Response to comments by Referee #3

The authors present an experimental technique using GNSS that can be used to provide continuous VOD estimates. The paper is well-written and the level of detail provided is generally appropriate. Given that the paper is likely to be read by a wide audience, many of whom are not familiar with GNSS, I would recommend including some additional details (see below). The technique and methodology presented have the potential to be hugely valuable for the microwave remote sensing community, particularly those concerned with observing soil and vegetation. The authors provided valuable recommendations on the deployment of similar set-ups, and outline several potential applications. This paper is highly innovative, timely and can be expected to have a significant impact in the field of hydrology and remote sensing. To my knowledge, the theory and methodology are sound. I recommend that it is accepted for publication in this special issue if the following comments can be addressed.

Thank you for the careful evaluation and the constructive review. We provide our point-by-point answers below.

Major comments:

In lines 554-556, and the discussion in lines 569-589, AGB and CWC are used to refer to the total aboveground portion of the vegetation, including leaves, branches, trunks etc.. However, as discussed in lines 523-531, observations and modeling studies have shown that L-band transmissivity is primarily sensitive to leaves. This suggests that the GNSS VOD produced here is primarily sensitive to leaves and that the dynamics observed in GNSS VOD are primarily due to variations in leaves with the sensitivity to branches and trunks depending on leaf moisture content. The same is also true for other L-band VOD products. Nonetheless, I think this should be mentioned in lines 565-576 as it provides some explanation for the difference among the estimates based on the models of Vitucci and Brandt. It also serves as a caution to users on the interpretation of AGB derived from VOD.. It is also relevant for the discussion of CWC because (1) the CWC is calculated using the estimated AGB and (2) the definition of "canopy", in the sense of which constituents are observed, varies depending on leaf moisture content - the dynamics in this CWC are expected to be primarily due to leaf water dynamics.

Thank you for this comment, this is a very good point. We add the following statements:

L567: "Note that this estimate should be interpreted with the awareness that VOD-based estimates of AGB likely do not weigh all canopy constituents evenly. While L-band VOD is primarily sensitive to leaves, the sensitivity to branches and trunks can also increase at lower leaf moisture content (Steele-Dunne et al., 2012)."

L578: "As for AGB, it's important to keep in mind that the CWC estimate does not weigh all canopy constituents evenly"

Define what is meant by canopy in the paper. Is it used to mean the aboveground portion of the vegetation? The portion above the sensor? Or the upper layer of the forest? It is important to be clear here because the paper is likely to be read from both the remote sensing community as well as the forest ecology community. This is also relevant in the context of the discussion above regarding canopy water content.

Thank you for raising this point. We now define canopy explicitly:

L115: *"Here, canopy is understood as the portion of vegetation lying above the sensor (in our case, this excludes the forest floor and ground vegetation)."*

Lines 107-110: I would not use "proxy" here. GNSS-VOD should not be considered a direct proxy for biomass or leaf water status. The current formulation suggest that the relationship between GNSS-VOD and biomass and leaf water status is more direct than it is. It is fine to say that GNSS-VOD could be useful to interpolate and gap-fill sparse and labour-intensive measurements of biomass and leaf water status but there many assumptions and models needed between the two.

We fully agree with the reviewer that there is a long way from GNSS-VOD to these quantities. However, we believe the term of "proxy" is warranted, considering the current practice in other studies on VOD, for instance in the same journal [e.g. Mucia *et al.*, 2022; Schmidt *et al.*, 2022].

At L108: we replace "… proxy …" with "… indirect proxy …"

Section 3.1: For readers not familiar with GNSS, it would be helpful here to provide some description of how the data shown in Figure 3 are obtained in terms of satellite overpasses, viewing geometry etc.. A short description of GNSS constellations would be helpful. It would also be helpful to explain how Figure 3 should be read in terms of azimuth and incidence/elevation angle. Please label azimuth and incidence angle on the plots and/or mention in the caption to improve readability for new users.

That's a good idea. We have updated Figure 3 and its caption to make it more readable for readers not familiar with GNSS (see below).

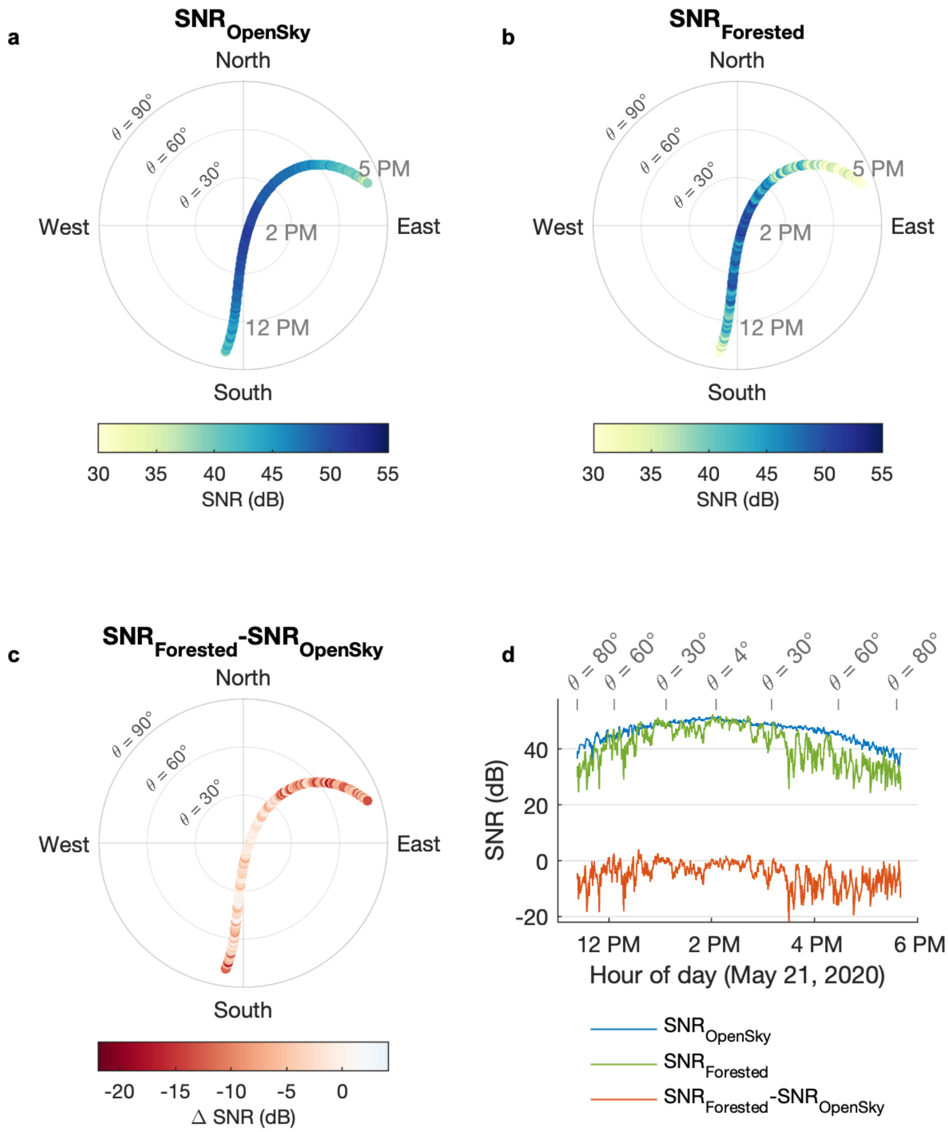


Figure 3. Sky plots illustrating the SNR observations on May 21, 2020 for one specific GPS satellite (PRN2) at the open sky site (a) and the forested site (b). (c) Difference in SNR between the two sites. (d) Same as a-c but showing the temporal evolution of the SNR. The centre of the polar plots corresponds to an incidence angle of zero between the ground and the satellite.

The text is also modified to include the following description:

L241: *"The hemispherical plot in Fig. 3a illustrates the SNR values measured over the course of one single day at the reference (open-sky) station for just one satellite of the GPS constellation (PRN2). GNSS satellites are commonly identified by their pseudorandom code (PRN) which allows the receiver to determine which satellite is being tracked, such that its azimuth and elevation can be calculated. Individual satellite tracks* *repeat after a period that depends on the GNSS constellation (e.g. twice per sidereal day (23h56) for GPS and every 10 sidereal days for Galileo)."*

Section 3.1: Provide details on how data from different GNSS constellations are merged. In particular, mention if there are any systematic differences and how they are handled during merging.

Thanks for this suggestion! We add the following:

L255: "Note that absolute SNR values vary from spacecraft to spacecraft, as those have different (and occasionally time-varying) transmit powers. It is thus very important to first pair the individual SNR measurements taken by the two receivers and only then, average the obtained ΔSNR values."

Line 294 – 302: I'm not convinced by this argument. If non-random multipath interferences are not excluded, will they not introduce or contribute to spurious values of VOD rather than random noise? If so, there is a danger that these are incorrectly interpreted as VOD variations? Please demonstrate that this is not the case.

Thank you for this comment. This statement refers only to temporal averages but we agree the formulation was maybe a bit unclear.

As the non-random coherent reflected multipath signals go in and out of phase, this produces peaks and lows in the collected SNR data [Nievinski and Larson, 2014]. When comparing data from two different receivers, these peaks and lows are not aligned, potentially leading to spurious values where SNR at the reference site is lower than at the forested site. Our argument is that these differences average out with time integration (the same argument is made in *Guerriero et al. [2020]*). See the following figure based on data collected with two antennas placed next to each other at an open-sky site. In this case, there is no vegetation so our VOD estimate should be zero. If we were to exclude spurious negative VOD values and only average positive VOD values, our hourly average VOD would be biased high. We make it clearer in the text that this applies to temporal averages.

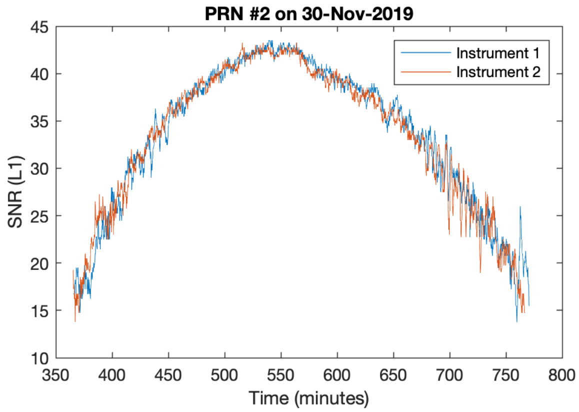
The following statement…

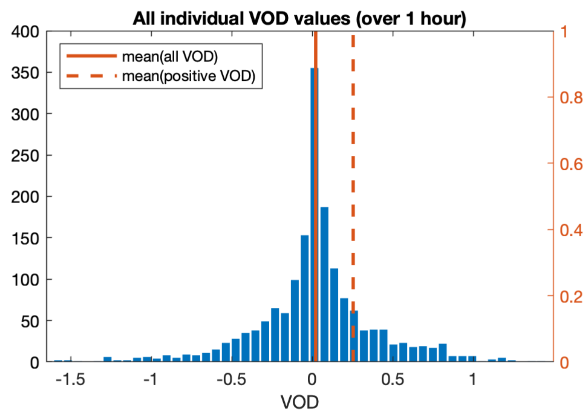
L297: *"To preserve the error structure of the measurements, we propose to still use these unphysical values whenever possible, and especially when computing averages, so that positive and negative random errors can cancel out, avoiding a potential bias in our estimate of the long-term average VOD."*

is modified as follows:

"To preserve the error structure of the measurements, we propose to still use these unphysical values when computing temporal (i.e. daily or hourly) averages later in the

paper, so that positive and negative errors can cancel out, avoiding a potential bias in our estimate of the average VOD."





Top: SNR values measured with two Trimble NetRS receivers equipped with Zephyr antennas on November 30, 2019, for a satellite of the GPS constellation (PRN2). Bottom: Histogram of all individual VOD values derived from all satellites over a period of 1 hour (and averages of that data).

Section 3.3: I found the nomenclature in this section, particularly the use of the terms anomaly and static, confusing and potentially misleading. In practice, the issue is that a robust estimate of the temporal variation can only be obtained at the expense of spatial aggregation, i.e. a loss of spatial resolution. The methodology to obtain the time series itself is fine, but I would recommend re-thinking the nomenclature.

Thank you for sharing these concerns. We reformulate the nomenclature as follows.

L338: "The goal is to subtract the angular heterogeneity in VOD, representing the uneven canopy distribution, and only retain residuals from the locally averaged attenuation (Eq. 12). The long-term average at a given incidence angle and azimuth (Eq. 13) is calculated *inside a neighbourhood N that includes all measurements within some chosen angular distance δ from that point of interest (Eq. 14).”*

And add the following:

L320: “In practice, this means that a continuous (gap-free) and robust VOD time series can only be obtained by aggregating data collected at different azimuth and elevation angles (i.e. trading angular resolution for temporal coverage).”

Lines 495 to 507: Why is it necessary to optimize v_veg with a daily time step? In a forest, in particular, this quantity is likely to vary over much longer time scales. This could obviate the need for some of the low-pass filtering in later steps.

That’s a good suggestion, in fact, calibrating v_veg at a longer time scale is what we did initially. However, optimizing v_veg at a daily time step provides a very efficient way of mitigating the influence of outliers (like the rainfall event at the beginning of the time series). With a daily estimate, only the v_veg of a single day is heavily biased and this is easily removed with the low-pass filter. If we were to optimize v_veg over a moving period of say, a whole week, the whole week would be biased high around that event.

The conclusion should include some discussion of the trade-off between temporal and spatial resolution. Lines 460-466 could be moved to the conclusion as part of this discussion. It is relevant in terms of the processing, but also in terms of sensor installation. I think it is important to emphasize that the capacity to obtain finer angular resolution comes at the expense of temporal resolution. There are applications where one might be more critical than the other, and many applications where the trade-off is non-trivial.

We agree. We add more discussion on this in the conclusion:

L706: *“Here, obtaining such high-frequency (e.g. hourly) VOD time series comes at the cost of angular resolution, since measurements taken at all azimuths and elevation angles are aggregated into hourly averages. Because of the configuration of the GNSS orbits, users face a trade-off between obtaining VOD estimates at high angular resolution (e.g. Figure 5b) versus obtaining VOD time series at high temporal resolution (e.g. Figure 7b).”*

Minor comments:

I would recommend having it proof-read by a native speaker to remove small errors.

We will do this. In the meantime, thank you for your corrections below.

Line 11: time-consuming destructive samples

Line 18: at a forested site

Line 24: Sensitivity to rainfall and dew deposition events ….

Line 33: remove "direct". The information is not direct. It needs to be inferred from retrieval products.

Thanks! All done.

Line 39: re-phrase. The use of arguably and currently is awkward.

We remove these two words to make the sentence simpler.

Line 166: How many leaf samples? Provide details of the protocol used to ensure that the leaves collected were representative.

Here is the updated description:

L166: "Forty-eight leaf samples were collected from two live oaks closest to the GNSS antenna on October 18, 2020, at 7am, 12pm and 5pm using a 2m long pruner. For each tree we equally sampled the same three different parts of the crown. Unless otherwise stated below, we followed the protocol advised in Mullan and Pietragalla [2012]. Leaves were weighed on-site immediately after being sampled (fresh weight; FW) and stored individually in cooled glass vials."

Line 228: Define vegetation density for readers not familiar with microwave remote sensing.

We update the definition at L226: *"where υ_veg represents the vegetation volumetric density, defined as the volume fraction of vegetation material within the canopy (on the order of 0.0001-0.01 m3/m3), a parameter that may vary as a function of the growth cycle"*

And we add the following:

"This parameter is not to be confused with other measures of vegetation density like crown volume (i.e. including empty space) per m^2 for instance."

Figure 2: In the caption, replace "Canopy transmissivity" with GNSS VOD.

We replace with "Modelled VOD".

Line 247: This should be Eq.9 ?

Yes, thanks a lot for spotting this.

Line 279: It would be useful to indicate which data are excluded on Figure 3(d), in terms of time of day so that the reader can put the discussion in this section in the context of the data they see in Figure 3.

Done, we have added the incidence angles in the revised Figure 3d (see above).

Line 419: The study of Vermunt describes a diurnal cycle in backscattern (not VOD) due to dew and interception. It belongs in the first paragraph of this section.

That's right. We reformulate L418: *"Overall, our results agree with previous observations of a diurnal cycle in VOD and backscatter (e.g. Konings et al., 2017b;Holtzman et al., 2021;Vermunt et al., 2021;Prigent et al., 2022)."*

Line 454: v_veg is called the volume density here and the vegetation density elsewhere. Define it once, clearly, in Line 228 and use a single term throughout.

We have made sure we use "vegetation volumetric density" everywhere.

Line 506: What metrics are used to evaluate agreement?

We made our statement clearer:

L487: *"The root mean square error between modelled and observed VOD is always used as the cost function and optimization at steps #2 and #4 is carried with a simplex search method."*

Line 515: What is the cost function used here?

See above.

Line 531 – 533: Remove "Thus," from this sentence. The assumption does not follow from the previous two sentences. Though it is a necessary assumption. You should write "It is assumed that the dielectric … ".

Agreed.

Line 538: the retrieved gravimetric …

Line 715: time of the Sentinel-2 overpass

Line 739: We suggest placing …

Thanks, we have made the suggested changes.

We thank the reviewer for their time and their constructive feedback!

Guerriero, L., F. Martin, A. Mollfulleda, S. Paloscia, N. Pierdicca, E. Santi, and N. Floury (2020), Ground-Based Remote Sensing of Forests Exploiting GNSS Signals, *Ieee T Geosci Remote*, *58*(10), 6844-6860.

Mucia, A., B. Bonan, C. Albergel, Y. Zheng, and J.-C. Calvet (2022), Assimilation of passive microwave vegetation optical depth in LDAS-Monde: a case study over the continental USA, *Biogeosciences*, *19*(10), 2557-2581.

Mullan, D., and J. Pietragalla (2012), Chapter 5. Leaf relative water content, in *Physiological breeding II: a field guide to wheat phenotyping*, edited by A. Pask, J. Pietragalla, D. Mullan and M. P. Reynolds, p. 132, CIMMYT, Mexico.

Nievinski, F. G., and K. M. Larson (2014), Inverse Modeling of GPS Multipath for Snow Depth Estimation—Part I: Formulation and Simulations, *Ieee T Geosci Remote*, *52*(10), 6555-6563.

Schmidt, L., M. Forkel, R.-M. Zotta, S. Scherrer, W. A. Dorigo, A. Kuhn-Régnier, R. van der Schalie, and M. Yebra (2022), Assessing the sensitivity of multi-frequency passive microwave vegetation optical depth to vegetation properties *Biogeosciences Discussions*.