Biogeosciences Discuss., author comment AC2
https://doi.org/10.5194/bg-2022-25-AC2, 2022

Reply on RC2

François Jonard et al.

Author comment on "Observed Water- and Light-Limitation Across Global Ecosystems" by François Jonard et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-25-AC2, 2022

Interactive comment on "Observed Water- and Light-Limitation Across Global Ecosystems" by François Jonard et al.

François Jonard et al.

Francois.jonard@uliege.be

Reviewer #2: Jonard and colleagues identify regions globally whos productivity during the growing season is (temporarily) limited by water or light availability. Based on S5-p Tropomi SIF, SMAP soil moisture, MERRA shortwave incoming radiation and MODIS LAI they identify whether the SIF in a pixel is constantly limited, constantly unlimited by water/ light, or whether a break point exists, where SIF changes from limited to unlimited behaviour. They find large regions for both the water and the light limited regimes which exhibit a breakpoint, claiming that the assumption of a linear relationship to SIF as in many studies does not hold. Particularly interesting is the identification of a breakpoint for the light limitation in many pixels. The locations of the breakpoints along gradients in soil moisture and light show a relationship with mean precipitation and soil type.

The authors have done a good job in outlining the scope of their study and the aspects that render it unique from related work in the literature. The methodological approach is overall sound and limitations openly discussed, except for one aspect related to the definition of the growing season that I will outline in more detail below. In my opinion, there are several aspects that need to be included (at least in the discussion, if not in also in the analysis), and they will render it more convincing (see below). The topic is relevant and fits Biogeosciences, I suggest publication after addressing the following points:

Authors: Thank you for your constructive comments on our study. We will address your comments as discussed in the responses below.

1) Given that it is a purely observational study, the limitations of the remotely sensed data streams at the basis of the analysis need to be taken into account and discussed, which so far has not happened. I suggest to include in the manuscript information on

whether there is quality control applied to the observations. Are there regions where the observational coverage is critically low during the growing season, such that uncertainty in the selected model becomes large? How is persistent cloud cover during the wet season handled, do I understand correctly that with the given definition of the growing season we rather evaluate the cloud-free dry down period in seasonally dry ecosystems? SIF penetrates through partial cloud cover, but persistent full cloud cover is critical as well. Please note, that LAI is not purely observational but also based on a model.

The different data streams cover different periods, why is that?

Authors R1: We will add in the methods section information on quality control applied to the observations (e.g., flag for pixels with low count, flag for SIF data with effective cloud fractions of < 30%, …).

We agree that LAI is not purely observational and propose to use NDVI.

We will clarify in the text (lines 109-110) that we used the same data period for the main analysis, but a different data period to obtain the LAI (or now NDVI) climatology.

2) The growing season is defined as the 6 months around the annual peak LAI, and the authors tested other definitions of the duration of the growing season (more details on which ones those are?) with qualitatively similar results. But I wonder to what extent regions with different length of the growing season can be compared to each other based on a period with fixed duration around the peak of LAI. Depending on the actual length of the growing season (I mean between the start of the green-up and the end of the greenness period), the 6 (or x) months around the peak will cover a different fraction of the growing season in different pixels (e.g. in higher latitudes or arid regions with only a short wet period, the 6 months might include a rather large fraction of the shoulder seasons or even the non-growing season, which will not be the case for temperate regions). As you discuss as well, the main driver of productivity can change over the course of the growing season, and also the year. So how can this affect your results? What about potential alternative definitions, such as to focus the analysis on the peak of the growing season of each individual pixel, defined as e.g. half/30% or similar of the duration between start of green-up and end of green period around the peak LAI (neglecting potential asymmetry in the growing season)?

Authors R2: We do agree the 6-month definition is a bit arbitrary. Similarly to what the reviewer suggests, one method that accounts for variable lengths of the growing season is, instead of the three months before and after the peak LAI, use the time before and after peak LAI when LAI is X% of the growing season maximum (or 95th percentile) which should be more sensitive to timing of the beginning and end of the growing season. We will assess which percentage to choose for X: too high of a value will more confidently remove the shoulder seasons, but will also risk losing too many data pairs. We may avoid a method that relies on estimation of green-up and brown-down timing because determination of these events can become uncertain in regions with multiple growing seasons or multiple growing season peaks. The magnitude threshold method we propose should be robust to more complicated seasonal features.

3) It is well appreciated that the authors tested their approach with other SIF data sets as well and come to qualitatively similar results. Although the results are not shown (which is fine, but would be nice to see in the SI), to me it would be important to clarify in the manuscript how this was done. Did you use the same gridding, and also the same years, or did you exploit the longer records of OCO2 and GOME2 SIF and did the analysis for more years, potentially more robustly? Does the fact that the study period focussed on the particularly dry years in central Europe in 2018/19 affect your conclusions for this region?

Authors R3: We agree with the reviewer's comment, and will clarify these points in the SI. Results based on GOME-2 SIF data will be shown in the SI with more years of data, along with appropriate methodological discussion.

4) The revelation of the two-regime behaviour for light limitation is something particularly novel in the manuscript. Its discussion is very short, I strongly suggest extending the discussion of this point and the related thresholds.

Authors R4: We agree, we will add more discussion about the two-regime behaviour for light limitation.

5) I like the summary of related literature in the introduction very much. However, one aspect goes missing in my opinion, and that is the aspect of the time scales. The studies cited focussed on different time scales, and the main drivers of productivity might change across time scales (Linscheid et al. 2020, Biogeosciences, 10.5194/bg-17-945-2020), and this aspect should be clarified in the introduction.

Authors R5: This will be clarified in the Introduction

6) The last part of the conclusions is missing. Parts of the conclusion read very similar to parts of the abstract.

Authors R6: We will revise the conclusion.