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Observational constraints on the response of high-latitude northern forests to warming

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Author Response to Peer Review Comments on 2020AV000228 and 2020AV000228R

Peer Review Comments on 2020AV000228

Reviewer #1

Using spatial pattern of GPP inferred from solar-induced chlorophyll Fluorescence in combination with net ecosystem exchange (NEE) inferred from column CO2 observations made by OCO-2, the authors found three quarters of the spatial variations in GPP and in the fPAR by the high-latitude northern forest can be explained by growing season mean temperature (GSMT). The authors further substitute space for time, estimating the historical warming trend in GSMT leads to 20% increase in GPP and seasonal cycle of NEE by ~20%. Overall, the manuscript is well written and easy to follow. Should the authors address my concern in a revision, I could recommend publishing the manuscript.

In the Abstract, the CO2 seasonal cycle amplitude has almost doubled only in some aircraft sampling (700 mb) around 70oN, the Barrow data and 500 mb aircraft sampling show 50% or even less increment during the same period (Graven et al., 2013). Thus, it is misleading to simply claim CO2 seasonal cycle amplitude has almost doubled.

In the Plain Language Summary, the authors suggest that their results imply future warming would lead to further woody encroachment and forest transition towards deciduous trees. I do not find evidence from the analyses to support these claims, please revise.

The analyses were performed with a very coarse resolution (4{degree sign} x 5{degree sign}), which is hard for me to understand. The authors claimed that "transport model has smaller transport errors over high latitudes at 2{degree sign} x 2.5{degree sign} resolution than at 4{degree sign} x 5{degree sign} resolution." But why the CMS flux with 4{degree sign} x 5{degree sign}, when 1{degree sign} x 1{degree sign} data can be available? The robustness of the spatial analyses should be tested with varying spatial resolutions, at least for high resolution satellite data, to confirm the findings are not coincident with the spatial resolution selected.

The space-for-time substitution is in the core of the paper, while this substitution may reflect long-term response to climate change, it is also possible to be confounded with other co-varying factors, such as nitrogen deposition and moisture variability. The limitation of this approach should be further discussed. If possible, it would be interesting to compared with tree-ring data, for example, to contest space-for-time approach.

I still have conservation about calling the warming-induced increase in productivity as "thermal fertilization". The CO2 effect is considered "fertilization" because its impacts are non-negative, similar to fertilizer applications. However, the warming impacts, even in the northern high-latitudes, could also be negative due to higher vapor pressure deficit (e.g. Novick et al., 2016). This analogy is not quite suitable in my opinion.

The reasoning regarding CO2 fertilization needs reconsideration. Even though the temperature relationship can well explain the interannual variations, the CO2 effects, as well as that from nutrient cycling, moisture condition may cancel out each other resulting in smaller residual magnitude. Similarly, the extrapolation to 2100 should also be drawn with cautions, as the 2-5 degree additional warming may goes beyond current norm and lead to non-linear temperature effects.

The response of GPP to temperature could be regulated by precipitation/moisture conditions (e.g. Wang et al., 2014; Reich et al., 2018). Should the authors find similar phenomenon?

Forkel et al. (2016) reported similar observed and simulated SCA change by one DGVM since 1980s. If considering the time period since the 1980s, will the authors s till find their estimates of NEE increment much larger than most biogeochemical models?

Page 5: Sullian -> Sullivan

Reference
Forkel, M., Carvalhais, N., Rödenbeck, C., Keeling, R., Heimann, M., Thonicke, K., . . . Reichstein, M. (2016). Enhanced seasonal CO2 exchange caused by amplified plant productivity in northern ecosystems. Science, 351(6274), 696-699.
Graven, H. D., Keeling, R. F., Piper, S. C., Patra, P. K., Stephens, B. B., Wofsy, S. C., . . . Bent, J. D. (2013). Enhanced Seasonal Exchange of CO2 by Northern Ecosystems Since 1960. Science, 341(6150), 1085-1089.
Novick, K. A., Ficklin, D. L., Stoy, P. C., Williams, C. A., Bohrer, G., Oishi, A. C., . . . Phillips, R. P. (2016). The increasing importance of atmospheric demand for ecosystem water and carbon fluxes. Nature Clim. Change, 6(11), 1023-1027.
Reich, P. B., Sendall, K. M., Stefanski, A., Rich, R. L., Hobbie, S. E., & Montgomery, R. A. (2018). Effects of climate warming on photosynthesis in boreal tree species depend on soil moisture. Nature, 562(7726), 263
Wang, X., Piao, S., Ciais, P., Friedlingstein, P., Myneni, R. B., Cox, P., . . . Chen, A. (2014). A two-fold increase of carbon cycle sensitivity to tropical temperature variations. Nature, 506(7487), 212-215.

Reviewer #2

This paper estimates the effect of temperature on the net carbon balance of high northern latitude forests (>50 deg N) and finds that temperature effects contribute 56-72% to the observed increase in the seasonal cycle amplitude of atmospheric CO2 at high northern latitudes. The key to getting this estimate is their use of spatial variations between temperature and GPP (normalised by PAR), and the assumption that the derived spatial sensitivity can be used to model the temporal sensitivity and thus trends over the past three decades. The data underlying this sensitivity analysis is a (to me unclear mix of) sun-induced fluorescence data from OCO-2 and GPP estimates from the FLUXCOM empirical upscaling model. The contributions to seasonal CO2 amplitude changes are made by using the estimated increase in GPP and TER (total ecosystem respiration) in an atmospheric transport model and comparing simulated CO2 concentrations with observations derived from two aircraft campaigns.

This is a useful analysis, made possible by the combination of diverse types of data and the simple, yet elegant assumption of the space-for-time substitute. There are many choices and assumptions that need to be made to get from actual observations to the results presented here. Some are discussed, but many are not (see points under MAJOR below). Most importantly, no actual validation is presented and we don't know how robust and accurate the space-for-time substitute is for simulating GPP-temperature relationships.

The contextualisation of results presented here is, in parts, not appropriate. It's argued here that "warming alone accounts for nearly all the observed fPAR trend is in agreement with prior observational studies that suggest CO2 fertilization is not a major contributor to carbon exchange in mature forests". However, their analysis (Fig. 4) suggests that the temperature trend explains only about half of the CO2 seasonal amplitude increase, still leaving substantial room for other effects, e.g., CO2 fertilisation.

The main result (important role of temperature for explaining the CO2 seasonal amplitude change) itself is not a surprise, yet the estimate for effects on the CO2 seasonal amplitude presented here a useful addition to the literature. The contribution of the present paper is to translate a relatively simple estimate of temperature effects into CO2 seasonal amplitude changes. This should be better reflected in their presentation.

I'm proposing major revisions before this paper can be accepted for publication to address open points regarding validation, discussion of assumptions, and explanation of methodological choices, as listed below.

MAJOR

- I don't like the term "thermal fertilization". In my understanding, 'fertilization' refers to increasing the availability of a substrate. Temperature is not a substrate. Warming rather acts on photosynthesis through the acceleration of biochemical rates and a relief of limiting effects under very low temperatures caused by photoprotection. I suggest to find a more appropriate title that clearly refers to temperature effects on the CO2 seasonal amplitude.

- The results should be contextualised more appropriately, in particular with respect to novelty and consistence with earlier results. For example, the greening was attributed by Zhu et al. (2016; DOI: 10.1038/NCLIMATE3004) predominantely to temperature at high northern latitudes. Keenan & Riley (2018; https://doi.org/10.1038/s41558-018-0258-y) have attributed the greening at high northern latitudes to warming. I also don't understand the basis for the statement in the abstract saying "Most ... biogeochemical models ... generally suggest a dominant CO2 fertilization effect on changes on NEE in the HLNF (and thereby the CO2 SCA)." This is not true. Zhu et al. (2016) show an attribution of observed greening to individual drivers, suggesting that temperature is the dominant cause for high northern latitudes.

- Missing validation: This is, in my view, the weakest point of this analysis, but I think that the gap can be filled. Doesn't the interannual time scale provide information that can be used to validate the space-for-time assumption for GPP-termperature relationships. The paper uses spatial correlations to estimate multi-decadal changes. But couldn't the same relationship also be used to estimate interannual variations in GPP? This may be limited by SiF data availability. Authors use OCO-2-SiF, but this is temporally and spatially sparse and covers only recent years. Alternatives may be GOME2-derived SiF or the new product by Duveiller et al., 2020. The same for fPAR: Fig. 3 shows a good correlation also at the interannual time scale. Couldn't this be presented as a validation of the space-for-time assumption? For GPP, one could also use FLUXNET data, which may be limited in the number of sites and total number of years covered, but may still contain useful information for validating interannual GPP variations estimated from the spatial relationships.

- Several key assumptions are not sufficiently discussed. For example, the space-for-time substitute assumes an immediate adjustment of vegetation structure. This is implausible and the analysis provides no insight into how big the related error is. The interannual time scale (also probably too short for vegetation structure to adapt) may yield relevant information.

- Some methodological choices imply assumptions that may introduce systematic bias, but the paper provides no insight into how sensitive results are to these assumptions. In particular, I am left wondering whether an extension of the growing season can be captured by the methods applied. The temporal resolution of underlying data is monthly and sensitivities are derived for different seasons. Fig. S4 shows a smaller spring sensitivity than in summer months. If the start of the growing advances, a given month that was previously classified as 'spring' may change to 'summer' and a different sensitivity would apply, but the method inflexibly ascribes that month to 'spring'. Another point: The spatial resolution is 4 x 5 deg. Is this sufficient to accurately determine a spatial gradient? I may be getting this wrong, but then, this indicates that methods are not described in sufficient detail. Leading to the next point...

- Methods are not clearly presented. A sometimes confusing mix of data sources is used (SiF, FLUXCOM GPP, fPAR, TRENDY [p. 9, top]) and it's not always clear why and how they are linked. For example, what is the rationale for analysing both GPP/PAR and fPAR? Such choices should be made explicit and motivated early on. Another example for unclear methods: Is spatial sensitivity derived for monthly data pooled from all gridcells? Or aggregated over the growing season? Is a constant growing season assumed per gridcell? In Fig. 4: It is unclear how data for the dashed line ("approximate observed SCA change from forest") was derived. What was assumed regarding trends in GPP and TER below 50 deg N for the simulation of the CO2 seasonal amplitude?

- Also regarding the separation of GPP vs. fPAR changes: in Abstract, authors write: "... three quarters of the spatial variations in GPP and in the fPAR absorbed by the HLNF can be explained by the spatial variation in the growing season mean temperature". This would imply that there is no change in LUE (GPP = LUE * fPAR * PAR) [PAR should not change greatly unless there has been a trend in cloud coverage - has there?]. Yet, at other points, LUE is reported to have increased (p. 15: "... the sensitivity of GPP/PAR to GSMT results from the sensitivity of both forest structure (fPAR) and light use efficiency, with similar contributions (0.090 vs. 0.100) from each (Figure 2 b and c)."). And in Discussions: "In particular, about 50% of the observed correlation between SIF and temperature is not explained by fPAR, but by the "light use efficiency"". Both can't be true at the same time.

- The quality of the introduction is found wanting in several instances. For example:

- Intro, p. 4: "Over the region northward of 50 degrees, precipitation substantially exceeds evaporation due to moisture convergence by the largescale eddy circulation, which results in a generally well-watered, thermally-limited ecosystem". The fact that these ecosystems are not water-limited is not only due to high precipitation, but also to low net radiation. Their ratio is relevant.
- Intro, p. 4: "Plants generally grow faster as CO2 increases, which is called CO2 fertilization (Kimball et al., 1983; Long et al., 2004)" CO2 fertilization is used as a term to describe a positive response to CO2 of different processes, ranging from photosynthesis, growth (NPP), to the net C balance.
- Intro, p. 4: "The optimal temperature for plants growth is higher than the mean annual temperature over high-latitude biomes (Huang et al., 2019)". Huang et al. quantify the temperature optima for photosynthesis, not growth.
- Result conflicting a large body of work on VPD effects on GPP is left undiscussed ("As shown in Figure S10, GPP increases with the increase in vapor pressure deficit, opposite to the expected relationship for water-limited ecosystems")

Reviewer #3

The manuscript "Thermal fertilization of the high latitude northern forests" by Liu and coauthors is a very interesting analysis that look at the effect of the sensitivity of GPP and TER to temperature and its effect on the amplitude of seasonal cycle of CO2. The authors use a combination of SIF and column CO2 observations from remote sensing, which is in my opinion the new interesting frontier for understanding global biogeochemical cycles. The authors used space for time substitution, by fitting relationships between GPP-T and TER-T in space and project in time. I find the article extremely interesting and congratulate with the authors for the idea and the analysis. The research question is timely and important. However, I have some comments that are related to the robustness of some assumptions, for instance in the space for time substitution approach, and the potential role of summer drought that is foreseen to increase in the higher latitudes. Please find my comments below.
Two small additional comments - please add the line numbers for the revision and please crosscheck the consistency between units and labels in the figure because often the same unit is reported in different ways (see comments below).

Page 5 - I completely agree the increase in temperature will stimulate growth but another important question is to what extent the increase of temperature can be exploited considering the day length is not going to change and the forests are also light limited.

At the beginning of the introduction I would also introduce the expected effects of temperature on TER, considering the article deals also with TER

Page 5 - space for time would require more attention also with respect to the adaptation and also if tested for fAPAR GIMMS I would suspect GPP and TER is different..

Controlling also for spatial variability of CO2 could be important - it is clear this is not as strong as the temporal but still is there a spatial gradient that can confound the relationship with T. A simple partial correlation analysis or variance partitioning can possibly help. Although in the northern latitude this problem could be minimized, at the fringe between more temperate and boreal areas the spatial variability in atmospheric CO2 concentration is not negligible and might confound the sensitivity to temperature. It could be this is tested already in the literature I am not aware of, in this case a citation would be enough. The authors refer to this at age 9 but looking at maps of atmospheric CO2 concentration seems to me that the differences are way larger than 10 ppm

Page 6 last line - I would say that SIF is a linear predictor under high light conditions and add at seasonal temporal scale to be more rigorous. Under high light conditions but shorter time scale SIF alone might not be sufficient

Page 7 - the most updated and comprehensive article on FLUXCOM GPP is Jung et al., 2020

Jung, M., Schwalm, C., Migliavacca, M., Walther, S., Camps-Valls, G., Koirala, S., Anthoni, P., Besnard, S., Bodesheim, P., Carvalhais, N., Chevallier, F., Gans, F., Goll, D. S., Haverd, V., Köhler, P., Ichii, K., Jain, A. K., Liu, J., Lombardozzi, D., Nabel, J. E. M. S., Nelson, J. A., O'Sullivan, M., Pallandt, M., Papale, D., Peters, W., Pongratz, J., Rödenbeck, C., Sitch, S., Tramontana, G., Walker, A., Weber, U., and Reichstein, M.: Scaling carbon fluxes from eddy covariance sites to globe: synthesis and evaluation of the FLUXCOM approach, Biogeosciences, 17, 1343-1365, https://doi.org/10.5194/bg-17-1343-2020, 2020.

Fig S1 panel a - I suggest to include SIF in the y axis label and not only the unit

Fig S1 - I suggest to indicate clearly the meaning of the color bar (I guess is the latitude)

Fig S1 Can one actually concludes hat the T sensitivity is the same between SIF-T and GPP-T considering such a difference in the units? Can the authors test this by rescaling the Y axis to a common range, fitting the curve applying a bootstrap, and estimating the posterior distribution of the parameters (in particular for the coefficient of T). I guess will not change too much but I think is worth to try and to have a statistical confirmation about the statement. The discussion on the scaling factors GPP-SIF goes in this direction, but then I would say is even more important to either compute GPP from SIF and then calculate the sensitivity or try to make comparable panel a with b, c, d.

Page 8 The dependence on TER to temperature can be related indeed to photosynthesis as the authors suggested (and this is quite clear from the curvature of TER-T relationship that increase from Spring to summer and decline in autumn). However, in particular for the spatial patterns (i.e. within the same season) there are other factors that can be more

important and are related to standing biomass (and the latitudinal gradient is pretty clear in this sense) but also soil organic carbon and sensitivity of microbial activity to temperature. Photosynthesis is important but not the only driver of the relationship. I suggest to add few considerations in this direction and one article to cite is Davidson and Jannssen 2004 for instance (https://www.nature.com/articles/nature04514), or even better (https://onlinelibrary.wiley.com/doi/full/10.1111/j.1365-2486.2005.01065.x).
The interaction aboveground biomass, soil organic carbon, and temperature can confound TER-T and I suggest trying to control for these controlling factors using aboveground biomass products and SOC from soil grid map. Otherwise the risk is that T sensitivity can be confounded and perhaps amplified.

Fig S2 - I suggest to include the label in the y axis and not only the unit

Page 9 - when discussing the space for time substitution please cite also the recent discussion (on semi arid areas but some of the argument can be brought here). I like a lot the idea of the authors to look at the fAPAR in space and time, this is solid and give a lot of confidence on GPP, but still for TER I would be careful and I would try to analyze if there are other drivers of the relationship in space (that can have then an impact when used in time).

Page 10 - another factor to include in the story is the potential summer drought due to increase in vegetation activity earlier in the season. This can both affect GPP and TER. This has been shown clearly for the high latitude ecosystem. I suggest the authors to add sentence that this can happen and discuss this point. See Buermann et al., 2018 Nature (https://www.nature.com/articles/s41586-018-0555-7).

Figure 1 and throughout the manuscript - Here the units are in KgC/m2 while in other figure KgC m-2. I suggest to keep consistency. Sometimes there is no variable name in the y axis (only the unit) sometime both - please

Page 14 the dependence on LUE by PFT it is probably related to nitrogen content in leaves (for instance deciduous trees have larger N content per area than conifer). Perhaps something to add.

Figure S10 c - The spatial relationship GPP-VPD is driven by temperature. Also I would expect that variance of mean VPD in space is less than in time. My suggestion would be to remove this part. In my opinion is questionable that this relationship can stand in time.

The section on the effects of the "thermal fertilization" on the amplitude of the seasonal cycle should be rediscussed in my opinion when the points above are analyzed.

Peer Review Comments on 2020AV000228R

Reviewer #1

I have reviewed the changes made by the authors. I am satisfied. The paper may be accepted for publication.
Shilong piao

Reviewer #3

I went through the answer of the authors. Indeed the authors did a good job.

My two main concerns were on the fundamental assumptions that certain relationship in space can hold in time - and can therefore be used to speculate on the effect of thermal fertilization (term that they removed and I agree)on the biospheric response.
The authors did a good job in answering these questions and they proved in a solid way, in particular for the CO_2 effect, that this is not the case and that the study is solid.
Also I asked a number of editorial improvements and to add some citations that they introduced and discussed.

Despite scientifically the manuscript is mature the presentation is still not 100% satisfying.

The figures can be still neater though, many Units are again missing or do not follow the scientific standards and the presentation could be better. This is of course a minor issue but I invite the authors to improve the presentation:

Figure 1 a, the unit of Temperature should not be celcius
(https://www.nist.gov/pml/weights-and-measures/si-units-temperature)

Figure 4 right axis - label is missing
Figure 2 unit missing on the x

Same in the supplements:
Fig S9, in my version the maps are overlaid with the name of the months
Figure S16 Units of Temperature missing
Fig S19 label missing in the y axis