

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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Impact of temperature and water availability on microwave-derived gross primary production by Irene E. Teubner

This study evaluates the capabilities of VOD to provide new information on the changes in vegetation productivity at a global scale. Specific improvements obtained by accounting for temperature effects on autotrophic respiration are analyzed.

I found that interesting results are presented. However, I think significant improvements should be made. As I am not familiar with the studies by the authors on this topic, I found it is difficult to understand many points in this manuscript, unless, maybe, I read in detail all papers published before. Basic elements of the modeling approach

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published before should be given here, so that the paper is “more autonomous”. I present below many points to be improved, so that readers who are not familiar with the papers published by the authors, may understand the results and the discussion

I have 4 main comments which should be accounted for before publication

1) I think, the lack of improvement in the tropics could be related to the low sensitivity of X-VOD to biomass changes, which was found in many regions of the world but particularly in the tropics. This should be better discussed and accounted for throughout the manuscript.

For instance, many references discussing the capabilities of VOD to monitor biomass are missing. Cf below references on this topic including applications on biomass changes / productivity monitoring, to better account for and reflect the published literature on this topic (Brandt et al., 2018, Fan et al., 2019; Al -Yaari et al., 2020; Lei et al., 2020; Frappart et al., 2020).

line 14: “regions outside the tropics”, many studies have shown that X-band and C-band VOD present saturation vs biomass (close to 200 t/ha). So, how do you expect to monitor GPP from VOD indices that saturate over dense vegetation forests, which represent a large fraction of the vegetation cover in those regions.

line 72-74, I did not review these previous papers, but I think it is quite surprising that X-VOD provide best agreement with GPP by considering “sink terms” related to biomass changes. X-VOD is better related to LAI/NDVI (and thus to photosynthesis and “source terms”), while L-VOD is better related to biomass changes (see Li et al., 2020).

Still based on L-VOD, Tian et al., 2018, found a decoupling between seasonal changes in VOD and in the leafy/biomass component in dry tropical forests. This should explain some errors too in the tropics, when attempting to relate VOD changes to vegetation productivity (?)

2) I found it is very difficult to understand section 2.2, except if you are an expert in

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this specific modelling approach Temperature is an important parameter in Ra but also in other key processes such as photosynthesis. How can you be sure that only the Ra(T) dependence was accounted for here? Because I cannot see any deterministic equations relating Ra to temperature: in Eq 1 and 2, is it fully a machine learning approach that you used, isn't it?

As I'm not expert of this kind of regressions and many terms are unclear to me. Maybe, it is very specific to me, but maybe it will apply to many other readers: Better explain what is "VOD time series", "delta VOD", "mdn VOD": over which time step? considering daily, monthly or yearly values? do you compute mean of delta VOD, etc. . . ? What is the time step of Eq 1 & 2: daily? "spline terms for representing 2-dim functions": what do you mean? which 2-dim parameters are considered here? "smoothing factor"?, etc.

3) Did GPP-VOD-temp showed improvements vs GPP-VOD, when considering correlation of residuals with SPEI? Since the present study focuses on analyzing possible improvement of the new GPP-VOD-temp, intercomparing residuals vs SPEI with GPP-VOD is key and should be added in this manuscript. The present description of results is a bit lengthy and should be reduced to the profit of the above inter-comparison.

4) I found that conclusions are much more nuanced considering the relative improvement obtained with the new GPP-VOD-temp product. This should be better reflected in the abstract which I found too optimistic.

Minor line 16- 20; it seems to me the two sentences are a bit contradictory line 25-30, Cf above remarks on C- and X-VOD saturation, many papers were published based on SMOS L-VOD and none is mentioned in this short review. This short review should be more "opened" line 35: "VOD as a proxy of AGB": I guess very few FLUXNET sites are available in relatively dense vegetation sites, and more generally in the tropics. The VOD-derived GPP is manly calibrated based on data in temperate climate? Line 43: " as a necessity": what do you mean here; not so convincing as a scientific term. Lien 50 define what is Q10? Line 94: why not using FLUXCOM RS + Meteo, which

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has more input and could be more reliable. I do not understand the reason given here “our approach is mainly based on RS..”. This is a not a good reason to me (?) Please provide more information on Fluxnet sites used here (maps of locations, main vegetation types, etc.) Fig. 1 saturation for VOD > 0.6, can this be related to the saturation of the VOD / Biomass relationship? (0.6 corresponds very well to the saturation level of X-band) Line 179, add that you consider 8-daily values. Line 218 “increase of” ? -check grammar in line 223-225. -Figure 3 and 4: is this based on the whole study period (please add the information caption and check throughout)? -Figure 4, the overestimation in the tropics seems to be much more significant than the very small decrease outside -35° , $+60^{\circ}$. Can we really consider this is “improvement”? -add site for Fig 5 in the Fluxnet map. Why selecting this site: is it representative of more general results (specific canopy types, climate?)? -line 239; “holds true” is too strong; here you only find no contradiction on a specific point; mathematically it is not right at all to say that the hypothesis is validated. It is only one indication you go the right way. . .

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