In the following, the review comments are included line-by-line in black whereby our answers are identifiable by blue color.

Answers to reviewer Martin Baur

General comments:

Dear Authors,

Thank you for the submitted manuscript. I believe the presented analysis of multi-frequency VOD is interesting and complements recent work. I think the study design, analyzing multiple VOD products with statistical models and predictor datasets, gives new insights into VOD dynamics and might help to understand the underlying reasons for these dynamics at different frequencies. Nevertheless, I think the manuscript gives away o lot of potential by not communicating methodology and results in a clear and intuitive way. Some parts of the discussion make claims that are not fully supported by the presented figures and results. I believe a main reason for some of these problems is the fact that the main manuscript has five figures only and a lot of result presentation and discussion is done referencing figures in the supplementary document. This makes it very difficult to follow the storyline of the manuscript. If this problem arises from a figure limit, please try to aggregate results into higher level figures with multiple labeled panels, I think this might be the best way to improve the manuscript.

We initially intended to keep the manuscript short by only including the main Figures. However, we are open to move Figures from the supplementary material to the main text and to produce higher-level Figures to make the manuscript more understandable. Please find our suggestions stated next to the Figures and as well as in the answers to the second reviewer.

Specific comments:

- I am not sure about the usefulness of Fig. 1, most of the shown maps are known to many readers and have been shown in many studies. Even if a reader is unfamiliar with VOD at multiple frequencies Fig. 1 does not provide much information as VOD is normalized to 0-1. This removes the frequency component. Even if the patterns are interesting 80% of the maps is not even used in the rest of the study as LFMC is only available for some regions. Most of the RF model results and discussion does not really include and spatial component, so a reader would hardly go back to Fig. 1 and look at a specific VOD map.

The VOD data are globally normalized to a range of 0 to 1. The maps and scales show different patterns which demonstrates that still after normalization the data is not the same. These patterns are induced by the different wavelength (and sensors for L-band), which demonstrates the purpose of this study to identify which vegetation-related factors contribute to VOD. We propose to add this information in section 2.1.1 after the normalization description. In addition, we wanted to visualize the spatial availability of the LFMC data to raise the attention that this study is inter-continental but not truly global. We propose to reference Figure 1 b in LFMC description paragraph in section 2.1.2. We still think that showing those maps is important for readers, which may not yet be very familiar with VOD data because it visualizes the association between e.g. high VOD, high AGB, high LAI and tree cover and vice versa.

- The use of the LFMC parameter seems to be motivated by Eq. 2. Predictor variables AGB, LFMC and landcover type explicitly and implicitly (through analysis of landcover classes) recreate Eq. 2.

It seems a little bit unintuitive that this logic is applied when average LFMC over whole Europe is 100% or even larger. It seems like this dataset might only work well in Australia or Southern Africa. Even if this is not a problem for the GAMs and RF analysis, I can't retrace why you would introduce Eq. 2 and then have a LFMC which is larger than 100% for nearly whole Europe. Even you could fix the range to 0-100% it is a problem that AGB is not dynamic in time. Furthermore, LFMC and LAI are probably quite similar as both originate from MODIS. At some point you say that LAI and LFMC are strongly correlated. I do not really see the point of introducing Eq. 2 and then not having the datasets to check whether this VOD formulation is sufficient or not.

We thank the reviewer for his correction of this statement in referee comment RC3: 'please disregard my second statement dealing with the LFMC value range. I did not fully understand the LFMC concept. I think Eq. 2 as it is is valid.'

- Section 2.5, which introduces the ALE figures should be simplified. I think it is crucial to have a sound understanding of what these plots show to understand the manuscript. I am sorry but currently I don't. ALE plots are introduced relative to PDP plots, readers might not know about neither of them. Can you try to work on this section and describe what the y-axis in Fig. 4 really shows?

Thanks for this comment. We agree that a good understanding of ALE (or PDP) plots is important for the understanding of our manuscript. Therefore, we propose the following modification in section 2.5:

'The relationships of VOD to the predictors are examined via Accumulated Local Effects (ALE) plots (Apley and Zhu, 2020). Like the commonly used Partial Dependence Plots (PDP) (Friedman, 2001), ALE plots show the marginal effect of a single predictor on the model predictions. The marginal effect is reflected in the local gradient of the ALE curve; for example, a positive gradient indicates that an increase in the investigated predictor should lead to an increase in the predicted model outcome all other predictors being equal. While both ALE and PDP take into account all other predictors to approximate the underlying relationship with the single investigated predictor, ALE does not combine each plotted predictor value with all possible combinations of the other predictors. Especially for correlated predictors, ALE plots are therefore more robust than PDPs (Kuhn-Régnier et al., 2021), as unlikely and unrealistic combinations of predictors are prevented. Therefore, equally-spaced quantiles across the range of the examined predictor are defined and each quantile is then used with only the closest existing combinations of the other predictors to calculate the marginal effects. The ALE plots were generated from the final models, where all available data were used for training.

To quantify the influence of the predictors on the target variable (sensitivities), we calculated the amplitude of the ALE curve (ΔA). '

- Currently GAMs results are only presented in text and not in a figure. If it is a goal of this manuscript to show that VOD to predictor relationship is complex, and therefore not captured well by GAMs, it would be nice to have visual results. I guess you ended up with many figures, one for each model type, landcover and monthly or 8-daily. Nevertheless, it would greatly improve the manuscript if these results could be aggregated into high level figures.

We agree that additional Figures or high-level Figures will improve the ability to guide the reader through the interpretation of the results. We propose to add further panels in Fig. 2 for the GAM and for results based on 8-daily time steps. Representative ALE plots for models with 8-daily data will be added in section 3.2.

- I don’t fully understand why all VOD products were normalized. Can you please explain this? I am not sure whether this is a requirement for the RF and GAMs model.

The normalization of the VOD data is not necessary for the algorithms. However, the normalization provides an easier comparison of the results. For example, RMSE depends on the value range. The different magnitudes of the five VOD data products would than not allow us to directly compare RMSE values. The same is true for the ALE plots, where the y-axis depends on VOD data range. Without the normalization, the ALEs will show different amplitudes by default, because for example the value for Ku-VOD will change more due to the greater basic value range than for L-VOD. The normalization prevents possible misinterpretation induced by the differing value ranges of all VOD products and allows a direct comparison of the model performance and of the ALE.

Line-specific comments:

Line 25: Sounds like VOD itself has a wavelength.

May be say "to select VOD at the most suitable wavelength"

We will change it in the proposed way.

Line 42-43: This statement seems to indicate that you want to present some references. May be consider adding 1-3.

Although a detailed description follows later on (line 56ff) we agree that the main references should be already included here. We suggest to include:

VOD in relation with AGB: Rodríguez-Fernández, N. J., Mialon, A., Mermoz, S., Bouvet, A., Richaume, P., Al Bitar, A., Al-Yaari, A., Brandt, M., Kaminski, T., Le Toan, T., Kerr, Y. H., and Wigneron, J. P.: An evaluation of SMOS L-band vegetation optical depth (L-VOD) data sets: High sensitivity of L-VOD to above-ground biomass in Africa, 15, 4627–4645, https://doi.org/10.5194/bg-15-4627-2018, 2018.

VOD in relation with LAI: Jones, M. O., Jones, L. A., Kimball, J. S., and McDonald, K. C.: Satellite passive microwave remote sensing for monitoring global land surface phenology, Remote Sens. Environ., 115, 1102–1114, https://doi.org/10.1016/j.rse.2010.12.015, 2011.

VOD in relation with VWC: Konings, A. G., Rao, K., and Steele-Dunne, S. C.: Macro to micro: microwave remote sensing of plant water content for physiology and ecology, New Phytol., 223, 1166–1172, https://doi.org/10.1111/nph.15808, 2019.

Line 58: Remove the bracket at the beginning of the sentence so the reference is in text? May be this is a problem with reference software. Please double check.

This is a typo and will be corrected.

Line 74: May be say "optical vegetation indices" .. more specific .. one could use a polarization based vegetation index (RVI or similar).

While NDVI can be named “optical vegetation index”, we would avoid to name LAI an “index” because it reflects a biophysical property of the land surface.

Line 77: why do you choose this specific example? I believe phenology is a difficult example where the performance of VOD might be worse than optical indices. I believe Jones et al. 2011 is Ku-band so the highest frequency you use in your study. At L-band this might not really be that valid anymore. Furthermore, VOD phenology in heterogeneous, low biomass environments will be very tricky to get right, even at high frequency.

I just wonder why you use this example out of many, which might be less easy to attack.

Indeed, assessing land surface phenology with VOD is complex. This might be due to the missing understanding of the information of VOD and this example highlights possible future applications if we get more insight in the controls of VOD.

We propose to change the lines 76-78 as:

‘Therefore, VOD provide complementary information to the usually visible-infrared based metrics (Jones et al., 2011). For example, metrics sensitive to biomass or water content shifts can be derived from VOD (Jones et al., 2011, 2014). VOD can also be used for assessing land surface phenology (Jones et al. 2011).’

Line 79-80: I believe this statement might be a little bit speculative. I am not sure whether I would be confident in making that statement. Just based on the underlying physics, VOD is most sensitive to water, not dry matter or structural carbohydrates. Depending on wood density, or in general whether a plant is more woody or not, there might be large differences between the amount of carbon stored in the plant biomass. I don't believe this has been studied in a detailed way.

The relationship between VOD and GPP has been studied in a detailed way in a series of publications: Teubner et al. (2018) compared VOD with GPP. Based on the found relationships, Teubner et al. (2019) developed an approach to estimate GPP from VOD which was further refined in Teubner et al. (2021) by accounting for effects of temperature and water availability, and which was used by Wild et al. (2022) to produce and validate a global VOD-based GPP dataset. However, the reviewer is right that the relative roles of moisture content and carbohydrates have not been yet disentangled in this method to estimate GPP from VOD.

We propose to alter the addressed sentence as ‘VOD and temporal changes in VOD are also correlated with gross primary production (GPP) (Teubner et al., 2018), which allows to use VOD as a predictor for GPP (Teubner et al., 2019, 2021; Wild et al., 2022).’

Line 91: What do you mean with this statement? The time lag between VOD and optical indices or precipitation likely has an ecological explanation.

We would like to state here, that the time lags between VOD and optical indices exist but that the underlying reasons are not fully explored until now. We propose the following change in the text:

"Additionally, Li et al. (2021) as well as Moesinger et al. (2022) found time lags between VOD and vegetation indices and climate variables, which are not yet fully understood. This shows the need to include further ecological parameter or vegetation variables which could account for a delayed response of VOD to temporal changes in the vegetation indices."

Line 114-117: I think some parts of the introduction leave implicit, which aspects of VOD make a specific VOD "suitable" for potential applications. This question is relevant, as it is a main motivation for this study. May be the following aspects could be reflected in some way:

- frequency: Might not only define which canopy constituents the signal is most sensitive, but other issues like RFI, observation resolution, or the validity of the tau-omega model are related to it too. Some people generally distrust 0-th order radiative transfer at frequencies higher than L-band.

- retrieval algorithm: There is a whole range of papers comparing algorithms. VOD retrieval is not trivial and can be subject to errors and artefacts. Especially for short term dynamics this is important.

- resolution: Both temporal and spatial. Generally, a big issue whether VOD is suitable for an ecological study or not. You can even get VOD from Sentinel-1 with very high resolution.

- system: Each system is only operational for a certain time and has different overpass timings etc. SMAP has 6am/6pm and AMSR-2 has 1:30am/pm.

These are very important points and we agree that those factors should be considered in a comprehensive assessment of the applicability of different VOD estimates. However, we decided to limit the degrees of freedom of our analysis by focusing on VOD from passive sensors that were retrieved with the same retrieval algorithm to be able to focus mostly on differences depending on wavelength (and sensors for the L-bands). A further analysis could account for different retrieval algorithms and sensors. We propose to include those factors either in the introduction line 115 with similar to: "Our study will help to identify a suitable VOD dataset in addition to the technical aspects of the datasets like: …" or to include them in section 2.1.1 VOD data.

Line 122-123: Why? Perhaps stress how both approaches are different and why

While the details about the regression models are provided in section 2.2, we propose to briefly mention the main differences of the two algorithms.

Line 126-127: Important. You could mention this and its implications earlier.

We propose to include this in line 118 after the overall aim of the study.

Line 144-145: Do you think this normalization is necessary? As far as I know in some tau-omega based retrievals any noise and uncertainty does not lead to normally distributed errors in tau, but will lead to overestimation of tau. Tau depends on the depolarisation of TBH/TBV, so if you approach full depolarisation any noise will make tau jump to high values.

I am worried that normalizing by max min might reduce the dynamic range of time series that have high mean VOD.

Our global normalization only scales the global distribution of VOD for each wavelength to the range between 0 and 1 to improve the comparability with VOD from other wavelengths. The dynamic range, distribution and variance remain the same as before the transformation. The normalisation process is individually applied for each VOD product, so that also min and max is individually defined for each VOD product.

Line 145-146: Is this done using the min and max of all data, or for each pixel. It only makes sense to me if it is done for min and max of all data. But I still don't fully understand why it is necessary

The normalization is done using the minimum and maximum of all data for the time span 2015-2017, not for each pixel itself. Please refer also to the previous comment answer.

Line 153: stem/stump isn't above-ground?

The dataset includes all above-ground biomass components of living trees, i.e. stems, branches and twigs. Remaining stumps from harvested trees are not included.

Table 1 – temporal coverage/ resolution: this is the maximum temporal range? In this study only 2015-2017 is used (limited by SMAP)? I am not sure if this could confuse readers. I am not sure about the importance of showing the availability if you end up using two years.

Same with the temporal resolution, in the end the analysis only uses 8 day time steps?

The table shows the maximal available temporal ranges for each data set but we only use the data 2015-2017, limited by SMAP and VODCA Ku-VOD as described in section 2.1.3. We carried out the analysis once with 8-daily temporal resolution and once with monthly temporal resolution. We propose to add 'Overview of the used datasets and their original technical attributes' as table caption.

Fig. 1: Is VOD already normalized? Colorscale seems to be different for each frequency. I am not sure why this should be the case after normalization. Think about what you want to show, normalization removes frequency increase. Most of these maps have been already shown in many publications and 80% of the shown land surface is irrelevant for your analysis as you are limited by LFMC, right?

Why is LFMC > 100% for most of Europe? LFMC should probably be capped at 100%. Not sure why that is, but it would be problematic if used with Eq. 2.

For the first part of this comment, please refer to the answer for the first specific comment regarding Figure 1.

Following RC3, we assume that the second comment part is resolved.

Line 197: This seems quite low, especially as land cover class homogeneity normally does not fully indicate homogeneity in the microwave realm. Do you have some results for highly homogeneous pixels (class > 0.8) for example?

We agree that the chosen threshold is quite low. We evaluated the results with thresholds between 0.55 and 0.95 with steps of 0.05. The model performances, i.e. R^2, improved but in favour of losing data points especially for the land cover classes deciduous trees and shrubs. For example, for treeBD at a threshold of 0.55 we have over 17,000 data samples for the 8-daily data (R^2=0.84), but for a threshold of 0.85 only 4,023 (R^2=0.86) and none for higher thresholds. Because the data is already limited by the LFMC availability, we did not want to further confine the data with using higher thresholds.

Line 254: After checking the ALE plots I believe they need a little bit more explanation. Readers might neither be familiar with ALE nor PDP plots. This is why the comparison of ALE relative to PDP does not seem useful. Can you focus on explaining x and y axis so everyone can follow the ALE figure later on.

Yes, we agree. Please see our answer to your comment for section 2.5.

Line 263: Is this on the y axis of Fig. 4?

Yes, this is correct. This might get more clear, when we will have added a detailed explanation of the interpretation of ALEs.

Fig. 2: I guess these are mean values. Is there a way to represent the distribution of all pixels in this figure?

The reported R^2 and RMSE values are not mean values but are computed from all model-data pairs across the study domain filtered for a specific land cover (i.e. from a global scatterplot).

Maybe this figure should have GAMs results too (4 panel figure)? This would be the figure to show that VOD ~ vegetation parameter relationship is complex and better captured by RF.

We agree and will include this Figure in the revised manuscript. As pointed out by reviewer Andrew Feldman (RC2) it would be also beneficial to include the 8-daily global results in the main manuscript.

Line 324-325: This is an explanation, but does not directly follow from Fig. 3. It would be nice if the figures would provide some explanation or analysis of the patterns we see, not just the patter itself. At the moment Fig. 3 only shows R^2 patterns.

The shown patterns originate from complex relations depending on different land cover types, ranging from homogeneous to heterogeneous land cover, and consequential depending on complex influences of the predictors. In addition, the temporal dynamic of the predictors per pixel has to be taken into account. There are several options to address this, for example analyzing the time series individually, comparing standard variations or dividing the results into similar ecosystems. We propose to quantify and visualize the relationship between the correlation and the standard deviation of LAI or LFMC as proxies for the seasonality.

Line 327-328: relating this to drought resistance seems a little bit too speculative to me.

We propose to weaken the statement as following: 'LFMC, *possibly induced by* more or less stable weather conditions or *by* more drought resistance of less plant-water sensitivity (Rao et al., 2022), such as the central areas of California, northern Europe and central Australia'.

Line 336-337: Be careful with this statement. R^2 increasing with VOD might (or should) break down with very high VOD (>1.5). You do not include any rainforests, so this might only be correct in the specific context of your study. Maybe try to weaken the statement.

Thank you for this comment. We will add the following sentence after the mentioned statement: 'Whereby this finding is based on the VOD range in our study, it might be not valid for very high VOD values, e.g. in rainforests, which are not considered here.'

Fig. 3: I would be really interested on how these patterns emerge. More than just seeing all of them. How does this relate to the predictor datasets?

This comment seems to be related to the comment on line 324-325. Please refer to the answer for this comment. However, to increase the opportunity for interpretation, we propose to add a Figure with boxplots of the spatial distributed R^2 results grouped by the dominant land cover type or stratified by the standard deviation of LAI.

Line 375-376: magnitude of VOD dynamics?

Here we address the whole range of the VOD values, which than applies also to the magnitude of VOD dynamics. We propose to change the line as following: 'fact that the magnitude of VOD *value range* decreases with increasing wavelength.'

Fig. 4: Please consider adding a), b) ... to label each panel in your figures. I believe this will greatly improve the readability of the manuscript. I think it is necessary as you discuss many ALE figures. Do you think you could stretch the figure so the 5-95th percentile is easier to see?

Line 380: some of these figures struggle with the fact that the x-axis spacing is very large, so 5-95th percentile data range might only get 20% of the figure.

We will add labels to the panels in the revised manuscript.

We evenly spaced the percentiles on the x-axis and included selected quantiles as lines. Other types of stretching of the x-axis values made the plot even more complicated and did not contribute to comprehend the Figures.

Line 387-389: I am sorry, I cannot really spot this. It is difficult to make a statement based on both manuscript figure and supplementary figure. Can you specify what you mean with "highly non-linear"

We agree, that this statement is not fitting the Figures. We will change the paragraph as follows:

'Comparing the ALEs of the treeAll and the individual forest type models (i.e. treeB, treeN, treeD, treeE, Figure S8), the influence of a specific forest type is partially recognizable within the treeAll ALEs. For example, the treeAll LFMC-ALE is highly influenced by the relationship for needle-leaved and evergreen trees. The apparent SMOS L-VOD decrease with LFMC is also pronounced within most tree types but not within deciduous trees. The AGB-ALE for needle-leaved trees is less nonlinear in comparison to the other tree cover models. Deciduous and broad-leaved trees exhibit a more complex relationship with AGB than evergreen and needle-leaved trees.'

Line 389-390: Is this just based on the visual inspection of the figures? Is there a way to quantify this? It is difficult for me to spot how strongly the effect of a single PFT is, especially as axis in S8 and Fig. 5 are different.

Yes, this is based on visual inspection as to our knowledge the here chosen approach does not provide the segregation between the influences of different vegetation types within the overall influence of a certain feature on VOD. An alternative way would the build-up of a physical based model for simulating VOD enabled by a detailed parametrization of the controls on the vegetation layer. To allow the direct visual comparison between the treeAll and the other tree models, the treeAll ALEs are plotted in the first row of S8. However, the reviewer is right, that the y-axis in S8 differs from Fig 5, and in addition, the x-axis within S8 are not constant of the rows. We propose to generalize the axis limits within the same temporal resolution and machine learning algorithm for the land cover models.

Line 392-393: I can't really follow. This statement is solely based on S8? What exactly makes the relationship more complex? All of the ALE figures with AGB are not really conclusive in my intuition.

We propose to incorporate the amplitude in this statement as follows: 'Deciduous and broad-leaved trees exhibit a more complex relationship with AGB than evergreen and needle-leaved trees over the wavelength range of the VODs. Comparing the amplitudes of the AGB-ALEs for the different tree cover types, the highest amplitudes were found for X-VOD for treeD $\Delta_A = 0.175$ and for SMOS L-band for treeB $\Delta_A = 0.313$.'

Line 395-396: I am not sure if a single feature from short vegetation making it into the global ALE curve is enough to postulate this. Is there a way to quantify the impact of the short vegetation ALE curve on the global ALE curve? I am more interested in why this feature appears in the first place? Do you have an explanation?

The herbaceous land cover is mostly driven by pixels in South Africa and Australia, whereby most of the pixels are dominated by a LFMC between 40% and 60% (also shown by the horizontal percentile lines in S10). In addition, most of the pixels are a mix of land cover types (PFT herb fraction is notable below 0.7). It might be that the South African and Australian herbaceous vegetation shows a strong species-specific behaviour around a LFMC of ~50% or that the relation before this drop is not well captured by the models.

Line 401: Eq. 2 and including LFMC becomes less clear if its median value is 100%. Why is LFMC so high? It seems like it only works for Australia.

Following RC 3, we assume that this comment is resolved.

Line 412-413: How is this conclusion evident from the manuscript figures? None of main figures is dedicated to show this fact (I believe Fig. 2 should do that). I believe there is too much discussion and storyline based on the supplementary figures. If RF is better than GAMs and this is a main finding of this paper, it would be best to have a figure supporting this.

We agree. In addition, this comment is related to the comment on Fig. 2 and might be resolved with the changes which are proposed in the related answer. Please refer here to the comment on Fig. 2.

Line 419-422: It does not become fully clear to me why L-band is so much more difficult to predict than higher frequencies. Can you hypothesize why that is? I believe this could lead into a really interesting direction.

The shortwave VODs are sensitive to the top canopy layer which can be represented by the chosen features, especially LAI and LFMC. Thereby also the structure of the vegetation layer (i.e. PFT fractions) play a role. L-band VOD is less sensitive to the top of the canopy but more sensitive to underlying compartments. Therefore, the chosen predictors might not be sufficient, namely LAI and LFMC. The inclusion of the PFT fractions improve the model efficiency. Nevertheless, L-VOD is controlled by vegetation properties and processes which might be distinct to the controls of shortwave VOD and are not well captured by the chosen predictors. This is also more prominent for the 8-daily analyses. Possible reasons, i.e. controls of L-VOD which might be important for the prediction but are not considered in this analyses, are attempted to be addressed in line 423-482.

Line 425-427: Is this the only possible explanation to why L-band is so much harder to predict?

Additional and more detailed explanations of the model performance for L-band VOD can be found in lines 452-482, e.g. influence of water as interception or standing water, soil properties and missing understanding of the controls on L-VOD as well as inadequate predictors.

Line 428-429: Why are these effects more prominent at L-band? I believe intercepted water might be even more problematic at higher frequencies.

Intercepted water can not only found on leafs but more often on the woody parts of the vegetation, e.g. twigs and stem which L-band VOD is more sensitive to than shortwave VOD due to the deeper penetration ability. This applies also to the soil properties and standing water.

Line 429-430: Interesting, but it is important how strong this effect is relative to the other frequencies.

Wigneron et al. (1996) reports also a possible doubling in C-VOD due to interception within a wheat field, which is comparable to the findings by Saleh et al. (2006). We propose to add this information after the addressed sentence.

Line 432-445: I do not really understand this statement. Land heterogeneity leads to VOD-LAI relationship modulated by wavelength? In my understanding the heterogeneity of the pixel is on kilometer scale (~40km pixels?).

The pixel themselves are heterogeneous regarding the land cover and vegetation structure. The PFT fractions of different vegetation types are themselves indicators for this heterogeneity and leading to clear distinguishable VOD-LAI relationships of the different wavelengths (Fig. 4). In the land cover-specific models, the PFTs are not included as predictors hence no description of the heterogeneity of the vegetation structure is explained in these models leading to VOD-LAI relationships of the different wavelength with very similar shapes and amplitudes Fig. 5).

Line 470-471: I am not sure if this can really confirm the penetration properties of L-band. Can you weaken this statement?

Indeed, this is an interpretation by the authors and not proved.

Line 477-478: In line 470 you hypothesize the higher sensitivity of L-VOD to LFMC rather than LAI indicates stronger canopy penetration at L-band. I believe this statement and line 470 are contradicting each other.

Same applies to LAI. Both LAI and LFMC only capture canopy surface in the case of dense canopies. One could hypothesize that this is a reason why both are bad predictors for L-band VOD in forests? Can you confirm this? Might be a nice message.

Yes, LAI and LFMC are not ideally predictors for L-band VOD because both are only sensitive to the top of the canopy in case of dense forests. Having this in mind, LFMC as a proxy for the plant water status of the whole vegetation layer is still higher influencing the L-VOD than LAI.

We propose to add LAI in the addressed lines.

Line 495-496: This is slightly contradicting. L-band VOD and then the reference uses C-band

We aimed to express that the approach by Scholze et al. (2019) for L-band VOD is based on the approach from Rodríguez-Fernández et al. (2018) which were in fact developed for C-band. We agree that the current expression might be confusing. The information that the approach by Scholze et al. (2019) is based on Rodríguez-Fernández et al. (2018) is also included in the following sentence and hence we propose to delete the addressed sentence.