.

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

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

L285 typo

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

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

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.

L310: "of dew" => "that dew"

L317: "This is comparable to estimated dew evaporation in this period, which was 0.09 kg m<sup>-2</sup>". Can you explain where this estimate comes from?

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?

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.

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

significant.

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..

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).