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Comment on bg-2022-85

Andrew Feldman (Referee)

Referee comment on "Assessing the sensitivity of multi-frequency passive microwave vegetation optical depth to vegetation properties" by Luisa Schmidt et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-85-RC2, 2022

Schmidt et al. aim to determine the vegetation canopy information that describes satellite VOD variability across X, C, and L band frequencies. They specifically use regression models to predict VOD with aboveground biomass, leaf area index, and live fuel moisture content. They show mainly monthly scale results with a strong control of AGB on L-VOD and LAI on C- and X-VOD. This is a well-focused and neat study addressing a much needed objective for us to interpret global VOD time variations. The methods are well thought out. I find this to be important work and encourage its publication. I spent some time thinking about each figure and find them to have a lot of value, despite some caveats. The manuscript personally interests me and will indeed influence my future work. Nevertheless, I do have some major points below that need to be addressed. I hope they help the authors improve the manuscript and hope the authors expand on this line of thinking with future articles.

-Andrew Feldman

Major Comments

1) The major concern I have is that LFMC may not adequately partition only the plant saturation amount (i.e., relative water content, predawn water potential of leaves or xylem, etc.). As a result, the results may have a strong bias in overemphasizing the role of biomass changes because saturation considerations are not independently considered by any of the datasets. Namely, as shown in Table 1 of Konings et al. 2019 referenced in this study, LFMC is still a strong function of AGB. LFMC ultimately does not normalize out effects of saturation alone - a change in dry biomass will still influence LFMC even without

a change in plant water status. Ideally, a plant water status parameter should be used that is insensitive to dry biomass. Additionally, LFMC is modeled from MODIS. MODIS is undesirably mainly a function of greenness/biomass (not always strongly a function of plant hydraulics) where use of this dataset relies heavily on how well plant hydraulics and water status are modeled (a highly uncertain modeling process). This is a lynchpin of a study that ultimately relies on partitioning effects of VOD signals into their components, where there is a bias of more certainty in the dry biomass and structure observations and less certainty in those about the plant hydraulic parameters.

One insight that suggests saturation may not be well modeled is that there is a large decline in explanation of 8-day versus monthly timescales (figures S1 and S2). While noise may act more at 8-day timescales reducing its R2 compared to monthly R2 as the authors discuss in lines 460-470, we also know that the short timescales are commonly describing plant saturation dynamics as described in the Konings et al. 2019 and Feldman et al. 2020 papers referenced here. There are also several other published works (see for example: https://doi.org/10.5194/bg-18-831-2021 with error analysis: 10.1109/JSTARS.2021.3124857) based on SMAP VOD that shows the sub-weekly timescale L-VOD variations are indicative of plant rehydration and water loss dynamics, especially in water-limited locations where the authors here tend to find some of the lowest R2 (lines 330-337). I am speculating (with help from fig. 4 that shows some stronger LVOD sensitivity to LFMC), but this issue could additionally be reflected in lower R2 for L-VOD than for X-VOD where potentially the former is more sensitive to canopy water status, especially in tree canopies. That is assuming the L-VOD and X-VOD retrieval processes are equivalent.

The issue is not enough to prevent publication of the study because there are still very valuable results here and reliable large scale partitioned plant water status information is practically unavailable. However, I strongly recommend that the authors explicitly address this issue beyond few sentences in 500-503. Either consider using a more detailed model output of plant water status (some common large scale DGVMs like LPJ and ED2 now have plant hydraulic schemes to provide canopy water status information) in place of the MODIS LFMC and/or do more to explain the uncertainties of the MODIS LFMC product and how that could bias the results as stated about. For example, I am not familiar with the Yebra et al. 2018 study - I think a summary of how they obtain LFMC and potential limitations relevant to its use in this study are needed.

2) I think the manuscript should emphasize the time dependence of the main results more. Even though monthly and 8-day timescales are different, the main predictors tend to be different and previous studies do not suggest that this would only be due to noise; the results nicely give evidence for what we have speculated all along that different aspects of the canopy (dry biomass, water, structure, etc.) influence VOD at different timescales. Specifically, the abstract (lines 31-33), overall results (lines 367-373), and much of the discussion are written as an absolute result. However, these statements mainly appear to be only a function of the monthly timescale. Future work should repeat the same analysis as equivalently as possible across different timescales.

Line-Specific Comments

Line 27: "…level 3 L-band derived…" should it be "…level 3 L-band [VOD] derived…"?

Line 47-52: Please reference some more recent work on the b parameter and other aspects by Kaitlin Togliatti et al.:

Togliatti et al., "Quantitative Assessment of Satellite L-Band Vegetation Optical Depth in the U.S. Corn Belt," in IEEE Geoscience and Remote Sensing Letters, vol. 19, pp. 1-5, 2022, Art no. 2500605, doi: 10.1109/LGRS.2020.3034174.

Line 118: I think the article would benefit from a more explicit research objective or question. Line 118 is very general and broad and has been done in various forms previously. I suggest adding some more nuance so the reader knows what the authors wish to establish.

Line 130: I want to double check that the authors were careful in keeping consistent retrieval processes for all VOD products here. It is a great step to normalize VOD at each frequency (lines 143-146). It is also good they all use LPRM (line 131). However, were there greatly different processes for choosing parameters like single scattering albedo and the modeling of surface roughness? Harmonizing VOD could potentially bias some results in disproportionately influencing certain components of the VOD power spectra (for example, monthly variances of VOD could have been greatly altered while the interannual VOD variance was not influenced).

Line 205-206: The authors means that the "AGB dataset is not representative…" not that AGB itself is not representative. AGB is certainly relevant to all plants.

Line 212: Please explain what "grid-search" means. Is this describing spatial pixels across the globe or a method within the random forest approach?

Line 218-219: I am not sure these definitions are common knowledge. What exactly is used to determine "minimum samples within a leaf", "number of estimators", etc.?

Line 230-234: It would be helpful to point out what the regressors are (f(x)) and what is being predicted? I am guessing VOD is g(mu) and vegetation structure, leaf, water, etc. observations are the (f(x))? Please clarify.

Line 236-237: The land cover maps are technically binary "dummy variables" here that tell whether or not to include AGB (values of zero or one). Or are they being used beyond this?

Table 2: A caution that for the “global” regression, one of the land cover “dummy variables” needs to be left out or else it will bias the regression. For example, if you have four binary regressors (tree, herb, shrub, crop), one needs to be chosen not to be in the regression.

Fig. 2 and Fig. S1: A point of major clarification: It may be helpful to walk the reader through what the authors are looking for here. Are you choosing between GAM and RF? Any other specific decisions? I am not sure how to interpret the difference between the short vegetation and tree cover models. If the tree cover models have AGB, they should be expected to have better variance explained by default because they have an additional parameter that the herbaceous pixels do not have. They are also comparing different regions. It may not be a one-to-one comparison. I am not sure how to also interpret the difference between the global model and land cover model (line 289-299). I am still struggling to understand what their difference is from Table 2 and whether it is a one-to-one comparison. One thing to consider is that models that have more regressors are forced to have higher R2 because adding regressors never reduces the variance explained (at least in least squares regression). These points should be clarified given that they influence the subsequent results.

Figure 3 and line 252: I make heavy use of linear regression models and least squares, but tend to not use ML approaches like random forest. The negative R2 are confusing because they are not possible in least squares, fundamentally because they are the square of the correlation coefficient which is forced to be non-negative. Negative R2 may be because a predictor (a constant like the y-intercept) is left out of the regression and therefore it is not interpreted like other R2. Can the authors clarify?

Figure 4: Is this monthly?

Section 3.2.2 and Figure 5: Given that I did not follow the specific differences between the land cover and global model, I am having trouble interpreting Figure 5 compared to Figure 4. Is the only difference that the global model (figure 4) includes all pixels, but the land cover model are different subsets of pixels? I am guessing the use of “dummy” variable regressors in the global model also has an effect that the land cover models do not? Please clarify.

Line 414: Local factors like what?

Line 419-422: See my major comment: this could in part be a limitation of not having an independent plant water status predictor. Lower prediction of VOD dynamics in these regions is not expected because VOD and soil moisture errors tend to be smaller in regions with herbaceous vegetation. R2 should therefore tend to be higher in these regions if we have relevant predictors with R2 in forested regions decreasing due to noise. See results from recent VOD error quantification work: see https://doi.org/10.1016/j.rse.2019.111257 and 10.1109/JSTARS.2021.3124857

Line 444: "Varies by wavelength." This is microwave wavelength, correct? (as opposed to a timescale-dependent wavelength related to power spectra of the time series)