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Interactive comment on "Impact of temperature and water availability on microwave-derived gross primary production" *by* Irene E. Teubner et al.

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This work presents a model to estimate Gross Primary Production (GPP) globally from a carbon sink driven approach. In particular, the paper aims at improving previous modelling of GPP as a function of the vegetation optical depth (VOD; Teubner et al., 2019) by including the effect of temperature on the autotrophic respiration. Authors explain that the model is based on the fact that VOD is a good proxy of above-ground biomass (AGB). The link between residuals of the model and the drought index SPEI is also analysed. The results presented show an improvement of model performance in terms of temporal dynamics, especially in non-tropical regions. Interestingly, results also report that the presented model does not require complementary information from precipitation or drought indicators.

Despite that the results presented are consistent with previous works and show interesting contribution to GPP modelling, I have important concerns that have to be addressed before publication. The most relevant are related to the lack of penetration through the vegetation canopy of the X-band VOD (which is not a good proxy of biomass if compared to other frequency bands), and to the need of further explaining the modelling framework both in the introduction and the methods sections. These comments and other major and minor proposals for improving the paper are detailed hereafter:

Major comments

1. Although the paper can be well understood if the reader knows previous literature on this topic published by the authors, it is necessary that the modelling approach (i.e., main ideas and equations from previous works) and the implementation in the current paper are explained in more detail. In particular:

a. I suggest that first paragraphs of the introduction provide a more detailed description of the framework explained in Teubner et al. (2019), including if necessary some equations (e.g., equations 4 to 6 in Teubner et al., 2019).

b. Please, provide more detail on how you are computing and including into the model the different variables (Section 2.2). For instance, does the term "VOD" refer to VOD time-series? If so, how is the time-domain processed (raw data, smoothing, etc…)? How is the variable computed?

In summary, please extend the text to provide enough information for readers that do not know your previous work.

2. The basis of the work is the fact that VOD is a good proxy of AGB. Nevertheless, it is very important to note that X-band VOD (hereafter XVOD) has poor capacity to penetrate the vegetation canopy, and therefore it is very limited to accurately track AGB in regions with dense vegetation. While the AGB - L-band VOD relationship shows low

saturation in tropical regions, X-band is not the most appropriate frequency to be used in these areas as a proxy of biomass (e.g., Brandt et al., 2018; Rodríguez-Fernández et al., 2018; Chaparro et al., 2019). Actually, even in low carbon density areas, XVOD is more representative of vegetation cover than it is for biomass, while lower frequencies (L-band VOD; hereafter LVOD) still have improved capacity to track AGB in these areas (e.g., see Fig. 9 in Chaparro et al., 2019).

It is very likely that this limitation explains the lack of improvement of the model in the tropical regions (Fig. 3b) and the low correlation between the model and the benchmark datasets in regions such as the Amazon (Fig. 3a). In addition, this could also explain the saturation of the partial dependency plots (Fig. 1) at high VOD and T2M values (darkest lines in the first panel, probably representing vegetation-temperature conditions in the tropics) and at mdnVOD values above 0.4 (i.e., dense vegetation; third panel).

The application of XVOD is justified in the paper by the higher correlation between XVOD and GPP if compared to the LVOD-GPP correlation (Teubner et al., 2018). Nevertheless, it is important to note that the GPP benchmark datasets have an important contribution from visible-infrared (VIS-IR) indices (EVI, LAI, MIR, NDVI and NDWI, as stated in l. 96). I think it is expected that GPP datasets based on VIS-IR indices show a greater correlation with XVOD than with lower frequency VOD data, because both of them capture the same layer of vegetation (top of the canopy). In contrast, I would expect greater correlations with in situ GPP FluxNet data (Fig. 2a and Fig. 2b) if GPPvod and GPPvodtemp were computed using L-band VOD. Although it is not a global dataset, FluxNet in situ information is not conditioned by physical properties of remote sensing sensor measurements, so it is probably the most "independent" tool the authors have for evaluating the accuracy of the GPP estimates.

For all these reasons, it would be very interesting if the authors include new GPPvod and GPPvodtemp models based on L-band VOD and validate their accuracy using in situ FluxNet data (i.e., including them in Fig. 2). They could show (and compare) the

resulting GPP estimates between different frequencies and, importantly to preserve the scope of the paper, between GPPvod and GPPvodtemp models. However, I am aware that this could move the work slightly beyond its initial scope, as it adds another factor (i.e., frequencies) in the comparisons. I encourage the authors to work on this possibility, although it is up to them to finally incorporate this change or to keep only XVOD in the paper. In any case, they must discuss all the possible implications of using XVOD. Within the discussion, they should address at least the following points/questions:

- It is stated that "the VOD-GPP model relies on estimating carbon sink terms, [. . .], based on VOD as a proxy of aboveground living biomass" (l. 35-37). To what point is this true, according to the facts that XVOD is more representative of vegetation cover than of biomass, and that lower VOD frequencies have enhanced capacities to capture biomass? Please clearly explain the possible limitations of the approach.

- Please, discuss about the saturation effects in Fig. 1 (first and third panel; see my comment above). Are they likely to be linked to XVOD saturation in the tropics? If so, which are the implications?

- In l. 197, it is mentioned (referring to tropical regions) that "[in these regions] sensitivity to temperature is also low, which makes the interaction term mainly controlled by VOD." If I correctly understood plots in Fig. 1, would it be more precise to affirm that it is mostly controlled by , as other dependencies (VOD, mdnVOD) saturate in tropical regions?

3. The GPP estimates (GPPvod and GPPvodtemp) are calibrated using FluxNet in situ data. Also, both FluxNet and FLUXCOM data (an upscaling of FluxNet) are used as reference datasets for evaluating GPP estimates. I think that, consequently, reference datasets may not be fully independent from GPP estimates. To what extent? Which is the contribution of FluxNet data in the reference datasets and in the estimates? This has to be acknowledged and possible implications discussed.

In addition, authors should try to guarantee at least one year of "fully independent" comparison between estimates and FluxNet/FLUXCOM data. I suggest they could calibrate the model by leaving one year of data apart (e.g., use 2004-2014 for calibration) and apply the remaining data (2003) for fully independent comparisons. They can show these new comparisons in supplementary materials and refer to them to show consistency/inconsistency with the full-period comparisons of Figures 2 to 6.

Minor comments

- Lines 5-6: "VOD-GPP model generally showed good agreement" → Please quantify (e.g. correlation coefficient).

- L. 6: "tended to overestimate" → By how much? Please quantify.

- L. 13: "Our results reveal an improvement" → Please quantify this improvement (e.g., increase on the average correlation).

- L. 14: "This increase in temporal dynamic" → "This improvement in temporal dynamics."

- L. 19: can you mention which are these regions?

- L. 19: "between [. . .] with" → "between [. . .] and"

- L. 25: provide → provides

- L. 25 to 30: you may want to include other references which are explicitly linked to water content: e.g., Feldman et al., 2018; Tian et al., 2018.

- L. 28: Chaparro et al., 2019 → Chaparro et al., 2018 (this is different from the "carbon-stocks work" in Chaparro et al., 2019). Add the new reference to the references list if you want to keep it in the text.

- L. 70: maybe saying "only a few years" is a bit excessive (e.g., SMOS spans >10 years). Try using another expression, please.

- L. 83-85: "During data processing. . ." → Please move these lines to the methods section.

- L. 118: "T2M was used in our analysis, since this parameter is most common for describing the temperature dependency" → Please add some references to show the common use of this variable.

- L. 121-122: "aggregated to 8-daily estimates" → Please, specify that this is done to match MODIS time-steps in case it was your intention.

- L. 162: "savitzky-golay" → "Savitzky-Golay."

- L. 170: "are consistent the previous" → "Are consistent with the previous."

- Figure 1: Please add marginal distribution rug plots to each panel. Name the panels as "a" to "d" and complete the figure caption with explanation of each panel.

- Figure 2: add GPPvod as another panel for comparison with GPPvodtemp and benchmark data. Also, I find that the blue to yellow colorbar shows very low contrasts. To me, it is difficult to appreciate color gradients in the figure. You could use other colors (e.g., blue to red?) or saturate the colorbar at the (e.g., 95th, 99th) percentile to improve contrast.

- L. 218: "For a region in Europe" → Please add coordinates also here, as well as the general situation (e.g., "Central Europe") to help the reader.

- L. 218: "increase" → "increase in".

- L. 227-229: there is no a verb in this sentence, please rephrase.

- L. 233: "between [. . .] with" → "between [. . .] and".

- Figures 4 and 6: please define in the text what are "zonal means".

- Figures 5 and 8: please, could you make each panel wider? Then there is more place for seeing interannual variability in the figures.

- Figure 6: Add GPPvod as another panel for comparison with GPPvodtemp and benchmark data.

- L. 236: “given that correlations in these regions are high” → Authors probably mean correlations between GPPvodtemp and benchmark data. Please specify this, because as you explained correlations of residuals in the previous sentence, it can be confusing.

- Figure 7: to improve the boxes for regions, you could use colors different than those from the colorbar (e.g., light green instead of blue?).

- L. 282: “increase” → Do you mean “improvement”?

- L. 338: "between with" → "with".

- Figure A1: please add GPPvod as well as a map of the median differences between GPPvod and GPPvodtemp. Also, note that the contrast in the blue to yellow colorbar could be improved, or colors changed to a blue-red scale.

- Figure A3: please add GPPvod.

- Figure A4: this seems an interesting result, but I do not fully understand what do you mean by “scaled latitudinal” distribution. Could you please explain this?

- Table A1: it could be useful to detail the dominant land cover in each station. Then, the reader will be able to see which vegetation types have been included in calibration of GPP estimates.

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