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.

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:

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."

"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."

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."

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."

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.

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.

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.

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.

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.

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

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.

Page 10, Line 234: RHCP backscatter?

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

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.

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.

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.

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.

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

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).

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

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.