BG-2022-85 – Author's response

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Dear associate editors and reviewers,

We thank you very much for the detailed review of the revised manuscript and the very helpful comments and suggestions. We believe, that these reviews significantly improved the manuscript.

Point-to-point responses

The point-by-point responses to the comments by the handling associate editor and the reviewers are stated below in blue colour. Afterwards, we list all implemented changes which are based on the reviews and the author's responses.

Answers to associate editor Alexandra Konings

Dear authors,

Thank you for submitting this substantially revised manuscript that addresses the reviewer's concern. Reviewer 2 also let me know separately that they found your reviewer response file after submitting their review and were satisfied by it. While the paper has improved substantially, both reviewers still have a large number of comments related to clarity. Please consider these.

In addition, while I appreciate that the "California" region was renamed to the Western US region in Figure 3, I believe this shape will still be a bit puzzling to readers with an interest in this regions' ecosystems. While the exact domain of study is the prerogative of the authors here, it would be helpful to a) clarify the reason for the unusual shape in the Figure 3 caption instead of elsewhere in the text and b) briefly justify why these three MODIS tiles were chosen. It appears the trapezoidal shape in the figure is due to tile h09v05 not being included.

We agree and will include the information in the caption of Figure 3. We added the following sentence: 'The shape of region Western USA is determined by the used MODIS tiles h08v04, h08v05 and h09v04 which form the basis for the retrieval of the LFMC data.'

Indeed, the missing tile h09v05 is causing the trapezoidal shape of this study region. The original aim for this study region was to cover California, wherefore h09v05 was not necessary and only the other three tiles were processed in favour of computing resources. The following sentence was added in Line 182:

The extend of the western USA region is determined by the purpose to cover California, wherefore the MODIS tiles h08v04, h08v05 and h09v04 were necessary and the tile h09v05 was not considered in favour of computational resources.

I also particularly agree with Reviewer 1 in particular that the new Figure 7 is somewhat overwhelming and very difficult to read. Is there a way to present these results in a different manner, or to consider showing only a subset of the figure's columns in the main text? Additionally, what determines the x-axis ordering? The lack of clarity regarding this issue contributes to the confusion about the figure.

We agree with this comment as well as with the comment by Reviewer 1. Please refer to the answer regarding the comment by Reviewer 1.

Best regards,

Alexandra Konings

Answers to reviewer Martin Baur

Dear Authors,

Thank you for revising your manuscript according to all comments. I believe several aspects of the manuscript have improved. Thank you for moving a lot more content into the manuscript and adding new figures. I believe that despite some limits of the methodology, pointed out by the other reviewer, the manuscript presents interesting results and is worth publishing. I have some remaining concerns mostly related to issues already pointed out in my earlier comments. I think they are of more technical nature and should be treated as my attempt to improving the manuscript rather than as intent to stop publication of the work. I leave this to the discretion of the authors but would hope some issues will still be addressed.

- I am still not convinced when it comes to the usefulness of Fig. 1. Each panel is so small it is hardly possible to see any patterns in the study areas (LFMC extent). Although overall patterns might indicate that products and frequencies have distinct sensitivities for some vegetation areas (e.g., rainforest relative to Sahel), these are strictly speaking not study areas and irrelevant for the presented results. I think this can be misleading. I leave this decision to you.

Thank you for your opinion. As this comment seems to be highly related to the comment for line 199 on Figure 1, please refer to the corresponding answer.

- Thank you for adding new figures, they help a lot. Is it possible to aggregate Fig. 7? Fig. 7 has so many data points it is extremely difficult to distinguish any of them. The figure is not really discussed in the text, being referenced only twice. It is a very comprehensive figure but likely overwhelming.

Indeed, Figure 7 contains a lot of information and hence does not fit the purpose of easy visualization of results. We redesigned the figure and include only information about the tree cover, short vegetation and global models based on monthly data and RF. The results for other land cover-specific models are similar to either subplot a) or b). This applies also to 8-daily results but those are excluded in favour of conciseness.

- In some ALE plots the 5-95th percentile range is hardly visible, it is crammed into a small area of the plot while the tails (< 5th or >95th percentile) get a lot of space. I have the concern that these areas, which in my understanding only represent a low number of samples, are over interpreted. I am not an expert when it comes to ALE plots, so I am not sure, but my intuition would be that the dynamics of the ALE curve is less certain in areas with very low sampling. This is most relevant for predictors with outliers (see Fig. 5 c) with AGB). Can you (if relevant) mention this in the manuscript. I couldn't follow some of the conclusions drawn from the ALE plots, mainly because of this issue.

We included also these extreme samples in the ALE plots in order to also account for ecosystems with high biomass, high LAI or high LFMC. As you pointed out, our study area is limited but however still includes some high-biomass forests like in Northern Australia. Using the data based on the 5th and 95th percentile leads to smaller amplitudes (differences in second or third decimal place) but did not change the overall outcome in regards of amplitude order or ratio between them. We agree that the subplots in Figure S5 of the tree cover-specific models are difficult to interpret which is due to the fixed y- and x-axis range but this allows a direct comparability of the different models (changed during the last revision).

For the purpose to increase the interpretability of the feature influences on the VOD data sets, the amplitudes of the ALE curves were included. However, it is valid that the upper and lower percentile of the ALE plots have to interpreted with caution due to smaller sample sizes. We propose to add this information in Line 315 to 320 as follows:

Thereby, relationships outside of the 5 %- and 95 %-ile have to be interpreted with caution due to the smaller sample size supporting these results.

To quantify the influence of the predictors on the target variable (sensitivities), we calculated the amplitude of the ALE curve (Δ_A) as the difference between maximum and minimum of the curve. *A restriction of the ALE plots by the 5 %- and 95 %-ile leads to slightly smaller ALE amplitudes but to the same conclusions as based on the maximum-minimum amplitude which offers the opportunity to exploit the results based on the whole data sample size*.

- Some speculative statements remain, I marked them in the pdf. Please check them and see if you can change them.

I added further comments to the pdf version of the manuscript. Most of them are of technical nature and can be addressed by changing the text.

Thank you very much for your extensive review of the manuscript, especially of the chapters 3.2.1 and 3.2.2. The changes will hopefully increase the understanding and clarity of the results.

The regarding comments are included below as line-specific answers.

Best wishes,

Martin Baur

Line-specific comments:

Line 29: typo ‘8-aily’ instead of ‘8-daily’

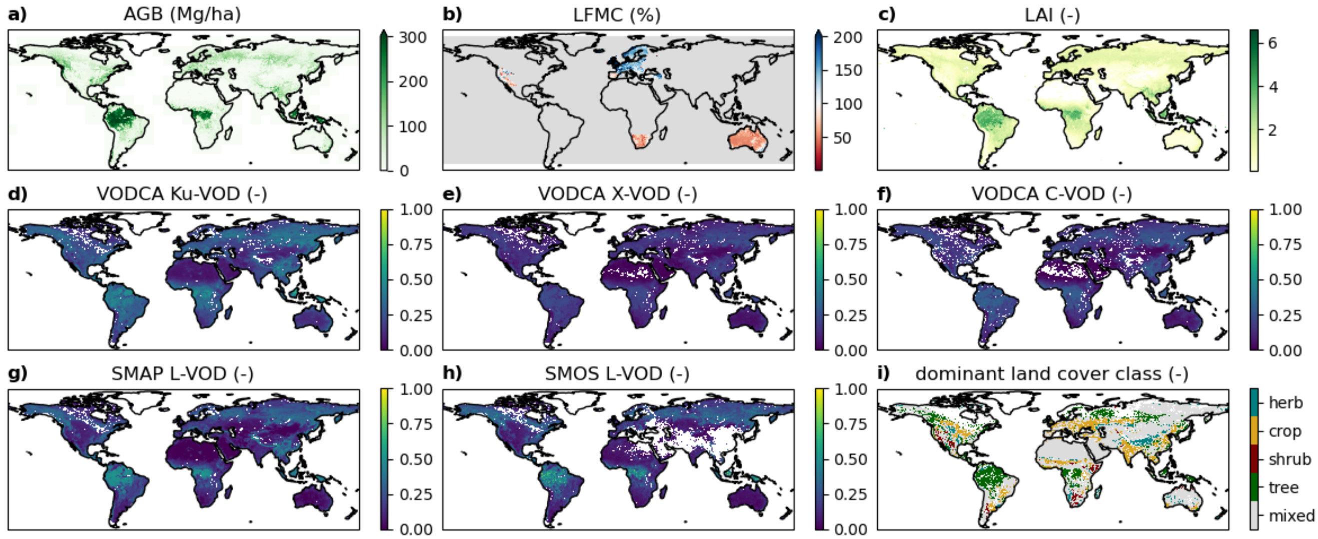
Thank you very much for pointing out this typo. It was changed in the proposed way.

Line 54-55: Usually kept constant for which conditions? Constant for both polarizations? Sentence seems incomplete ... message is clear though.

We changed the sentence as follows: ‘The parameter b is usually kept constant *for one vegetation type and wavelength*, which might be insufficient due to its possible dependency on polarization.’

Line 199 on Figure 1: I am still not fully happy with the different colour-scales for VOD datasets. If all datasets are scaled to 0-1 this seems to be the logical colour-scale for me. Potentially some patterns are more/less visible with different colour-scales. LFMC is just too small to see any patterns and these are the patterns relevant to your study. I leave it to you to decide whether you want to include it or not.

Indeed, a colour-bar scaled to 0-1 for all VOD data sets seems logical. Below is a figure with the scaled colour-bar. However, with the scaled colour bar differences between the VOD datasets are not distinguishable. Hence we keep the original colour bar.



In addition, it is true, that the study sites determined by the LFMC data set are difficult to explore in detail. A figure similar to Figure 3 would then be the alternative, which is in our opinion exaggerated for the purpose of this figure. Here we want to show, that the VOD data sets differ in their spatial patterns in general and to allow the interested reader a brief comparison of the VOD data sets and with the used vegetation properties on a global scale. Our intention here is to show the need for this study, which is unfortunately limited by the LFMC-dataset and therefore points out a need for an extension of this study to other study sites or on global scale.

Line 291 referring to ‘PDP’: can you maybe change it to Partial Dependence Plots (PDP)? Better to know the full name.
Thank you very much, we agree that it is better to insert here the full name. It was changed in the proposed way.

Line 301-302 referring to ‘To quantify the influence of the predictors on the target variable (sensitivities), we calculated the amplitude of the ALE curve (Δ_A).’: Is this just the max to min distance for all ALE curves? How do you address ALE curves that have a max at the very end (past 95 percentile) of the curve?
Yes, this is correct. The regarding sentence was extended as follows:
To quantify the influence of the predictors on the target variable (sensitivities), we calculated the amplitude of the ALE curve (Δ_A) *as the difference between maximum and minimum of the curve*.
Please refer also to the answer regarding your major comment on the interpretation of the 5 %- and 95 %-ile of ALE plots.

Line 392 on Figure 4: Is there a specific reason for no whiskers in a) (60,80)? Looks like for these boxes the medians sit at the box edges. Normally happens with low sampling... maybe explain.

Indeed, the low sampling result in missing whiskers or for no data in missing boxes. The following sentence was added to the caption: *Please note, that sparse data samples or even missing data result in missing boxes or whiskers*.

Line 407-408: Can you please mention which figure you are referring to? It is not visible to me that tree coverage has the highest Delta A. How relevant is it to treat < 5 % and > 95 % areas? Many of the ALE curves have extreme behaviour at the tails of the explanatory variable.

Thank you very much for pointing this out. The feature treeNE is one of main contributors not the first one. This is hopefully now better visible in the revised Figure 7. The sentence was modified as follows:

The coverages of trees are for all models *one of* the main contributors to the VOD predictions *(Figure 7 c).*

For the second part of this comment, please refer also to the answer regarding comment on Line 301-302. Analysis based on 5^{th} to 95^{th} percentile show the same results but with smaller and therefore more difficult to distinguishable values.

Line 411-413: Please refer to supplement figures if possible. It is extremely hard to follow ALE results for the variety of models if the figure is not present.

References of relevant figures were added in chapter 3.2.1 and 3.2.2 on Line 430, 432, 433, 435, 438, 440, 441, 442, 450, 499, 508 and 520.

Line 423-425: Is this mainly because of the increase of SMOS 8-daily past the 95th percentile? I don't know how to interpret this.

This is mainly visible in the part 75%- to 95%-ile where we can see a decrease/ stable relation for SMOS but not for the other VOD data sets. The second referenced sentence is modified as follows:

Interestingly, the relationship between LAI and SMAP L-VOD is more similar to the relationship of LAI and shortwave VODs *(*e.g. X-VOD*)* than the relationship with SMOS L-VOD *(especially shown between the 75%- and 95%-ile in Figure 5 a)*.

Line 426-427: Is there a way to explain this behaviour? Why would there be a spike in the ALE curve at 50%? This might raise some questions regarding the LFMC dataset. It seems like this could be caused by some data irregularity?

We hypothesize, that this spike is a species-specific behaviour or a not well captured relation for herbaceous vegetation pixels in South Africa and Australia (please refer here also to the answer on Line 395-396 in AC2 https://doi.org/10.5194/bg-2022-85-AC2). However, further investigation is required to investigate if this is a real response of the vegetation. We propose to extend the referenced sentence as follows:

The relationship with LFMC is more complex for all VOD datasets (Figure 5 b). From 0 % to 50 % LFMC, the relationships are negative with a negative spike at 50 % LFMC. *We hypothesize, that this spike is a species-specific behaviour or a not well captured relation for herbaceous vegetation pixels in South Africa and Australia, however, further investigation is required to investigate if this is a real response of the vegetation.*

Line 429-430: Is this the case for LAI and AGB too?

Yes, this is correct. It is mentioned here for LFMC because this is especially an uncertainty for the LFMC data set (high LFMC values have in general high uncertainty). We propose to change the sentence as follows:

Despite all relations within the 5 %- and 95 %-ile have to be interpreted with caution, this is especially the case for the 95 %-percentile *of the LFMC-ALE due to the uncertainties of the original data set where* higher LFMC values also have a higher uncertainty (Yebra et al., 2018).

Line 462 on Figure 7: I think this figure has too many data points and is difficult to read. I know I suggested adding more aggregated high level figures, but this figure is not aggregated, more the opposite as it displays too many data points. Please do only keep this if you consider it necessary. It is only referred to twice in the following.

We agree, that the original figure is not aggregated and overwhelming. As this comment is related to one of your major comments, please refer to the regarding answer. The figure is now replaced by a new figure with three separate panels a-c.

Line 485-487: this is hard to spot without reference to specific figure. How generalizable is the behaviour of a land cover ALE curve relative to the global model if the sampling for the land cover model is lower? I am not sure if this is an issue with RF and ALE plots.

The referenced statement refers to Figure 5 c and S6, which is included in the sentence. It is correct, that the global and the land cover-specific ALE plots do not provide a one-to-one comparison. However, if the main data points of a global ALE are corresponding to the main data points of a land cover-specific ALE (i.e. in regards of percentiles), it can be assumed that the ALE plot within a certain data range is driven by this land cover. For this specific case, the 75 %-ile of croplands correspond to a LFMC value of ~170 %, which is higher than for the other land cover-specific models. In addition to the similar shape between the cropland and the global model of the LFMC-ALE after 50 % LFMC, we can assume that the global LFMC-ALE is driven by the cropland LFMC-ALE for this data range.

Line 488-489: I am sorry this is not really visible to me. Seems like any increase/decrease is small for this case and I am not sure whether the threshold of LAI = 2 is based on any quantitative assessment. If not and this is from visual inspection only it might better to drop or simplify the message. In my opinion, when compared to other ALE curves it looks very static.

This is indeed difficult to spot with the current figures. This statement was originally referenced with Figure 5 of the first submitted manuscript, where the y-axis covered a smaller data range. The statement was weakened as follows:

In tree-covered areas (treeAll model), the ALE shows that VOD *marginally* increases with LAI up to LAI = 2 and is then stable or slightly decreases (Figure S5).

Line 489: I am not sure what unimodal means for an ALE curve? Unsure whether this is the correct term and what it means.

‘unimodal’ was changed to ‘non-monotonic’.

Line 490: Does this refer to the shape of the ALE curves in Figure S5? I think all of them are clearly not linear and not monotonic?

Yes, this is correct. Here we want to express that the AGB-ALE is very complex in comparison to the other ALEs. The sentence was modified as follows:

AGB is the dominant predictor for all tree-covered models but the relationship with VOD is *highly* non-linear and non-monotonic*, especially in comparison to the relationships with LAI- and LFMC*.

Line 492-494: What is their fraction relative to the total number of tree pixels?

The specific tree cover fraction of treeAll for the threshold 0.55 is: treeE – 69.9 %, treeN – 46.9 %, treeB – 28.2 % and treeD – 4.8 %.

Line 494-495: The panels in Fig. S5 are so small I cannot really see the SMOS line well. Again the 5-95th percentile area is so small, I am not sure whether this area is more important than the tails.

This comment seems to be correlated to your main comment on the ALE plots. Please refer to the regarding answer.

Line 509-511: Can you weaken this statement? You can only really know for the ones you tested, which are AGB (not time dynamic) and two optical index-based properties.

This is indeed applicable. The expression *ecosystem properties* is replaced with *LAI, LFMC as well as AGB*.

Line 515-517: This is speculative... might be true but probably very hard to support with data. Do you have any heterogeneity metrics of VOD and land cover classes?

We examined the ratio of pixels with a higher threshold to the used data. For Australia 61 % pixels are dominated by land cover with a threshold of 0.7 and for Europe only 50 %. For a threshold of 0.8, one land cover class dominates 40 % of all pixels in Australia and 33 % in Europe.

However, we propose to weaken the statement as follows:

This conclusion is supported by the performance difference between the four studied regions. For example, Europe has a *more* fragmented landscape than most areas in Australia causing mixed effects on VOD within the coarse 0.25° grid cells leading to a lower predictability in Europe than Australia.

Line 567-573: I think the phase shift between L-band and C/X-band or optical indices is not caused by the problems you mention here. It is likely an ecological signal.

Several papers have identified this phase lag. Following paper gives a hypothesis why that is. Feel free to mention it here.

Tian et al. 2018: Coupling of ecosystem-scale plant water storage and leaf phenology observed by satellite

Thank you for pointing this out as well as for the paper recommendation. It is not our intention to explain the time lags by the noisy signal of L-band VOD. To make this clearer, we inserted a line break after the time lag part to separate these two issues. Additionally, we included the recommended paper as follows:

Vaglio Laurin et al. (2020) found a time lag of up to 6 months between SMOS L-VOD and ecosystem functional properties in tree-covered areas in South America and Africa; *Tian et al. (2018) found it between SMOS L-VOD and LAI in tropical woodlands.*

Line 583-585: including seasonal dynamics of AGB could be key for a better prediction of L-band tau.

We agree and the following sentence was included:

Especially seasonal dynamics of AGB could contribute to a better prediction of L-band VOD.

Answers to reviewer Andrew Feldman

The authors gave a thorough effort going through both reviewers' comments. I think this manuscript is valuable. I did have trouble tracking down point by point responses to my comments and was unable to find where all responses/revisions were made to specific points (this may be my fault). Nevertheless, I do appreciate the additional discussion of 8-day vs monthly timescales as well as acknowledgement of effects of plant saturation on results. It is interesting that AGB ultimately has more explanatory power of VOD, but 8-day and for herbaceous plants, LMFC starts to become more important. However, I am surprised that LFMC doesn't have more explanatory value than it does – thought provoking. I have a few more points below that should be considered, and endorse its publication after these are addressed.

Many thanks for your comments and suggestions.

Major:

Line 596-600: These statements are likely too strong. Your results partly support this, but I don't think we have done enough study as a community to suggest that we should neglect plant water status in these model/assimilation frameworks. In fact, Konings et al. 2021 referenced here has arguments against that point. Additionally, Tian et al. (2018) found negative correlations between LAI and VOD in some locations (dry tropics of Africa) indicating a potentially strong role of plant water status that is not correlated to LAI. I think with the existence of several of these counterarguments, these statements need to be tempered. That these approximations may work, but care needs to be taken in light of these counterarguments.

Tian, F., Wigneron, J.-P., Ciais, P., Chave, J., Ogée, J., Peñuelas, J., Ræbild, A., Domec, J.-C., Tong, X., Brandt, M., Mialon, A., Rodriguez-Fernandez, N., Tagesson, T., Al-Yaari, A., Kerr, Y., Chen, C., Myneni, R.B., Zhang, W., Ardö, J., Fensholt,

R., 2018. Coupling of ecosystem-scale plant water storage and leaf phenology observed by satellite. Nat. Ecol. Evol. 2, 1428–1435. https://doi.org/10.1038/s41559-018-0630-3

We agree with your statement. We included the recommended paper and changed the paragraph as follows:

Yet, those applications of VOD require a solid understanding of the biophysical controls on VOD. The relatively high effect of LAI on the short wavelength VODs indicates that data assimilation approaches that only use LAI for estimating the temporal dynamic of VOD (as they were used by Scholze et al., 2019 and Kumar et al., 2020) are valid approximations. *However, other studies also found relationship between shortwave VOD and plant water status (Konings et al., 2021) and negative correlation between VOD and LAI (Tian et al., 2018).* This *indicates that* even models without an explicit representation of plant water status are suitable for VOD assimilation, but *this might not hold for all vegetation types and needs further investigation.*

Line-specific comments**:**

Line 99: this sentence seems strange. Shouldn't it be the other way around? That water content also influences VOD?

The sentence was changed as follows: 'Among others, the studies by Momen et al. (2017) and Teubner et al. (2019) show that the water content of the vegetation *is influencing VOD and therefore is affecting* the relation between vegetation indices and VOD but also the relation between VOD and AGB.'

Line 142: are different microwave parameters used across the different frequencies? It might be helpful for reproducibility's sake to mention this given that single scattering albedo and roughness parameters can influence the temporal dynamics.

We agree, that this an important fact which should be mentioned. The beginning of chapter 2.1.1 was extended as follows:

An overview of the datasets is given in Table 1 and Figure 1. All used VOD datasets are derived from passive sensors using the LPRM algorithm (van der Schalie et al., 2016) to reduce the degrees of freedom of this analysis. *Thereby, for each wavelength a different parametrization was used with the exception for the retrieval of X- and C-band VOD where the identical single scattering albedo was applied. For roughness a constant parametrization is used for Ku-band but a dynamical parameter is used for the other wavelengths. Hence the parametrization essentially differs for the wavelengths. This can affect the similarity of the data sets, but is necessary to allow for valid retrievals in general.*

Fig 1: Why is LMFC over 100% in some cases? A quantitative formula or definition for LMFC is needed to help the reader see why this may happen.

We added the quantitative formula after equation 2 (section introduction):

Whereby LFMC is defined as the ratio of water mass in the vegetation to the dry mass of the vegetation usually expressed in percentage (Konings et al., 2019b):

$$LFMC = \frac{M_f - M_d}{M_d} * 100, \qquad (3)$$

with M_f as fresh mass of vegetation and M_d as dry mass of the vegetation.

and a short explanation what a LFMC value over 100 % means after the description of the LFMC data set in section 2.1.2:

Following Equation 3, LFMC can range from 0 % up to more than 400 %. A value over 100 % means that the vegetation holds more water compared to the dry mass. This depends on the part of a plant and on the vegetation type.

Line 209: maybe note that these are mainly drylands except for Western Europe.

The sentence was extended as follows:

This implies that the 'global' models as stated in the following are indeed inter-continental models restricted to the spatial extent of the LFMC dataset *which are mainly covering drylands except for Europe*.

Line 266-270: how does one interpret differences in land cover and pft models? It is not clear why the authors include land cover as predictors. Ultimately, it is biomass and water content that drive VOD variations (unless I am missing a vegetation structure argument that the authors are making). PFTs can change the VOD-LFMC or VOD-AGB relationships, for example, but should they be used to predict variations in VOD? Additionally, what does it mean if one performs better? Please clarify these points.

The information of vegetation structure will not affect temporal changes of one pixel but yet we hypothesize that it will help to improve the understanding of VOD values regarding pixels of heterogeneous land cover which might not be explained by models referring simply to one land cover type.

We agree, that it is useful to mention why land cover classes and PFT data sets are important for our setup within the section model experiments. The first sentence was extended as follows:

The parameter b (Equation 2) and therefore the relationship between vegetation water content and VOD depends on the vegetation and plant type (Jackson and Schmugge, 1991). Therefore, we account for plant types by using two main classes of regression models to predict VOD.

In addition, the following sentence was added at the end of the paragraph to constitute the model choices:

We hypothesize a better performance of global models compared to land cover-specific models indicating that including information of the vegetation type (i.e. as proxy for vegetation structure) in the model will improve the understanding of VOD, especially for pixels with heterogeneous land cover.