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

May 10, 2021

The manuscript by Winkler et al investigates drivers of global LAI trends using a mix of long-term observations from AVHRR combined with Earth System Model sensitivity runs to provide causal attribution. The manuscript is in general well written and the results interesting and of general scientific interest. Please find some comments/questions below.

We sincerely thank Prof. Dr. Christian Frankenberg for his thorough review of our manuscript and his thoughtful comments. We address each comment below.

1 General Comments

1.1 *Title: I am not convinced the title conveys the gist of the paper, in fact I find it somewhat misleading. It reads as if the slow down of greening is driven by a further rise in CO2. The word "instead" instead of "with" would have made more sense but then again, the authors would have to make the topic of the title the key message of their paper, which it isn't (and it is hard to attribute to a weakening CO2 fertilization effect anyhow). For this, the author would have to use their counter-factual theory on the change in LAI changes between the beginning and end of the time period.*

We understand the confusion that may arise from the current title. We are aware that the scientific community in ecology sometimes uses "CO_2 effect" and "CO_2 fertilization" interchangeably, rendering the title counter-intuitive. But from a "Earth system" perspective, rising CO_2 as a forcing agent interacts with various processes in the system, which in turn have an effect on ecosystems. In this paper we investigate the impact of the physiological (PE) versus the radiative effect (RE) of rising CO_2 on leaf area. The title is meant to reflect that the Earth largely greened in the 1980s and 1990s as rising CO_2 had mainly LAI-increasing effects, e.g., by warming high northern latitudes (consequence of RE) and overall more carbon allocation through CO_2 fertilization (consequence of PE). However, as CO_2 continues to rise, the system appears to be entering or has entered a regime in which LAI-decreasing effects are amplified, i.e., climatic changes associated with rising CO_2 become more pronounced and have stronger effects on various ecosystems/biomes (consequence of RE), and possibly plant sensitivity to CO_2 fertilization decreases (as hypothesized in e.g., Wang et al. (2020), as also mentioned by the viewer in comment 1.2). Therefore, we find the title "Slow-down of the greening trend in natural vegetation with further rise in atmospheric CO_2" reflects the key content of the paper.

1.2 *That said, it would be necessary to also discuss the results of Wang et al. (2020) (Recent global decline of CO2 fertilization effects on vegetation photosynthesis) in the current manuscript as it is related to trends in CO2 fertilization as well (especially a reported decline of it, which differs strongly from Trendy).*

We thank the reviewer for pointing us to this recent paper on the global decline of CO_2 fertilization by Wang et al. (2020). We already integrated and discuss this study in the revised version of the manuscript. The authors used linear and non-linear regression methods and observational data ranging from remote sensing to *in-situ* atmospheric CO_2, and obtain the key result that global CO_2 fertilization has decreased. While this result seems to be inline with our findings, we do not argue that the sensitivity of the terrestrial biosphere to the CO_2 fertilization effect has declined, but that the effects of climatic changes rooted in the radiative effect of CO_2 (e.g. precipitation changes or increase in VPD) have strengthened, which probably counteracted the physiological effects of CO_2.

1.3 *One main strong statement of the paper is that it challenges finding by Zhu et al. (2016) (with some shared co-authors!). It sounds like a strong statement early on but if I look at Figure 3, I would say that the CO2 fertilization effect appears to be dominant at the global scale (despite some regional variations). It expands and adds nuance to Zhu et al, but challenges is too strong a word in my mind. There is enough material in this paper to warrant publication and no need to over-emphasize differences wrt to a previous publication.*

We thank the reviewer for this very good comment. We will rephrase this passage so as not to over-emphasize the differences with respect to Zhu et al. (2016)'s study in the revised manuscript. One point we make in the paper is that at the global scale, even the causal attribution technique like the optimal fingerprinting method used by Zhu et al. (2016) points to CO_2 fertilization as the main driver. This aspect motivated the biome-level analysis that led to the point that in many biomes, not all, the CO_2 fertilization effect cannot be identified as the main driver. But there are clear imprints of climatic changes that are obscured in a global analysis.

1.4 *Browning Trend in 2000-2017: When I look at Figure 3B, it appears a lot of the apparent browning trend in the later time-period is driven by a sudden decline in the relative change in years 2015-2017. What happens if you omit these years from the investigated time-period? What might cause such a sudden decline that might be related to the effects of CO2 fertilization or Radiative Effects? If this is related to detector issues or years with strong internal variability, I would remove these years (as long term drivers appear unlikely to suddenly appear). In fact, models and obs seem very consistent with each other between 2000-2014. As far as I can see, most discrepancies might be due to years 2015-2017 but I might be wrong. A critical discussion would be required here. Surprisingly, I couldn't find these strong effects of the last 3 years in the SOM plots, was it specific to some areas only? Can it be checked against MODIS data as well, which could be more reliable now? In fact, the first few years in Figure 3B are also VERY small, so you are fitting a linear trend through a time-period in which both ends are highly unusual. This can heavily bias derived trends, please evaluate and discuss the impact of chosen time-periods for trend analysis critically. https://doi.org/10.31223/X5K89V outlines some concerns I have with respect to AVHRR and the application to look at small changes (beyond pure trends). Please answer all questions in this paragraph.*

We thank the reviewer for this detailed and important comment. To investigate the sensitivity of our results towards the rapid decline in the years 2015–2017, we recalculated the relative trends in global LAI for the time windows for 2000-{2013, 2014, 2015, 2016, 2017} and for comparison for 1982-{1995, 1996, 1997, 1998, 1999}. Figure R2-1a compares the trend sensitivity analysis between the first and the last two decades of the AVHRR GIMMS LAI3g record. Where the relative trend in LAI in the 1980s/1990s is around 5% decade^{-1}, it is between 0-1% decade^{-1} in the 2000s/2010s. The different end years have an effect on the trend calculation, especially in the last two decades, but the differences are rather minor when compared to estimates of first two decades. Accordingly, the slow-down of the trend is also apparent when the sharp decline from 2015–2017 is excluded.

Figure 1b depicts how the global distribution of relative trend changes with varying end-years in the 2000s/2010s (for better readability only three time periods are displayed: 2000-{2013, 2015, 2017}; only time-series which pass the Mann-Kendall trend significance test ($p < 0.1$) are included). With respect to the periods 2000-2013/2015, there is a clear decrease in the pixels count of significant positive trends at the high range (between 10-20% decade^{-1}), a slight increase in the low range of positive trends, and an overall increase in negative trends for the period 2000-2017. Studying the results of the biome-level analysis (Fig. S3–Fig. S16), we find that the apparent rapid shift in the years 2015–2017 is not a global phenomenon, but rather "driven" by the tropical forests. It is currently being investigated whether this rapid decline in recent years could also be a detector problem. We also include here the current Fig. S3, which compares five different remote sensing datasets and how they depict the development of the natural vegetation over the last four decades. NCEI-FAPAR, LTDR-NDVI, and GLASS-LAI do not show this rapid decline in the years 2015–2017 as found in GIMMS-LAI, yet they agree on the slow-down of vegetation greening for the 2000s/2010s. All in all, our results and the overall conclusion of the slow-down of greening are not affected by the singular years from 2015–2017. As suggested by the reviewer, we will discuss the impact of the chosen time-periods for trend analysis based on the material presented here in the revised version of the manuscript.

In the Fig. S3 (Figure R2-2), we now also include MODIS-LAI for natural vegetation only, as suggested by the reviewer. MODIS-LAI depicts a stable moderate greening trend for the time-span of 2000-2019. Since the MODIS record cannot provide any information on the state of the vegetation in the 1980s and 1990s, we cannot assess whether MODIS would also depict a slow-down of the overall greening trend over this time-period. Also, please note that the comparability of relative trends in the long-term remote sensing products (baseline period 1982-1984) and MODIS-LAI (baseline period 2000-2002) is limited. Please see also our response to Comment 1.1 by Reviewer 1 and the discussion on MODIS and AVHRR discrepancies in the manuscript (LL362–369). Since this important issue was raised by both reviewers, we will move Fig. S3 in the main manuscript document and extend the discussion on the various datasets.

The reviewer raised the point that the values of the first few years in Figure 3B are very small. These values are very small be definition, since we are displaying relative changes in % with respect to the baseline period 1982-1984, so the initial values of the time-series are around zero %.

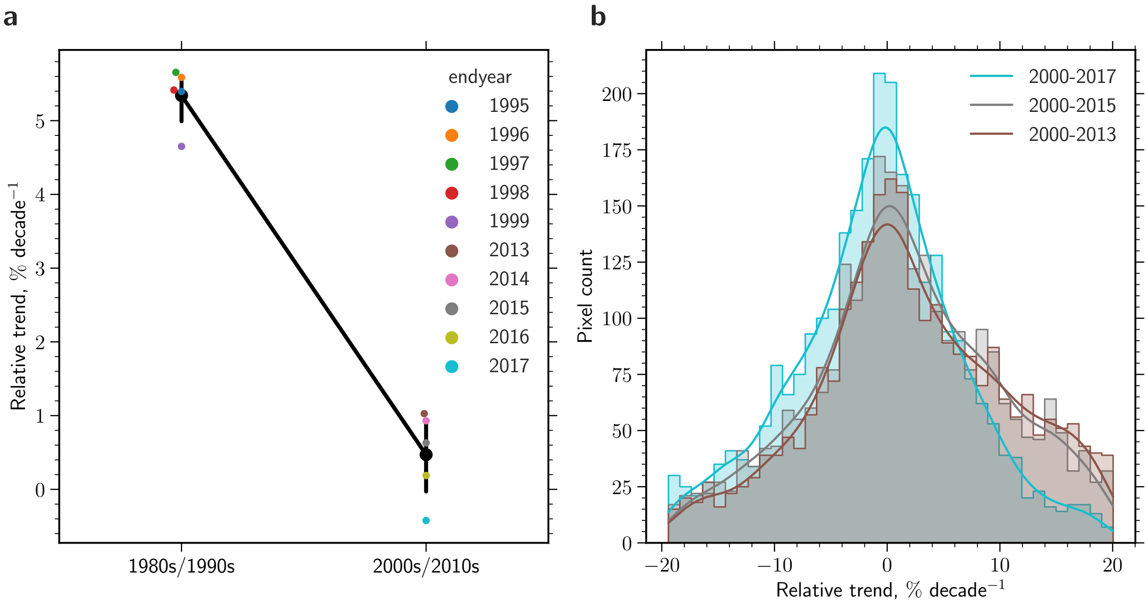

Figure R2- 1: **Estimating the sensitivity in the trend calculation with respect to the selection of the window size. a** Relative trends (in % decade^{-1}) in LAI relative to the average state from 1982–1984, calculated for different end-years, comparing the first two decades (1982–1999) with the last two decades (2000–2017) of the AVHRR GIMMS LAI3g record. The colored dots represent the trend estimates for different end years of the time series. Black dots represents the average value of the five estimates for each period, i.e. 1980s/1990s versus 2000s/2010s, including whiskers which denote their standard deviation. **b** Histograms of relative trends over the last two decades (2000s/2010s) in the AVHRR GIMMS LAI3g record including probability density functions (kernel density estimation) comparing estimates based on varying end-years. Only trends which pass the Mann-Kendall trend significance test ($p < 0.1$) are included.

1.5 *A more general question regarding vegetation dynamics and CO2 fertilization, as you mention "as thoroughly equilibrated global carbon cycle" on line 192: What are the time-scales in ESM for CO2 fertilization? At the leaf scale, the gain in GPP is immediate but if you consider LAI, CO2 fertilization might cause a new state, which won't be achieved within a year, especially if species compositions will be affected. I would be curious what time-scales the models predict. E.g. if you changed CO2 suddenly but keep it at a higher level, how long would it take to run the carbon cycle into a new steady-state? I am mostly asking because the CC was certainly not in equilibrium in 1980 as CO2 increase and human land impacts are constantly shifting the needle. How much of the greening effects would have occurred (persisted for a while) even if we had suddenly frozen the CO2 levels at the 1983 mixing ratio and how would these "legacy" effects affect your overall conclusion? This is not a strong criticism but rather scientific curiosity.*

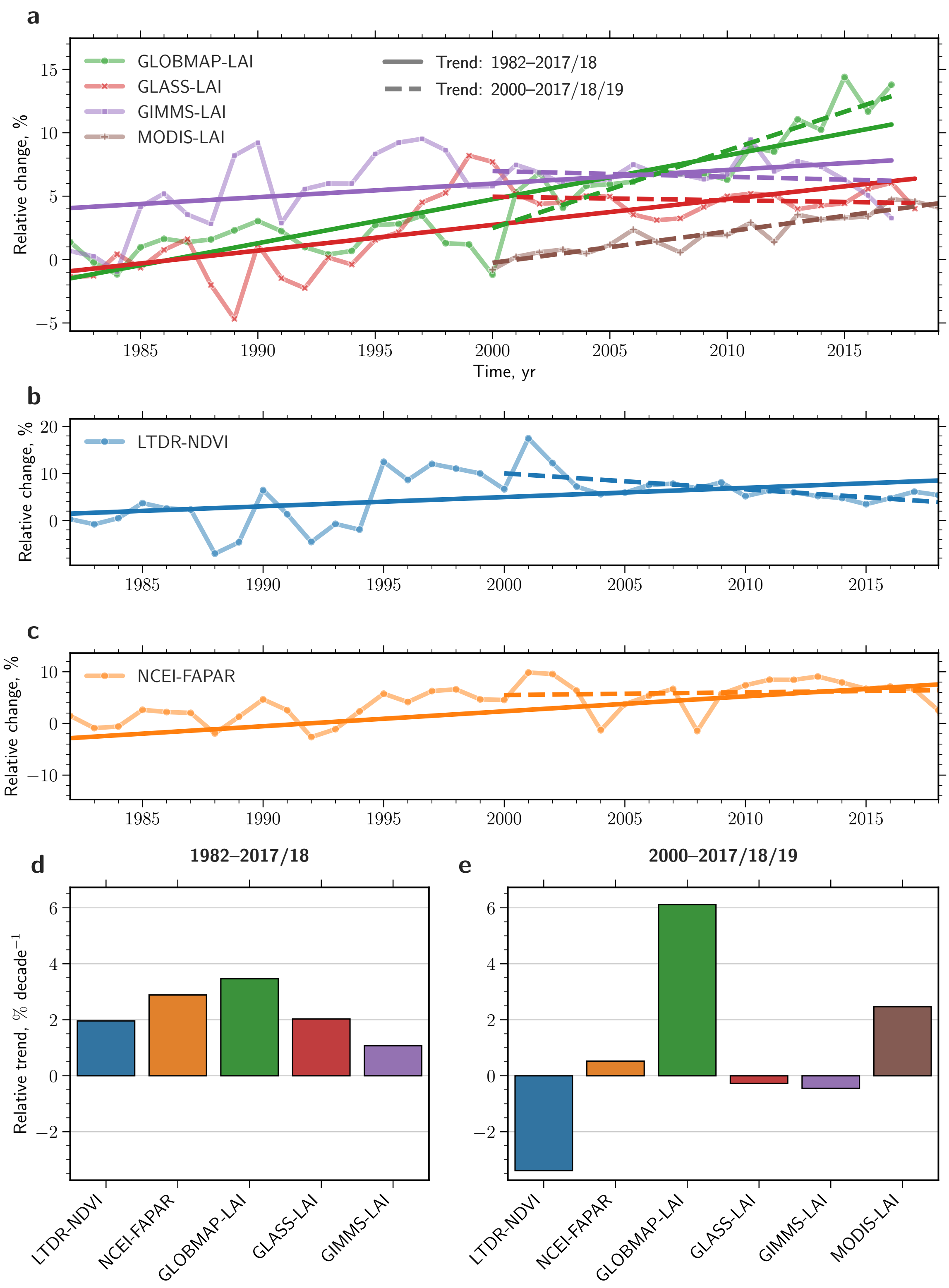

Figure R2- 2: **Five different remote sensing datasets displaying the development of the natural vegetation over the last four decades.** **a** Time series of changes in LAI relative to the average state from 1982–1984 as depicted in three different datasets (green: GLOBMAP-LAI, red: GLASS-LAI, purple: GIMMS-LAI and brown: MODIS-LAI; see Materials and Methods section of the main paper for further details). The solid straight line represents the best linear fit for the entire period (1982–2017/2018), the dashed line represents the best linear fit for the second half of the period (2000–2017/18/19). **b** as in **a** but for the dataset LTDR-NDVI (blue; see Materials and Methods section of the main paper for further details). **c** as in **a** but for the dataset NCEI-FAPAR (orange; see Materials and Methods section of the main paper for further details). **d** Bar chart comparing relative trends (in % decade^{-1}) in LAI, NDVI and FAPAR from different datasets for the entire period (1982–2017/2018) obtained from the gradients shown in **a**-**c**, respectively. **e** as in **d** but for the second half of the period (2000–2017/18/19).

We thank the reviewer for raising this interesting discussion point. Bringing an Earth system model (ESM) into general equilibrium can take many thousands of years, especially when deep ocean circulation and slow biogeochemical cycles such as that of nitrogen are included in the feedback network of an ESM (both of which are the case with the MPI-ESM used in this study). Even after the ocean circulation has reached a steady state and all the matter pools have built up, various variables may still exhibit drifts, especially on a regional scale. Thus, it requires expertise and patience to bring an ESM into general equilibrium in all of its subsystems - this is what we meant by a ”thoroughly equilibrated” ESM / carbon cycle. For our study, we took the pre-industrial equilibrium of the MPI-ESM prepared by the MPI-M development team for CMIP6.

So, regarding the question ... *if you changed CO2 suddenly but keep it at a higher level, how long would it take to run the carbon cycle into a new steady-state?*: This strongly depends on the magnitude of change, i.e. CO_2 forcing. Many processes respond fairly immediately, such as GPP or radiative forcing, but others respond more slowly, such as ocean heat uptake, dynamical vegetation changes, or the global cycling of carbon. With all the feedbacks between ocean, atmosphere, land, and biosphere, a fixed increase in atmospheric CO_2 of, say, the order of 100 ppm would push the system so strongly that it would again take on the order of a thousand years to reach a new equilibrium for the carbon cycle.

Yes, absolutely, there are legacy effects in the system. Let’s say we froze atmospheric CO_2 in the early 1980s, as for many other variables, greening would also continue after CO_2 stopped increasing, e.g. due to slower processes regarding dynamical vegetation. This is a very interesting research question in itself! As described above, our simulations, like the TRENDY simulations, are initialized from a pre-industrial equilibrium (for TRENDY, *near* pre-industrial: year 1900), accordingly these legacy effects are accounted for in this study, and thus the conclusions of this study should not be affected.

1.6 *Causal theory: One caveat that could/should be added is that this is only valid if the models, which are the basis for the sensitivity runs, are representing the truth. E.g. for the browning trend, you would actually find NO causal attribution from models alone, is that right?*

Yes, the causality is based on what the models predict for each counterfactual experiment and region. This is also true for every other method in ”Detection and Attribution” using model output, such as the optimal fingerprinting method. We integrate an explicit statement about this caveat in the revised version of the manuscript.

1.7 *Overall, I would recommend revisiting the statements regarding Zhu et al, mention caveats in counter-factual theory using models as surrogate truth, investigate the impact of 2015-2017 on the greening/browning trend in the later time-period.*

We again thank Prof. Dr. Christian Frankenberg for his comments. We will follow his recommendations when preparing the revised manuscript.

2 Specific Comments

2.1 *Line 36: Stomata can even respond at short time-scales when CO2 changes, stomatal density or max conductance takes time to adapt. (you mention ”in time”).*

Thank you. We have adapted this passage in the manuscript to address the different time scales on which the physiological effects of CO_2 act.

2.2 *Line 88: ”not dominant globally”. Again, I am having difficulty to not see a similar effect in Figure 3c. In line 421, you even say so yourself. I am a bit lost here.*

We understand the confusion. The effect is dominant when we look at the global-aggregate signal. However, when we look at the regional analysis, we find that the effect is not dominant everywhere (i.e., globally) as the globally-aggregated signal would suggest. We rephrase these statements to be more specific in the revised manuscript.

2.3 Line 449: weaken -> weaker

Thanks, we corrected the typo in the revised manuscript.

2.4 Sections 3.10+: I was just a bit confused as the discussions now move from causal theories to more local descriptions, partially just citing other papers to explain specific events. It also shows the limits of your causal method as the lack of drought legacy effects (e.g. in tropics) can potentially bias your mode sensitivity runs. For some effects that you mention are due to RF, it would actually be interesting to separate out effects of CO2 RF into VPD, temperature and PAR effects (due to cloud cover changes), CO2 RF has various impact factors, which can very regionally in importance...

Yes, we thank and agree with the reviewer, that it'd could be an interesting next step to further decompose the radiative effect of CO_2 into changes in VPD, temperature, and changes in shortwave radiation (PAR / cloud cover). Further, the physiological effect could be decomposed into the stomatal effect and the direct carbon assimilation stimulation effect (RUBISCO). We leave this analysis step for a future study, since this would go beyond the scope of this manuscript.

We show that models are limited in their predictive power in simulating vegetation response to climate change. To address this issue, we rely on the published literature to evaluate evidence in observations that confirm or refute the results based on the causal attribution study.

References

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.

Zhu, Z., Piao, S., Myneni, R. B., Huang, M., Zeng, Z., Canadell, J. G., Ciais, P., Sitch, S., Friedlingstein, P., Arneth, A., Cao, C., Cheng, L., Kato, E., Koven, C., Li, Y., Lian, X., Liu, Y., Liu, R., Mao, J., Pan, Y., Peng, S., Peñuelas, J., Poulter, B., Pugh, T. A. M., Stocker, B. D., Viovy, N., Wang, X., Wang, Y., Xiao, Z., Yang, H., Zaehle, S., and Zeng, N. (2016). Greening of the Earth and its drivers. *Nature Climate Change*, 6(8):791–795.