Authors' Response to the Editor (BGD bg-2021-37)

June 29, 2021

Dear Authors, both reviewers have commented on your revision, they both raise some outstanding issues. R1 suggests a few technical corrections. R2 still thinks the concluding text could better convey uncertainty across LAI products given that the statements are largely dependent on one product. I think that is fair and is a small amendment. R2 also asks about internal variability in the ESM. Perhaps a small comment could be added about how this might influence attribution? There are only 6 ensembles per experiment? That doesn't strike me as a particularly large number, so simply acknowledging this in a sentence again seems fair. In addition, on re-reading your manuscript I have a few points myself, see below (these are all small). I am recommending this be published subject to minor revisions that I will review. Best wishes, Martin De Kauwe

Thanks for the thoughtful comments and for your quick response. We addressed the technical corrections by **Referee 2 (i.e. Prof. Dr. Christian Frankenberg)**. We included another statement about the uncertainty in the LAI product as suggested by the **anonymous Referee 1** in the revised manuscript. We read the comment on internal variability by Referee 1 such that he/she argues that there is no internal variability in land-surface models, because all the variability originates from the observed climate forcing. We address this point again in the response to the comments of Referee 1. Regarding the internal variability in the ESMs, please see our response to your specific comment 1.3. We address each of your specific comments below. For detailed changes to the manuscript, please refer to the manuscript version with highlighted changes with respect to the last version.

1 Specific Comments

1.1 *Line 37: "stomatal density" - please add a reference (Woodward?). Is there any evidence this has happened over the satellite record? I'm not personally aware of this, please clarify this point in the text.*

Our original manuscript did not discuss the aspect about changes in stomatal density. Referee 1 pointed us at this and we agreed that this aspect should be included in the manuscript. In the revised manuscript, we now include the reference by Lammertsma et al. (2011) who reported a strong decrease in stomatal density for various species, also throughout the satellite record (Fig 4A).

1.2 *Line 53: re-enters*

Thanks, we corrected the typo.

1.3 *MPI-ESM: I wasn't clear if this model was running its own climate (presumably?) or you were prescribing the forcing? Could this be clarified in the methods, sorry if I missed this? I also feel like this section of the methods would be helped by some understanding of how "well" the MPI model simulates historical LAI magnitudes and patterns. The reason you presumably use an ensemble of TRENDY models is that each model's simulation differs from one another, but here we don't have an ensemble of ESMs, so the radiative vs physiological effects are only derived from a single model. I suspect this is actually quite important. At the very least it warrants a few sentences and I would prefer S2 became a main figure, perhaps in the methods, but ideally, a map of discrepancies would be more valuable.*

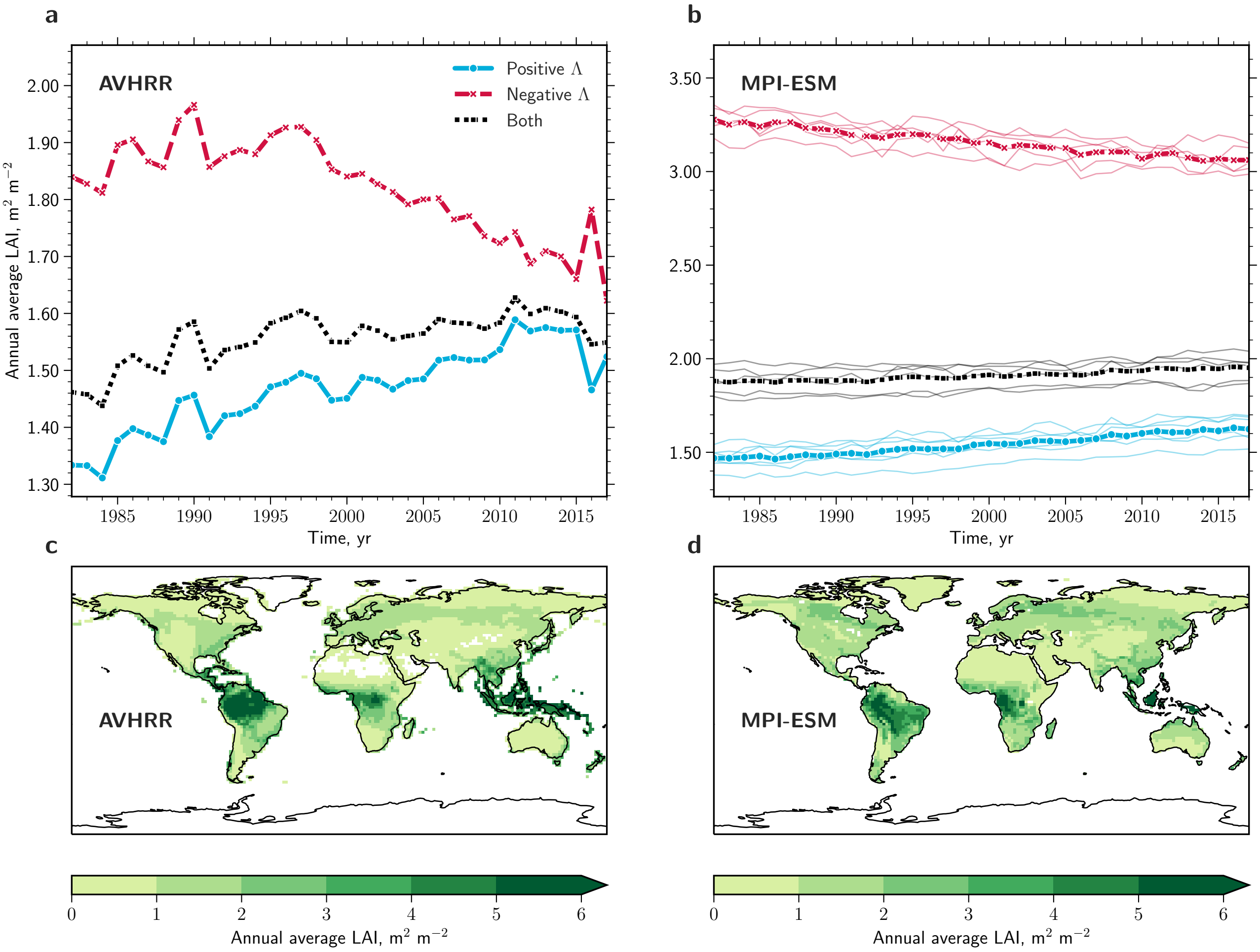

Figure R2- 1: **LAI observations versus MPI-ESM ensemble. a** Time series of area-weighted annual average LAI for regions exhibiting positive (blue line) and negative trends (red line) masked for natural vegetation (denoted Λ). Black lines represent the overall signal of all pixels. **b** as **a**, but for the MPI-ESM. The individual realizations are represented as thin lines and the ensemble means are shown in bold lines. **c** Global patterns of annual average LAI over the time period 1982–2017. **d** as in **c**, but for the MPI-ESM.

The MPI-ESM is a fully coupled Earth system model that links, among other submodels, an atmospheric, oceanic and land model. Thus, the MPI-ESM simulates its own climate, where changes are driven by historical forcings such as deforestation patterns, aerosol loadings, and greenhouse gas emissions. We only have resources, access and the expertise to run the factorial simulations with the MPI-ESM, but of course, a multi-model ensemble of ESMs would be better and would provide an estimate of inter-model variations between ESMs. Yes, this shortcoming was the main motivation for analyzing the TRENDY ensemble so that we can address the aspect of inter-model variations (most ESMs use one of the land-surface model analyzed here as stand-alone TRENDY model). We include S2 as a main figure (Figure 1 now) in the methods section together with global maps of LAI magnitudes and patterns based on your suggestion (Figure R-1).

1.4 Line 331: I'm struck by this narrative "more and more browning clusters are beginning to emerge". Wouldn't one expect browning clusters anyway (e.g. due to droughts, fire, insect attacks, etc.)? Even where the climate not changing, there should be a baseline background browning due to climate IAV, I wonder how robustly we can assert the "emergence" from a baseline? This point connects more widely to the "intensification of leaf area loss in recent years" and the "slowdown of the greening trend". Do we have sufficient evidence to form this conclusion, or could the text more strongly reflect recent variability and sampling?

Browning and greening are defined as long-term changes. So, if frequency and magnitude of droughts, fires and insect outbreaks etc. are constant, which is the case for the system without external forcing, these events would not emerge as browning trends. Thus, no, one would not expect that browning clusters emerge anyway on the long-term. We characterize the occurrence of browning primarily by the observation that, on the one hand, the number of pixels and thus the area of declining leaf area increases (e.g. see inset in Figure 2 in revised manuscript) and, on the other hand, in some biomes the sign reverses from positive to negative trends (e.g., see Figure 4 in revised manuscript). Considering the overall uncertainty in the AVHRR-based data products, there is sufficient evidence, supported by most of the long-term products shown in Figure 5 in revised manuscript, that the Earth's greening trend is slowing down. We agree though that another statement about possible implications and general uncertainty should be included. Please see the revisions in the manuscript version with highlighted changes.

1.5 *Line 429: can we also include the TRENDY model range (min, max), not just the ensemble mean.*

The overall uncertainty is reflected by the yellow bar in the panel **c** and **e** in the "attribution figures", e.g. Figure 4 following Hannart and Naveau (2018), as we describe in the Methods section:

The uncertainty "[...] Σ denotes the overall uncertainty and is estimated based on all simulations, comprising factual, counterfactual, and centuries-long unforced (pre-industrial) model runs (for details see Hannart and Naveau, 2018)."

We decided to adapt the approach by Hannart and Naveau (2018) as it provides an integrated estimate for the overall uncertainty including the range of the TRENDY models and does not overload the already crowded figures. Further, we provide the TRENDY-specific and MPI-ESM-specific uncertainty (shaded area denotes the 95% confidence interval) in panel **b** of Figure 4 and analogous SI Figures.

References

Hannart, A. and Naveau, P. (2018). Probabilities of Causation of Climate Changes. *Journal of Climate*, 31(14):5507–5524.

Lammertsma, E. I., de Boer, H. J., Dekker, S. C., Dilcher, D. L., Lotter, A. F., and Wagner-Cremer, F. (2011). Global CO2 rise leads to reduced maximum stomatal conductance in Florida vegetation. *Proceedings of the National Academy of Sciences*, 108(10):4035–4040.

Authors' Response to Referee 1 (BGD bg-2021-37)

June 29, 2021

1 General Comments

1.1 *In view of these two points (which are - in my view - still not satisfyingly addressed), the (still) strong statements in the abstract, l. 11 ("Our results do not support provide only little support to previously published accounts of dominant global-scale effects of CO2 fertilization.") and in the conclusion section, l. 652 ("This finding questions the study by *Zhu et al.* [2016] that identified CO2 fertilization as the most dominant globally prevailing driver of the Earth's greening trend.") have unclear support. Also reviewer 3 stated "... I would say that the CO2 fertilization effect appears to be dominant at the global scale".*

Thanks for the comment. Yes, as also Referee 2 addressed this point, we rephrased the sentence again, which now reads:

"*The probabilistic attribution method clearly identifies the CO_2 fertilization effect as the dominant driver in only two biomes, the temperate forests and cool grasslands, challenging the view of a dominant global-scale effect.*"

1.2 *Authors have made some revisions to their manuscript. In particular, they have added Figs. 4 and 5. Fig. 4 is helpful for placing the LAI trends in the GIMMS-LAI product in context. In this sense, I accept the reply by the authors multiple estimates of LAI trends have been used. However, (and this is what my initial comment 1.1 about an unjustified reliance on a single product was referring to), the main conclusions and results presented in Fig. 1, 2, and 3 rely solely on GIMMS-LAI product. Authors have not revised this strong reliance on a single estimate and have not placed their results in a larger context that would account for the uncertainty in recent LAI trends.*

The main conclusion, which is also reflected in the title is "Slow-down of the greening trend in natural vegetation with further rise in atmospheric $\mathbf{CO_2}$". Four out of five long-term products support this statement. Another central conclusion is that the effect of CO_2 fertilization can only be clearly identified as the main driver in the biomes of the temperate forest and cool grasslands, using the approach of causal counterfactual theory. This attribution analysis is mostly based on the factorial runs of the models and depends less on the estimates of the GIMMS LAI dataset. To address the caveats that still exist, we have implemented another statement that once again addresses the uncertainty in the GIMMS LAI product based on AVHRR:

"*Overall, the analyses of the different remote sensing datasets support to a large extent the findings drawn from the GIMMS LAI3g dataset. For these reasons, and for reasons described earlier in the introduction, we focus on the GIMMS LAI3g dataset in these analyses, but we note that the single-product-centric view may imply some additional uncertainties besides the general uncertainty associated with AVHRR-based datasets.*"

1.3 *Regarding my initial comment 1.3 about the method of driver attribution. I stated that "[...] for vegetation dynamics, [...] the (simulated) internal unforced variability is typically zero [...]". I meant that, if these models are forced with a constant environment, their simulated fluxes and pools will typically be constant (dX/dt = 0). This is due to the fact that, typically, no random process is operating in these models, and that the simulated* *system does not typically behave in a deterministically chaotic manner. This is in contrast to general circulation models, where slight changes in initial conditions can lead to substantial changes in simulated quantities (internal variability, particularly expressed on short time scale and small spatial scales). Absence of internal variability facilitates driver attribution massively. A quantitative attribution to a driver is defined simply by the difference to a counterfactual simulation where the driver is not operating (no need to account for unforced internal variability - all variability is forced). Authors rejected this argument and did not make any changes to their applied method.*

We thank the referee for revisiting this point, but we reiterate our response. It is true that variability in the atmospheric forcing translates into variability in land surface models, and constitutes the dominant contributor in the overall variability. However, there are also several ways, that coupled processes in land surface models alone can lead to internal variability. There a various feedback loops connecting, for example, processes controlling dynamic vegetation (competition among plant types), biomass accumulation, fire events, nitrogen limitation, soil moisture effects, which can result into temporal and spatial variability. Let's take the fire-vegetation feedback as an example: Lasslop et al. (2016) showed that the fire-vegetation feedback can create bi-stability in vegetation cover (trees versus grasses) in offline land surface models. Thus, even in an offline land surface model, changes in initial conditions can result in different steady states for fluxes and pools. Further, and this is very important, **the term variability here refers to a more broader concept of variability**, including inter-model variability. To estimate uncertainty / variability in this causal framework we again follow and adapt the approach by Hannart and Naveau (2018) who argue that the overall uncertainty estimates comprises various components, such as climate variability (the type of variability the referee is referring to), inter-model variability, and variability in observations (Please read Section "2.7 Causal Counterfactual Theory": "*[...] the overall uncertainty [...] is estimated based on all simulations, comprising factual, counterfactual, and centuries-long unforced (pre-industrial) model runs*"). The intent behind robustly estimating an overall uncertainty is to evaluate the probability of occurrence and magnitude of greening/browning trends over $\sim$ 40-year periods across models and between forced versus unforced systems. We understand that the term "variability" can be misleading, thus we replaced it with the term "overall uncertainty" in the revised manuscript.

References

Hannart, A. and Naveau, P. (2018). Probabilities of Causation of Climate Changes. *Journal of Climate*, 31(14):5507–5524.

Lasslop, G., Brovkin, V., Reick, C. H., Bathiany, S., and Kloster, S. (2016). Multiple stable states of tree cover in a global land surface model due to a fire-vegetation feedback. *Geophysical Research Letters*, page 2016GL069365.

Authors' Response to Referee 2 (BGD bg-2021-37)

June 29, 2021

Thanks for the thorough revisions, just a few nit-picky remaining points, which I consider "technical corrections" (in the hope that they are smaller than "minor" but maybe I confuse these.).

We sincerely thank Prof. Dr. Christian Frankenberg for reviewing our revised manuscript and his suggestions for minor corrections. We address each comment below.

1 Specific Comments

2.1 *Abstract: you went from "do not support" to "provide only little support", which might still be confusing to read in the abstract (which is unfortunately sometimes all that readers look at). I would suggest a sentence more similar to the one you use later, which makes it clear that CO2 is a strong factor (I would even say the strongest) but that there are regions where it is not dominant.*

We thank the referee for this comment. As the Referee 1 also addressed this point, we revised this section again to be more specific. The sentence reads now:

"*The probabilistic attribution method clearly identifies the CO_2 fertilization effect as the dominant driver in only two biomes, the temperate forests and cool grasslands, challenging the view of a dominant global-scale effect.*"

2.2 *Line 91: "In many others" Can you quantify this here? In what fraction of the vegetated land surface is it not dominant?*

We rephrase this sentence and are more specific about where the CO_2 fertilization effect can be identified as the dominant driver.

2.3 *Line 123: Awkward sentence, please check structure (esp. the for more details, see Chen at al part)*

Thanks, the restructure sentence reads:

"*The two LAI datasets are aggregated into a 16-day composite by taking the mean of all valid LAI values after an additional data quality assessment is performed (for more details, please see Chen et al., 2019).*"

2.4 *Line 488: "While these results are consistent with ours": The key result from Wang et al is not necessarily the decline in the effect, which is also apparent in Trendy and virtually all models, but the magnitude of the decline. Would be good to put this into context (e.g. what does your model say?)*

Yes, we agree that the main result of the study by Wang et al. (2020) is the magnitude of the effect rather than its existence. We rephrase the section accordingly in the revised manuscript. Wang et al. (2020) focused on the β factor which is defined as $\beta = \frac{\partial GPP}{\partial C_a}$, where GPP is gross primary production (GPP) in response to a 100-ppm increase in atmospheric CO_2 concentration (C_a). These results cannot be compared with the results of this study because the variables analyzed and the study design are conceptually different. Also, we analyzed more or less the same set of models (Wang et al. (2020) used version TRENDY v6; this study uses TRENDY v7), including JSBACH, the land component of the MPI-ESM. Hence, the model-based estimates for the magnitude of the decline in β should be very similar.

References

Chen, C., Park, T., Wang, X., Piao, S., Xu, B., Chaturvedi, R. K., Fuchs, R., Brovkin, V., Ciais, P., Fensholt, R., Tømmervik, H., Bala, G., Zhu, Z., Nemani, R. R., and Myneni, R. B. (2019). China and India lead in greening of the world through land-use management. *Nature Sustainability*, 2(2):122.

Wang, S., Zhang, Y., Ju, W., Chen, J. M., Ciais, P., Cescatti, A., Sardans, J., Janssens, I. A., Wu, M., Berry, J. A., Campbell, E., Fernández-Martínez, M., Alkama, R., Sitch, S., Friedlingstein, P., Smith, W. K., Yuan, W., He, W., Lombardozzi, D., Kautz, M., Zhu, D., Lienert, S., Kato, E., Poulter, B., Sanders, T. G. M., Krüger, I., Wang, R., Zeng, N., Tian, H., Vuichard, N., Jain, A. K., Wiltshire, A., Haverd, V., Goll, D. S., and Peñuelas, J. (2020). Recent global decline of CO2 fertilization effects on vegetation photosynthesis. *Science*, 370(6522):1295–1300.