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Reply on RC1

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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-AC1, 2022

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

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Reviewer #1: This study investigates the emergent relationship of vegetation functioning with water and energy availability across the globe. This is done largely with satellite-derived datasets, where vegetation functioning is characterized with Sun-induced fluorescence data and energy availability is represented by the photosyntetically active radiation. The authors test different linear models including breakpoints to show that the vegetation response to climate is non-linear in many areas for both energy and water limitation, as expected from physical principles. Recommendation: I think the paper requires major revisions.

The topic of this study is interesting and timely. The response of vegetation to climate drivers is well known at small spatial scales from laboratory and field experiments, but it remains more unclear at larger spatial scales. At the same time, these vegetation-climate interactions are particularly relevant as they affect surface climate and need to be taken into account, and accurately captured by Earth system models, e.g. for projections of future climate scenarios. In this context, satellite-based datasets present an excellent opportunity to study these processes, and involved non-linearities and thresholds at model-relevant spatial scales.

I think that the manuscript is easy to read and understand and is a great match for the readership of Biogeosciences. However, before it is ready for publication some concerns regarding the analysis approach and robustness should be resolved, as detailed below.

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

General comments:

(1) I think the fact that the seasonal cycles are not removed from the SIF, PAR and soil moisture data is a major shortcoming in this analysis. This way, confounding impacts by for example temperature can introduce artifacts in the results. For example, the positive correlation between soil moisture and SIF across a band across Canada in Figure 2 does not make sense from a physical point of view and could be related to such effects. I realize that the authors mention this point in section 4.3, and think it would be important if they could actually demostrate the negligible impact of the seasonal cycles by showing that similar results can be obtained with a shorter growing season of 1-2 months. Another way could be a synthetic experiment with e.g. evapotranspiration, radiation and soil moisture data from reanalysis where a similar time period can be used and the analysis can be done without removing the seasonal cycle (as here), and with the removal (while a representative seasonal cycle can be computed from many available years).

Authors R1: We agree with the reviewer's comment, and will deal with non-stationary seasonal effects more directly. Due to the signal-to-noise ratio inherent to remotely sensed SIF, we were concerned that further subsetting the time series would inhibit proper parameter estimation. We will perform the analysis on 3-4 months (for comparison with our 6-month analyses), though we expect the sample size may be too low. However, we will also reassess the parameter estimates using deseasoned anomalies of SIF, PAR, and soil moisture. In these analyses, we still need the mean magnitudes to find regimes – regimes are dependent on the magnitude of the state variables. Determining the soil moisture and PAR thresholds will follow still from the analysis on the mean state (Figs. 6b and 7b). We will then use those thresholds to estimate the slopes in the respective limiting regimes for PAR and SIF based on deseasoned SM, PAR, and SIF. We expect that this will provide a test for the role of seasonality on the results.

(2) There is no consideration of uncertainty for the performed analyses. In this context it would for example be informative to see some goodness-of-fit metric for the for the chosen model type (linear/two-regime/zero-slope), and possibly to introduce another category in case no reasonable fit was found for any of these types. Further, it would be interesting to which extent the model selection holds when for example synchronous bootstrapping would be performed for soil moisture and the other investigated variable. Moreover, in this context it would also be relevant to understand to which extent the results depend on the selected input datasets. I appreciate that the authors mention this aspect in section 4.3 in the case of alternative SIF datasets and think it would be important to demonstrate this in the supplementary material. Additionally, also the role of the soil moisture dataset should be tested by replacing the SMAP data with e.g. satellite-derived ESA-CCI soil moisture (https://www.esa-soilmoisture-cci.org/data) or upscaled in-situ soil moisture from the SoMo dataset (https://springernature.figshare.com/collections/Global_soil_moisture_from_in_situ_measurements_using_machine_learning_-_SoMo_ml/5142185).

Authors R2: We agree with the importance of considering the uncertainty in our results (and in results more broadly). Our goal here is to provide analyses of the spatiotemporal patterns of physical light and water limitation on SIF. Major sources of error in these analyses include: 1) observation errors from all of the data streams (SIF, SM, PAR), 2) model structural errors, 3) parameter estimation errors, and 4) omitted variable effects. We should be more clear on these sources of uncertainty in our discussion, and will add manuscript text to clarify each of these and their roles.

In our revision, we will quantify these more directly as possible. Our models (simple piecewise linear) are intentionally simplistic to allow readers to understand the model structural limitations. To quantify these, we propose to show the coefficient of determination (R2) of the fit for each pixel.

Observational errors are important as well, particularly when they are likely to be of similar or larger magnitude than the errors of using simple empirical methods. While it would be possible to perform the same analyses on different soil moisture data sets, we wish to avoid data sets that 1) significantly rely on modeled relationships between surface variables, 2) perform rescaling to match different sensors and microwave properties, or 3) will have major spatial representativeness uncertainties in comparison with aggregate SIF and PAR. This limits us to either the SMOS or SMAP datasets, which have a trade-off between a longer data record (SMOS) or low-level radio-frequency interference mitigation (SMAP). We selected SMAP in this case, particularly in the face of noisy data streams from satellite SIF observations. We don't feel that the addition of more SM data sets is likely to add physical insight, it will lead towards more of a combinatorial data set comparison study, which is not our intent. That said, SIF is both far noisier and a more complex proxy of ecosystem productivity status (as compared to satellite soil moisture), and we do see real benefit in terms of the robustness of response of physical relationships in assessing the impact of different SIF datasets rather than soil moisture datasets. Following the reviewer's suggestion, we propose to present the impact of different SIF data sets. The impact of the SIF dataset (TROPOMI vs GOME-2) will be demonstrated in the supplementary material only to qualitatively provide an uncertainty test on the overall spatial patterns of parameters rather than provide an analysis of differences of the datasets.

To evaluate the interactions between observation errors and our parameter estimation process, we could also perform a bootstrapping procedure to estimate the variance of the soil moisture threshold and slope on a randomly selected subset of pixels. We expect this to be of minimal utility to our main objective of giving first-order analyses of the spatiotemporal patterns of water and light limitation (rather than creation of a parameter data set), and suggest that this is beyond the scope of the article.

(3) I like that the authors recognize and determine non-linearities in the soil moisture-climate relationships. I think they could move a bit further in this direction by assessing the degree of non-linearity, for example as the difference in BIC scores between linear and non-linear models in each grid cell, or using the bootstrapping approach mentioned above. Further, it would be interesting to evaluate the spatial distribution of non-linearities as displayed in Figures 6a and 7a against climate and land surface characteristics, as done for the thresholds.

Authors R3: While we appreciate the path the reviewer suggests here, we also need to frame the purpose of such analyses in this study. We are using non-linearity as a model structural representation of limitation versus non-limitation regimes for SM and PAR. This is based on well-understood theory that if one variable is abundant, its marginal changes likely have no impact as a predictor (see for example citations below for thresholding relationships of some land surface variables). Our two-regime model then treats all limitation behavior as linear, which is oversimplified, but valuable as a first-order demonstration of the existence of limitation regimes, and to quantify how common each regime is for a location. The degree of nonlinearity will be a function of some landscape characteristics, but also of the local exogeneous forcings (e.g., the patterns of rainfall that might lead one location to switch between water and energy limited regimes). Disentangling these is not straightforward, and we wish to be careful to not oversimplify this for readers.

Furthermore, statistical tests for how nonlinear a relationship is rely on an assumption of the functional form of the nonlinearity, and therefore any metric for the "degree of nonlinearity" that would result from such a test is a large function of this assumed relationship. Our functional form (linear in the limitation regime, constant in the non-limitation regime) is the simplest possible for regime definition, but not optimal for characterizing the full shape of a non-linear limitation relationship. To do so would require, 1) more theoretical understanding of what these shapes should be, and 2) a full sampling of the exogenous forcings space, which is not possible in “natural experiments” such as this.

Additionally, we stress that our current test is already effectively a test for whether the relationship is nonlinear. The current model (see Fig 4) tests for whether or not linearity suffices to describe the relationships between SM and SIF and PAR and SIF. If a BIC score for the two-regime linear model is lowest, this means that linear behavior does not suffice. Differences in BIC scores are often used heuristically to compare goodness of fit between models, but do not have interpretable meaning in comparing one pixel to another (i.e., across data sets). Since inter-dataset comparison of information criteria is not possible, we propose to instead compute the AIC and QIC parameters to show whether other information criteria also detect nonlinearity (the existence of dual regimes) in the same pixels that BIC does. We expect BIC will be the most conservative of the common information criterion (among AIC, QIC, and BIC) which will tend to penalize and reject higher-order (nonlinear) models to a greater degree. Therefore, AIC and QIC may select dual regimes (nonlinearity) more readily.

References:
Hydrological storage length scales represented by remote sensing estimates of soil moisture and precipitation. Akbar, R., DJ Short Gianotti, KA McColl, E. Haghighi, GD Salvucci, and D. Entekhabi - Water Resources Research, 2018

Soil and atmospheric controls on the land surface energy balance: A generalized framework for distinguishing moisture□limited and energy□limited evaporation regimes, E Haghighi, DJ Short Gianotti, R Akbar, GD Salvucci… - Water Resources Research, 2018

Satellite and station observations demonstrate water availability's effect on continental□scale evaporative and photosynthetic land surface dynamics, DJ Short Gianotti, AJ Rigden, GD Salvucci… - Water Resources Research, 2019

Landscape□Scale Plant Water Content and Carbon Flux Behavior Following Moisture Pulses: From Dryland to Mesic Environments, AF Feldman, A Chulakadabba, DJ Short Gianotti… - Water Resources Research, 2021

Satellite□based assessment of land surface energy partitioning–soil moisture relationships and effects of confounding variables, AF Feldman, DJ Short Gianotti, IF Trigo, GD Salvucci… - Water Resources Research, 2019

(4) I appreciate the analysis of the spatial patterns of the thresholds displayed in Figure S3 against climate and land surface characteristics. I think this analysis should additionally cover the role of the vegetation type (averaged across each grid cell), as this also affects the SIF-climate relationships. I realize this is mentioned by the authors in the conlusions section, and encourage them to include this into the analyses.

Authors R4: We propose to add a figure assessing the spatial patterns of the thresholds as a function of vegetation types using IGBP land cover classifications.

(5) I did not quite understand why LAI was only obtained from a relatively short 4.5 year period only. The MODIS record extents further back in time, and a longer data record would allow to infer a more robust seasonal cycle.

Authors R5: We propose to use MODIS data from 2003 to 2020 to get the seasonal vegetation cycle to generate more confidence in the vegetation seasonality.

Specific comments:

lines 38/39: this is not only true for increasing temperatures

Authors R6: We agree and we will modify the text accordingly in lines 38-39.

line 41: what is meant with "rate-limiting"?

Authors R7: This will be clarified in the text.

lines 42 & 62-68: the work from Li et al. (2021) is similar and relevant in this context and could be mentioned here

Authors R8: The work from Li et al. (2021) will be cited.

line 116: "day" should be singular, and a space should be removed in "Sentinel-5"

Authors R9: This change will be made accordingly in line 116.

lines 142/143: what is the source of the soil texture data?

Authors R10: Texture data were obtained from the SoilGrids database. This will be added to the manuscript.

lines 169-171: Why not selecting the growing seasons as the 6 months with the highest LAI, independent on whether or not they would be consecutive, in order to better capture the highest LAI months in regions with more than one peak in the seasonal cycle?

Authors R11: as mentioned in the manuscript, there are many approaches described in the literature to define the growing season, but we believe that the approach we have followed is sufficient to characterize the active growing season encompassing the primary water and energy interactions with the carbon cycle. As we will be performing additional analyses using deseasoned time series, we will clarify our approach and its justification in the manuscript.

line 177: replace "." with "x"

Authors R12: We will replace the multiplication sign with "x"

lines 291-296: the work from Denissen et al. (2020) is similar and relevant in this context and could be mentioned here

Authors R13: The work from Denissen et al. (2020) will be cited.

lines 332-334: temperature limitation of vegetation functioning might play a role here, as mentioned also a few lines below

Authors R14: This will be clarified in the text.

Figure S1:

Why is LAI in the mean seasonal cycles in b) and d) always the same across some consecutive days?

Authors R15: This is because LAI data are only available every 8 days. Figure S1 will be changed as we will perform new analyses using a longer time series of MODIS data.