

Review “Global biosphere–climate interaction: a multi-scale appraisal of observations and models by J. Claessen”

G. Forzieri

I would like to thank the authors for their extra-efforts in order to account my major concerns. I have sincerely appreciated their work. I have still a series of comments. The most important ones relate to the need of additional clarifications in the text, issues related to the significance and some possible numerical inconsistency amongst figures.

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Line 10: I find this a bit redundant ... would not be better to refer explicitly to what geographic regions they refer?

Line 12: I would explicit four models, as now it seems that you tested the whole CMIP ensemble

Line 15: I would try to be consistent here using "vegetation". You are not focusing only on forests...

Line 18: I think this is an over-interpretation. I would remove the downwind influence.

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Line 28: I would suggest to split this long sentence.

Page 3

Line 26: explicit four models

Line 27: I would suggest to restructure a bit this sentence.

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Table 1: I would suggest to distinguish this info in 4 fields, as: spatial resolution, temporal resolution, temporal coverage

Line 8: I would explicit in the text the temporal coverage so that the info provided in the following section can be better understood

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Line 8: CSGC, ... add comma

Line 16: typo: remove "is"

Line 29: Would it make sense to refer to figure 1b?

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Line 34: Refer to fig.1a?

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Line 3: I would add in a dedicated section or somewhere in the methods a description on the multi-product (or multi-model) ensembles. Describing for instance how you utilized the different observational products (modes). For observations, I suppose that you run the CSGC for each combination of LAI, T, P, Radiation and then compute the average over the ensemble of results. the model ensemble has 4 members. What about the size of the observational ensemble? Please, clarify and provide this info in the text.

Line 15: I have still some concerns about this issue. To my understanding, some LAI products have a lot of missing data during winter periods. It would be important that authors clarify how they solved this issue. Times with missing data are simply excluded the time series? Do this affect the time-frequency transformation? Did you keep pixels with a minimum number of observations during winter seasons? This details should be clarified maybe in the method section.

Line 22: not need of parenthesis, just put a comma.

Line 24: Looking at figure 2a, I would say that radiation is the primary control on a larger portion of vegetated land... can you please check these numbers? I suppose, you have accounted for the dependence of the spatial extent of grid cells on the latitude.

Line 30: I would restructure a bit this sentence...this is clearly in response to my previous comment, but as now you are assuming the reader is complaining about the effect of irrigation...

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Line 5: I agree with this interpretation. It would be interesting to check the climate impact on vegetation for different irrigation patterns. For instance, plotting separately P, Ta, Rn controls over a gradient of percentage of irrigated area. I would expect that an increase irrigation, for some areas, may tend to amplify the control of Rn and Ta (as also argued by authors). The analysis should be relatively easy to perform and could add interesting material to your interpretation.

A possible dataset to explore this:

<http://www.fao.org/nr/water/aquastat/irrigationmap/index.stm>

It is more complex to extend the same rationale to explore the effect of deforestation. One option, not sure about the results, could be to compute the long-term linear trends in cover fraction of non-forest classes (including natural grasses, crops, bare soil), for instance from annual land cover maps. The trend computed at the 0.5 degree spatial resolution should reflect the long-term transition from forest to non-forest state. The idea would be to dependence of the climate controls across a gradient of deforestation rate (the retrieved linear trend).

Possible datasets to explore this:

<https://www.esa-landcover-cci.org/>

https://earthenginepartners.appspot.com/science-2013-global-forest/download_v1.2.html
https://www.nature.com/articles/s41586-018-0411-9?_sg=RshFR75ayixme4g7CasqNzvfZF2Mw0bNFQFtRQcDkFh7_rL-j96NyYt0O3442MI74umt9nXWfDbnR_A.bLXfW_RzdtPGLPmYQ16GWH9fCT3jhEyrMsymQU_fr1sGhDI4iXJiEbqMaOxTXxm-3NyW9qnZ-7WwWgJfw9BkQ

Analogously, the same could be computed for the vegetation impact on climate (for observation and models). In theory could be described in a dedicated section to better disentangle the human effect. Please, consider this as a suggestion and develop only you think that can add important material. As potential reader of your work, I believe that this analysis would add value to your analysis.

Line 30: energy-to-water?

Line 32: why? Please, clarify

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Line 14: Can you please comment the low significance of these results? Seems that the inter-product spread tend to substantially alter the significance level (compare with your results in the previous submission). Are different combination of observational products leading to completely different results? To me it is a bit worrying such low level of agreement amongst the products. It may also be a warning in the use of such product (with results so different each other) to benchmark ESMs. I would really appreciate some more details on this.

Line 21: There is also a substantial overestimation of the LAI control on Ta and an underestimation of LAI control on PP in models compared to observations at monthly scale (see for instance sub-tropics). Am I right? Can you please comment on this?

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Line 14: due to compensatory effects?

Line 22: Please, clarify this statement. Without an appropriate context this appears a pure speculation. Based on the same rationale, one could say that also shadow effects, or many other micro-biometereological effects, are not accounted for...

I honestly think that you do not have enough data to support this statement.

Line 33: global in place of continental?

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Line 8: This appears in figure 3 but not in figure 6. To me there are some inconsistencies between latitudinal profiles in figure 3 and the global averages shown in figure 6. Just as example, compare Fig. 6b with Fig. 3a. The average absolute vegetation feedback on radiation is about 43% in Fig. 6b. If you look at the latitudinal profile in Fig. 3a, values of vegetation feedback on radiation are always below 10%. Please, check these numbers.

Code availability: please, be sure to have uploaded your codes, or to have properly labeled them. I had a look, and I could not find the codes related to this publication.

Figure 1

if in the text you refer to panel b earlier than panel a, I would suggest to change the order of the panels to be consistent with the text

Figure 2

- Not sure if this can be easily visualized, but it would be interesting to see the inter-product and inter-model spread. It would be interesting to compare the uncertainty across observational products and that one emerging from models... If the information is relevant you could consider to move the panels with latitudinal profiles in a separate figure in order to better visualize results.
- I would suggest to use a different color or style for the reticular grid of the maps. Now, it tends to be confused with black dots of significance. Same for the other figures
- I would suggest to put at the corners of the triangle the number "100%" in addition to the variable label. For instance "Ta 100%". the same for the other figures.

Figure 6

I would say just global averages... There is no need to introduce such new regional aggregation term. You do not distinguish values amongst continents.