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Biogeosciences
Discussions

Interactive comment on "Global biosphere–climate interaction: a multi-scale appraisal of observations and models" *by* Jeroen Claessen et al.

Anonymous Referee #2

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The manuscript explores the biosphere-climate interactions at global scale. The method, based on a Granger Causality framework, quantifies the climate impact on vegetation and the vegetation feedback on climate using satellite observations. The same approach is then applied to four ESMs and differences between data and model results are discussed. The study is well written and potentially interesting as – to my knowledge – is the first work aimed to isolate the climate-vegetation interactions analytically using observations and can help the modelling community to improve ESMs. However, I have some major concerns that need to be carefully addressed before publication.

Major comments 1) The study is based on a limited set of observational datasets: only one product per variable. In particular, LAI and precipitation data show large discrepancies and inconsistencies across products (Jiang et al., 2017). Results, based on a so limited set of products, may be largely affected by specific product uncertainties. The analysis should be replicated by using an ensemble of different products for LAI, P and possibly T and RN. Results based on an ensemble of combinations would be much more robust. Comparison of results obtained from different combinations of products would also enable you to assess the validity of your approach and the consistency of your results. Jiang, C. et al. Inconsistencies of interannual variability and trends in long-term satellite leaf area index products. Glob. Change Biol. 23, 4133–4146 (2017). 2) Spatial patterns shown in figures (e.g., figs. 2, 3 and appendices) are very jeopardized and – a part of the radiation control patterns – are not very credible. There is a huge spatial heterogeneity even in regions characterized by the same environmental conditions. I'm wondering, if such spatial variability reflects some problems of stability in the algorithm or noise in the modelled signal. These strange patterns emerge particularly at longer time scales (seasonal, interannual) maybe because the sample size is more limited (?). I really find difficult to believe in such patterns and authors should make an extra-effort to improve or at least understand such spatial variability. In my opinion, such spatial variability could originate from the native time series (possible uncertainties in the signal) and the processing of the signal, as I do not see any patterns that can be easily related to physical conditions. Maybe, the use of ensemble of different observational products (see comment 1) may help to retrieve a more robust signal. 3) The benchmark of ESMs is very useful and interesting. However, the authors should try to identify potential areas of model improvements. This exercise should be aimed to clearly understand what are the strengths and deficiencies of each single model with respect to the data-model comparison performed. A table to synthesize areas of improvements could help to convey the key information to modellers. 4) Remote sensing LAI data in winter season are affected by snow cover conditions. I'm wondering how you have addressed this issue. If you did not account for this, I think your results may

be strongly affected by this bias. 5) The relevance of the multi-temporal scale needs to be clarified, what is the added value of a such analysis compared to previous studies focusing only on monthly scale?

Specific comments Page 1 Paper of interest: https://www.nature.com/articles/s41467-019-10105-3 Line 9: Are you referring to the onset, end of growing season, or what? please, clarify. LAI is not synonym of phenology Line 11: For completeness, can you also briefly refer to the role of temperature? Line 13: please, specify over which temporal scale? Line 15: again, it is not clear here phenology to what is referring to? Line 17: I found a bit too much speculative the interpretation ... could not be just because the direct effect of climate on LAI is larger than the opposite feedback of vegetation on climate in nature? The fact that you are focusing on local scale without remote effect does not imply per se that the feedback of vegetation could be larger than the climate impact on vegetation...

Page 2 Line 8: our biosphere→ the biosphere Line 10: our Earth system → the Earth system Line 12: you give per granted that models do not work well... I would reformulate the sentence... something like : model have shown limitations in capturing ... Line 14: Consider to include the following publication: https://www.earth-syst-sci-data.net/10/1265/2018/ Line 15: Given its relevance in the article, I would contextualize briefly the multi-temporal issue already in this first paragraph. Line 15: Clarify why it is important "the representation of particular inter-variable sensitivities" Line 24: Please, clarify why is important to explore the multi-temporal issue. This would help the reader to follow your rationale and to better appreciate your findings. I would also stress here the challenges that you try to address. From what I understood the multi-temporal scales and the explicit representation of causal relation between vegetation and climate represent the key novelty of your work. I would put more emphasis on these two aspects. Line 25: I would mention that Papagiannopoulou et al. do not address the seasonal and inter-annual scales in order to clearly differentiate your study form the previous work.

Page 3 Line 1: I would suggest integrating your literature review with these relevant articles. https://science.sciencemag.org/content/351/6273/600 https://www.nature.com/articles/s41467-017-02810-8 Line 18: in principle, it may serve also to detect areas where models work well. I would rephrase a bit the sentence in a more general way. Line 31: remote sensing LAI data in winter season are affected by snow cover conditions. I'm wondering how you have addressed this issue. If you did not account for this, I think your results may be strongly affected by this bias.

Page 4 Line 4: Why you do not use the ESA-CCI land cover product (and conversion to pass to PFT)? In principle this would enable to track for changes in PFT over almost the entire period of study. The ESA-CCI product represent the state-of-the-art product aimed to improve the link between remote sensing users and climate modelers ... https://www.esa-landcover-cci.org/ Line 8: Given the large differences amongst different products for some of the variables considered, I would strongly suggest to account for multiple products (https://onlinelibrary.wiley.com/doi/10.1111/gcb.13787). For instance, for LAI, data from GLASS, LTDR, GLOBMAP could also be included in the study. The same for precipitation which show large discrepancies - especially at interannual scale - depending on the dataset used. The use of ensemble of observational products would make your results more robust and substantially improve the work. Line 9: Please, clarify the value of using online model simulations in place of offline simulations. I see a potential limitation as in online ESMs the climate signal may largely determine the response of the land surface and then mask the interplay between vegetation and biophysical processes. Further reading: Blyth, E., Clark, D. B., Ellis, R., Huntingford, C., Los, S., Pryor, M., et al. (2011). A comprehensive set of benchmark tests for a land surface model of simultaneous fluxes of water and carbon at both the global and seasonal scale. Geoscientific Model Development, 4(2), 255–269. https://doi.org/10.5194/gmd-4-255-2011 Winckler, J., Reick, C. H., & Pongratz, J. (2016). Robust identification of local biogeophysical effects of landâĂŘcover change in a global climate model. Journal of Climate, 30(3), 1159–

1176. https://doi.org/10.1175/JCLI-D-16-0067.1 Line 13: Please, clarify the selection, why only these 4 models are used here? Line 15: Are all models run under a consistent modelling setup (e.g., same land cover changes, same climate forcing)? Please, clarify. The consistency in modelling experiment is important to compare the different model results each other. Line 17: Not sure this is correct. You basically used two different periods of analysis for observations and models: 1981-2015 (ca 35 years) for observations; 1956-2005 (50 years) for models. A part of the temporal shift between the two experiments, I would suggest to verify that the different length in the time series do not introduce a systematic bias between observational- and model-based results. Why you decided to start from 1956 for models? To me it would be more logic at least th preserve the same length of observations (35 years). Please, check this and clarify your choices.

Page 5 Line 24: In the presented formulation of GC, the temporal lag m is implicitly assumed the same for all predictors. In practice, I expected that the legacy effects may differ depending on the predictor. Can this be included in the formulation? Please, discuss the implications. Line 27: In principle, data could be aggregated at seasonal or annual level and the GC applied to such values. I presume that the limited sample size hampers the use of GC in a such "aggregated" mode. Please, clarify.

Page 6 Line 2: temporal scale-dependent Line 19: I believe that eq. 7 needs more clarification for readers not familiar with the method. Line 29: Zp in place of Xp?

Page 7 Line 14: I would clearly mention that CSGC does not allow to quantify the sign of causal relation. It is already mentioned in results... but I would also mention here - or somewhere in the method section - because important.

Page 9 Line 3: I would refer to seasonal LAI variability here and in the rest of the manuscript. Phenology implies other metrics that are not accounted for in this work Line 8: please clarify this upper value

Page 10 Line 9: Could irrigation or land enlargement, particularly relevant in some

regions of the globe, partially explain some patterns (e.g., India and China)? Should not be the irrigated lands factored out? https://www.nature.com/articles/s41893-019-0220-7 Line 16: I presume that if you mask irrigated lands this fraction will increase. Can you please comment on this. Line 17: same detrending and deseasonalization approach used for predictors, right? Line 22: compared to what? Fig A1? PA17? Line 23: The snow precipitation should not be already considered in CRU data that used here? Line 24: battery → set? Line 22: also the methods used to quantify the causal relations differ

Page 11 Line 1: forcing on vegetation Line 6: I find the dominance of precipitation very elusive... There are not clear patterns emerging at interannual scale. Probably the P control is just a bit over the other drivers ... but to me what emerges from figure 2e is a major co-dominance of multiple drivers. Please, can you please comment on this. Line 28: To me the comparison performed only on these numbers is misleading because they refer to the relative contribution to the total explained variance. Therefore, ESMs could be in principle represent well the variability of the T control on vegetation in absolute terms, but could overestimate the P control on vegetation in absolute terms. This would lead to an underestimation of the T control in relative terms over the globe ... again not because they fail to represent the T control but because they fail the P or RN controls. The analyses should be complemented with the comparison in absolute terms.

Page 12 Line 1: clarify what do you mean? precipitation and temperature? Line 20: I found the patterns from observations very jeopardized across all temporal scale analysed, and in particular at seasonal and interannual scales. It is really difficult to believe that in the real word you pass from one dominant control to another one while remaining in the same environmental conditions. This heterogeneity should be better explored and understood. The use of ensemble of combination of different observational product of LAI, P, T, RN could help to derive more robust and spatially consistent patterns.

Page 13 Line 13: clearer Line 19: You could move figure 6 earlier and refer to it. Line

25: ESMs capture correctly the LAI effects on net radiation throughout most of the Northern Hemisphere. How do you reconcile with results from Forzieri et al., 2018? Forzieri, G., Duveiller, G., Georgievski, G., Li, W., Robertson, E., Kautz, M., Lawrence, P., Garcia San Martin, L., Anthoni, P., Ciais, P., et al.: Evaluating the Interplay Between Biophysical Processes and Leaf Area Changes in Land Surface Models, Journal of Advances in Modeling Earth Systems, 2018.

Page 14 Line 9: I would say only for EBF, DNF, DBF, MF. For the rest of classes the data-model comparison is fine... Line 19: Only when averaged at biome level. Maps in figure 3 differ substantially. Maybe this concept would merit to be expanded a bit. Results from ESMs and satellite tend to converge when averaged at biome level... can you please comment on this? Line 30: Can you please reconcile or at least interpret these divergences?

Figure 2, 3 and appendices: I suggest a different colour palette because colours tend to saturate quickly and differences cannot be appreciated well. Figure 4: change order of variables consistently with figure legend. Same f

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