Contents of the response document

1. Response to comments by Referee #1
2. Response to comments by Referee #2

We reproduce each comment, followed by our response and the corresponding manuscript changes.

Line numbers in comments correspond to the original preprint. Line numbers in our responses correspond to the *revised* manuscript.

1. Response to comments by Referee #1

I am satisfied with the authors' response to my comments. I recommend that the paper is accepted for publication.

One very minor suggestion: I would recommend including the explanation provided in the response to the following comment in the final manuscript.

Reviewer comment: 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.

Author response: 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 more 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 may be biased high around that event.

> We thank the referee for the careful evaluation. We have included the following explanation:
>
> <u>*L541: Note that although calibrating v_veg over a time period longer than a day would also smooth the estimate, we found that optimizing v_veg at a daily time step and then applying a low-pass filter was much more effective in mitigating the influence of outliers.*</u>

2. Response to comments by Referee #2

I want to thank the authors for their diligent work and responses to my comments. While the paper's clarity has been improved concerning the previous version, I felt I needed to clarify and reemphasize some of my earlier comments. In this round of the review, I provide my significant disagreements, and then I provide detailed comments on specific text in the manuscript. Also, I appreciate the authors' effort to make the assumptions 100% transparent throughout the manuscript, but this is not a favor; it is expected from every scientific manuscript.

We thank the referee for the careful evaluation and detailed comments.

I'm afraid I have to disagree with the major assumption, "Forest attenuation due to leaves dominates at L-band," as I stated in my previous review. The previous research overwhelmingly showed that "branches are the most dominant constituent for L-band attenuation and scattering of a forest canopy (please see Ferrazzoli and Guerriero, 1996, Ferrazzoli et al., 2002, Kurum et al., 2009)." I am adding a few excerpts from these papers:

We acknowledge your disagreement with the statement "Forest attenuation due to leaves dominates at L-band". We note that this statement does not appear anywhere in our manuscript. Our manuscript does not state that forest attenuation due to leaves dominates at L-band and is in fact much more nuanced, see e.g.:

L198: *"Note that this formulation and the overall concept of bulk coefficients is applicable only when the inclusions in the canopy (i.e. the pockets of water within the vegetation tissues) are smaller in size compared to the observation wavelength (here $\lambda_0 \approx 19$ cm for the GPS L1 frequency), so that scattering effects are small enough to be neglected [also see Jackson and Schmugge, 1991; Ulaby and Long, 2014]. While this may be true for leaves, this assumption may not hold for larger elements such as branches and trunks."*

L232: *"It is important to note that this simple formulation neglects several aspects, including volume scattering, which may be important in configurations with denser biomass (and also when interpreting LHCP backscatter or LHCP signals)."*

L554: *"Before we interpret these results, some limitations to the presented approach need to be emphasized. First, the dielectric model of Ulaby and El-rayes (1987) was originally developed for leaves, however, it is clear that branches and stems may also contribute to canopy extinction."*

L563: *"It is assumed that the dielectric model of leaves and its sensitivity to moisture, temperature, and salinity provides a sufficient approximation for the behaviour of the whole crown (branches and stems included)."*

Thus our actual assumption is that a model appropriate for a canopy constituted of small elements provides a reasonable first-order approximation, even though large branches, and to a lesser extent, trunks, also contribute to attenuation by the canopy in some proportions, the determination of which lies outside the scope of this study (and remains an active area of research in our opinion).

We discuss the implications of this assumption at several points in the manuscript and recognize that using other, more detailed, retrieval approaches may be helpful in the future, should the data be able to support the added complexity and higher degrees of freedom.

Kurum et al, 2019 (DOI: 10.1109/TGRS.2009.2026641)

"The contribution of leaves at L-band to the total backscattering response was found to be negligible compared with the contribution from branches and trunks. The leaves, however, were included in the simulations due to their significant effect on the canopy extinction."

We note that the beginning of this quote refers to backscatter and not attenuation of the direct signal as seen from the perspective of a sub-canopy receiver. The significant contribution of branches to backscatter is already acknowledged twice in our manuscript, at L232 and L558. The second part of the quote is in line with our assumption.

"Branches are main contributor to the volume scattering."

"The effect of the trunk layer in the total extinction at L-band is relatively small due to the large densities of branches in the crown layer and the small heights of tree trunks [11]. This is typically true for two-layer forest canopies where the trunk layer is composed of vertical trunks. For the deciduous trees considered in this paper, the attenuation in the trunk layer was found about 1 dB. This trunk attenuation factor is assumed to be negligible."

The implication is mentioned explicitly at L599: *"Note that this estimate should be interpreted with the awareness that VOD-based estimates of AGB likely do not weigh all canopy constituents evenly."*

We note that these limitations similarly apply to a large body of published studies which have used VOD observations as a proxy to investigate forest dynamics and inter-annual variations in aboveground biomass stocks.

We also mention this at L613: *"As for AGB, it's important to keep in mind that the CWC estimate does not weigh all canopy constituents evenly. Comparisons with other studies are quite difficult here because the relationship between VOD and vegetation water content is poorly known for forests"*

Finally we now add the following statement immediately afterwards at L614: *"<u>In particular, our retrieval assumes that the attenuation is dominated by small canopy elements, even though the contribution of large elements (like large branches or trunks) to CWC is likely not negligible.</u>"*

Ferrazzoli et al., 2002 (DOI: 10.1109/TGRS.2002.807577)

"Figs. 5 and 6 report, respectively, the model-simulated emissivities and transmissivities of trunks, branches, needles, and understory (in Fig. 5 soil contribution is also shown). The figures show that from the various forest components, branches mainly contribute to both emissivity and attenuation, while needles, trunks, and understory are minor contributors. We note that trunks, although containing most biomass, produce little effects; on the opposite, branches, which represent only a small percentage (10% to 30%) of the total biomass, are the main elements responsible for wave extinction and emission."

"A detailed analysis indicated that, at L-band, the main contribution to emission and attenuation was due to forest branches, while trunks had smaller effects."

> We note that this study is focusing on a maritime pine forest and discusses the case of a spaceborne radiometer. Our setup is in a broadleaf forest and considers right polarized GNSS signals measured below the canopy, a situation which we believe is closer to the study by Guerriero et al. (2020) or Steele-Dunne et al. (2012).

P. Ferrazzoli; L. Guerriero 1996 (DOI: 10.1109/36.485121)

"Model predicted vegetation attenuation is plotted in Fig. 6 as a function of frequency, in the range 1-10 GHz, for a deciduous forest with high biomass (240 tonsha), at B = 15" and B = 45". The total attenuation and the single contributions of leaves, branches and trunks are shown. Leaf attenuation is low at 1 GHz and appreciably increases with frequency. Branch attenuation, on the contrary, is slightly affected by frequency. These results are in good agreement with experimental data of [26], which indicate crown attenuation of a deciduous forest to increase with frequency in summer time (with leaves) and to be almost frequency independent in winter time (without leaves). According to Fig. 6, when the frequency increases from 1 to 10 GHz, variations in the overall attenuation are low in absence of leaves, while are higher but still limited (-5 dB at 15", -10 dB at 45") in presence of leaves, although a forest with high biomass has been considered."

> Thank you for pointing us to this reference. We acknowledge that the conclusions of this model-based study are different compared to the (also model-based) conclusions of Steele-Dunne et al. 2012. We add the following statement (underlined) to L561:
>
> *"Steele-Dunne et al. (2012) arrived at similar conclusions and concluded that leaf moisture is by far the dominant control on vegetation transmissivity at L-band for both polarizations <u>(but see Ferrazzoli and Guerriero (1996) for a different perspective)</u>."*

The authors provided further evidence for the claim of dominant leaf contribution by referring Matzler (1994), Seele-Dunne et al. (2012), and Schwank et al. (2021).

However, Matzler (1994) mainly studied transmissivity at frequencies above 5 GHz. It is challenging to extrapolate his results to L-band. Seele-Dunne et al. (2012) looked at backscatter data at C-band and did some simulations for a specific tree (trembling aspen) at L-band, and hard to generalize for a global conclusion on leaves dominance on the VOD. Schwank et al. (2021) acknowledge the dominance of branches by stating, "Branches are most determinative for L-VOD of a forest canopy (Ferrazzoli and Guerriero, 1996)." However, they violate the validity of the Maxwell Garnet mixing rule by formulating a canopy layer mixing model. Maxwell Garnet mixing rule is only valid within the limits of the quasi-static approach (the scattering losses are not incorporated), which is true when the wavelength is larger than the dominant inclusions.

We would like to emphasize that nowhere in the manuscript do we *claim* that leaves have a dominant contribution to attenuation. Instead, our assumption is that a model appropriate for a canopy constituted of small elements (such as leaves) provides a reasonable first-order approximation. See e.g.:

L198: *"Note that this formulation and the overall concept of bulk coefficients is applicable only when the inclusions in the canopy (i.e. the pockets of water within the vegetation tissues) are smaller in size compared to the observation wavelength (here $\lambda_0 \approx 19$ cm for the GPS L1 frequency), so that scattering effects are small enough to be neglected [also see Jackson and Schmugge, 1991; Ulaby and Long, 2014]. While this may be true for leaves, this assumption may not hold for larger elements such as branches and trunks."*

L563: *"It is assumed that the dielectric model of leaves and its sensitivity to moisture, temperature, and salinity provides a sufficient approximation for the behaviour of the whole crown (branches and stems included)."*

The authors completely ignore contributions from branches with the assumption of leaf dominance in forest attenuation. This also leads to the misintereptration of the whole canopy average VOD results. It is well known that most biomass is confined within trunks and branches. As clearly articulated in the paper, VOD has a consequence of aggregate due to vegetation water content (short-term fluctuations) and dry matter (seasonal variations). Thus, it is contradictory to attribute the leaf water content to the whole canopy's average VOD. In addition, a high correlation between VOD and leaf water content does not necessarily mean that the leaf water content dominates VOD. From the present analysis, it is unclear which part of the plant influences VOD more, as the study completely ignores contributions from branches.

We note that none of our findings or conclusions implies (or aims to make) any statement on which part of the plant influences VOD the most and how to best deal with it. We believe this remains an area of active research which will hopefully benefit from more widespread and continuous in situ measurements of in situ VOD.

To sum up, it is known that the branches are the dominant constituents in L-band attenuation and volume scattering. Hence any forest model that only considers leaves cannot accurately mimic the physics (wave interaction and propagation). Thus, I can't entirely agree with using the leaf dielectric-mixing model to analyze experimental data at L-band.

Specific comments:

Page 2, Line 46: Microwave remote sensing is generally characterized by active (radar), passive (radiometry), and signals of opportunity (e.g., GNSS-R). GNSS-R has been mentioned on line 132 on page 5. You may want to move these up where you describe microwave remote sensing on page 2.

Thank you for this suggestion. As page 2 aims to provide a broad introduction, we prefer to discuss GNSS-R after we have introduced global navigation satellite systems in general on page 5.

Page 3, Lines 77-82: Authors correctly argue that VOD is a better proxy for forest height and biomass than optical indexes by citing literature. Then, they compare their VOD results against the enhanced vegetation index derived from 30-meter satellite images of Sentinel 2 in section 4.1 and page 32, Line 623. A clarification would be helpful.

Optical vegetation indices still provide useful information on vegetation development and are not used here as validation for VOD. This is why differences between VOD and EVI are discussed in section 4.1:

L403: "EVI is a commonly used vegetation index and an overall proxy for vegetation greenness, health, and photosynthetic activity. Generally, we find that the temporal evolution of VOD appears to lag behind that of EVI by about 2 months. This is consistent with previous findings over drylands by Tian et al. (2016) who found a temporal shift (increasing as a function of forest density) between satellite-based VOD and vegetation greenness."

The discussion on page 32, line 623 refers to NDWI and not EVI. Here, NDWI is used as an independent data source, not as a proxy for height or biomass:

L630: "NDWI is a good proxy for vegetation water content and is based on optical and near-infrared measurements, thus providing fully independent observations with respect to our retrieval."

Page 6, Lines 135-136. This is not necessarily correct. The GNSS signals under dense forests can be smaller than GNSS reflections from wet surfaces.

Thanks for noting this, we have made our statement more accurate (underlined):

L135: *"GNSS reflectometry relies on GNSS signals that are reflected from the Earth's surface and which are weaker than the open-sky GNSS signals used as reference here."*

Page 7, Line 159. Please define the acronym RHCP.

Thanks, we moved the acronym's first definition to that line.

Page 9, Lines 203-204. It is also important to point out under what conditions these statements are correct. Otherwise, it sounds like these are general conclusions. As I mentioned in the previous review, it is hard to generalize Guerriero et al. (2020) result as it is only done for a particular case.

We modify the statement as follows (underlined):

L203: "However, using a more complex theoretical scattering model, Guerriero et al. (2020) showed (for the case of a poplar forest) that the RHCP GNSS signals measured below a forest canopy are dominated by coherent attenuation whereas only the left-hand circular polarized signals (which most geodetic ground-based GNSS antennas are designed to reject) are dominated by volume scattering."

Page 10, Line 234: RHCP backscatter?

Thanks a lot for spotting this, changed to LHCP.

Page 11, Line 252: Is it SNR or CN0? https://insidegnss.com/measuring-gnss-signal-strength/

The receivers log C/N_0. For simplicity we used the notation 'SNR' as it does not introduce a fraction in the equations and is the most common notation (e.g. the same is done in https://doi.org/10.1007/s10291-012-0259-7 or https://doi.org/10.1038/s41598-019-40456-2). But it's a good point to mention this explicitly, thanks.

L254: "The quantity logged by the Septentrio receiver is the carrier-to-noise density ratio (C/N_0), which we report as SNR for simplicity, assuming a 1-Hz bandwidth [Larson and Nievinski, 2012]."

Page 14, Lines 296-298: This is a very strong statement regarding the dominance of direct signals. This is a very significant assumption for this paper and needs to be clearly backed. I suggest that the authors point out the similarities and differences between Guerriero et al. (2020) and the present setups. It is too fast to state that both setups are similar. In addition, geodetic-grade antennas are designed to reject reflected multipath coming from the lower hemisphere. The volume scattering is also a multipath and is received through the antenna's upper hemisphere. Yes, a significantly reduced ground-reflected multipath can be considered of second order, but a multipath due to the volume scattering can still be significant. Without a piece of concrete evidence on volume scattering, the interpretation of the results will be speculative.

Maybe this was a misunderstanding. We modify the statement (underlined) to make it clear this refers to ground multipath (which is discussed earlier):

L298: "Thus, in our case, the difference in SNR between the two sites is predominantly due to the attenuation of the direct RHCP signal by the forest canopy and it is reasonable to assume that <u>ground</u> multipath effects are of second order"

Page 15, Lines 315-319: I would not call the negative difference unphysical; as you correctly stated, individual measurements include (1) random noise (slight system, and configuration differences) and (2) multipath interference. The negative difference mostly happens when the transmitter is visible through the gaps in the canopy. The supporting figure 1 (in the response letter) only shows the random noise in an open field; this does not necessarily match those happening within the gaps. Then, I can easily argue that other noise components (the multipath interference) might be due to the volume scattering since it provides an additional signal for under-canopy measurements through the gaps.

We update the statement as follows:

L317: "In some cases, the instantaneous transmissivity values computed from the raw GNSS measurements were higher than 1, leading to a VOD lower than zero~~, which is unphysical~~ (about 8% of all measurements).

Page 23, Lines 449-450. It is hard to generalize any results with a few leaf samples. Figure 9(b) shows only three points on two different days.

We do not believe our statement is implying any generalization.

L451: "The leaf samples collected on the site in October also confirm that some intra-day variability exists in relative leaf water content (Fig. 9b)."

Besides, diurnal variability in leaf water content has been widely reported (see the studies cited at this point in the text).

Page 29, Line 552 - Page 30, Line 563: This is my main disagreement with this paper, as I stated in my general comment in the beginning.

As mentioned our general response above, we modify one sentence in this section as follows:

L559: "Steele-Dunne et al. (2012) arrived at similar conclusions and concluded that leaf moisture is by far the dominant control on vegetation transmissivity at L-band for both polarizations, (but see Ferrazzoli and Guerriero (1996) for a different perspective)."

We also modify L554 as follows:

L554: "Before we interpret these results, some limitations to the presented approach need to be emphasized. First, the dielectric model of Ulaby and El-rayes (1987) was originally developed for leaves, however, it is clear that branches and stems ~~may~~ also contribute to canopy extinction."

Page 30, Figure 11(a): Again, it is hard to arrive at any conclusion with three medians of leaf measurements.

See our statement a few lines later:

L574: "this does not provide any formal validation of the m_g retrieval"

Page 31, Line 593: 10.9 kg m^-2 is an excessively high value since the mixing model only considers leaves. The cited values in the literature are the AGB of the whole canopy (trunks, branches, and leaves).

The retrieved AGB is not that of leaves only since the VOD measurements also include the contribution from larger elements like branches as mentioned in the manuscript and as argued earlier by the referee. It is only that the retrieved AGB is estimated with a simple model which is physically valid for electrically small canopy constituents and not necessarily fully appropriate for the larger elements (and this latter part is the main assumption).

The cited values are fully comparable since they stem from empirical relationships derived between L-band VOD and biomass reference measurements, and similarly do not individually resolve the uneven contributions of leaves, branches and trunks to VOD:

"L605: For instance, using the exponential relationship calibrated at L-band by Vittucci et al. [2019], we obtain (for an average VOD value of 0.79 at our site) an AGB of 13.8 kg m-2, not so far from our estimate."

Page 31, Line 596. I disagree that "L-band VOD is primarily sensitive to leaves," as I stated in the beginning.

This sentence (L601) reports the results of Steele-Dunne et al. 2012. We remove *"primarily"*.

Page 36, Lines 691-693. Wave interactions at X-band (~3cm) and L-band (~20 cm) are fundamentally different. I would not compare the results at these two distinct frequencies.

We reformulate as:

L698: "These results indicate that L-band VOD is quite sensitive to intercepted rainfall, as has been shown with X-band VOD observations from the AMSR-E satellite (Xu et al., 2021) and from an in-situ radiometer (Schneebeli et al., 2011)."