Earth Syst. Dynam. Discuss.,
https://doi.org/10.5194/esd-2018-71-RC1, 2019

Earth System Dynamics
Discussions

Interactive comment on "Limitations of Emergent Constraints on Multi-Model Projections: Case Study of Constraining Vegetation Productivity With Observed Greening Sensitivity" *by* Alexander J. Winkler et al.

Anonymous Referee #1

Received and published: 13 January 2019

The large disagreement of projections of future net land-atmosphere CO2 flux in Earth-system models is the biggest uncertainty in future climate projections (Arora et al., 2013; Friedligstein et al. (2014). To tackle this issue, the application of emergent constraints (EC) to different carbon-cycle and ecosystem processes to reduce the range of the future land-sink estimates has become increasingly popular (Cox et al., 2013; Wenzel et al., 2014; Mystakidis et al., 2016; Wenzel et al., 2016). In this study Winkler et al. discuss the reasoning behind the application of EC in Earth-system modelling. They point to potential limitations, such as the need to accurately measure

Printer-friendly version

Discussion paper

the predictor and to find a robust relationship between predictor and predictand, and how that might change over time. They then use the sensitivity of Leaf-Area Index (LAI) to CO2 and temperature to constrain future estimates of Gross Primary Productivity in the Northern High-Latitudes.
In my opinion, the theoretical examination of the EC framework, sources of uncertainty and its limitations is particularly noteworthy and useful for the community (discussion around Figures 1, 4 and 6). I find the manuscript in the present form rather strenuous to read, without a fluid structure, several repetitions and sometimes omissions and inconsistencies that generate confusion. This can easily be improved during the revision: my suggestion would be to have a complete conceptual part discussing uncertainties and complications of the EC method before moving to the analysis of LAI data. There are, however, other points of this study that I find more problematic, and that need consideration before I can recommend its publication. I first describe my general concerns, and then include more specific comments for your consideration.

The introduction delves into the assumptions underlying the EC, different studies using EC to constrain the carbon-cycle sensitivity to global change and their limitations and uncertainties. I find that the introduction is missing a motivation statement that explains: (i) the need for the conceptual study presented here; (ii) why did the authors focused on the relationship between LAI and GPP (more on this below); (iii) the rationale behind the choice of trying to constrain ΔGPP in the NHL only, since models that do well at simulating the effect of boreal/temperate ecosystem CO2 fluxes do not necessarily constrain better the global terrestrial sink (Schimel et al. 2015, Figure 3). The description of Winkler et al. (p2, l25 – p3, l2) is partly (but with less detail) described in the methods. I suggest mentioning here just the relevant aspects of their study. However, from this paragraph, it seems that one of the main conclusions of this manuscript is also an outcome of Winkler et al. (2018) – I mean the values of 3.4 $\pm$ 0.2 PgC.yr^{-1} which are then presented again in the results section. This leaves me wondering to which extent is this study original, compared to that in revision in Nature

Communications. It's important that the authors clarify this, at least in their reply to the comments.

In the Methods section, the authors state that they "revisit the study of Winkler et al. (2018)" and "largely follow the methodology detailed in Winkler et al. (2018).". However, the reviewers (and potential readers) do not have access to this study to evaluate the methodology in detail nor to understand what exactly is being revisited. Moreover, that companion paper is not yet accepted for publication. Therefore, the authors should at least describe the methodology in more detail.

This is especially the case for the calculation of ω, which is then used for a big part of the analysis of LAImax drivers. You explain that a PCA is performed on both variables (CO_2 and GDD0) to derive a proxy time-series that summarizes the evolution of both variables. The PCA is indeed suitable for such type of analysis and is probably better than multiple linear regressions used in other studies (e.g. Zhu et al., 2016). However, the authors give very little information about this crucial step of the analysis: is the PCA performed at pixel level, or for the large-scale aggregated values? What components do they retain from the PCA? (I'm assuming only PC1 is retained) What fraction of the variance does it explain? How does it relate to GDD0 and CO_2? How does it vary over time? Here, a plot showing ω over time would be very helpful. Moreover, the authors should keep in mind that ω does not "represent the overall forcing" (p9, l8-9), but only CO_2 and temperature.

The authors correctly state that one requirement of the EC method is that "a physically (or physiologically) based correlation between inter-model variations in an observable entity of the contemporary climate system (predictor) and a projected variable (predictand)" (p2, l26-27) exists. I find it, therefore, striking, that the authors do not discuss in any way why should LAI be used as a predictor of the CO2 fertilization effect on GPP, and whether the linearity between the two variables in ESMs holds true for observations. Experimental CO2 enrichment studies did not find a direct effect between CO2 fertilization and increase in LAI (e.g. Körner et al., 2005) and LAI seems to increase non-linearly with increasing CO2 (Norby et al., 2005). Moreover, Norby et al. (2010) found strong influence of nutrient availability/limitation (not simulated in most CMIP5 ESMs) in the CO2 fertilization effect on ecosystem productivity, possibly because of mycorrhizal effect (Terrer et al., 2016). DeKawe and Medlyn (2014) have also shown that under increasing CO2, allocation of carbon to leaves decreased, rather than increasing (as implicitly assumed here), which was not well simulated by DGVMs. The link between CO2 fertilization, LAI and GPP is further complicated by how models simulate mortality and disturbances.

I understand that the authors have a stronger background on earth-system modelling and I would not expect them to make a full case on the relationships between CO2 fertilization, LAI and GPP. However, since they describe so well the need for a physical basis to the EC, they need to explain the choice of LAI as a predictor of future GPP (i.e. evidence for a mechanistic link), and whether the land-surface models composing the ESMs are able or not to correctly simulate the relevant processes for this relationship (see also Smith et al., 2016). In the current version of the manuscript, the authors do not make a strong case for their choice, and there is limited evidence (mostly from model-based studies to the best of my knowledge) to suggest that LAI sensitivity to CO2 can be a suitable predictor of future GPP. The authors could, for example, combine their analysis of LAI_{max} sensitivity to CO2 and temperature with GPP changes estimated from observation-based datasets (e.g. FLUXCOM).

Specific comments:

P1, L 2: “promising results” of what?

P1, L3: What do you mean by “difficult to measure variable [. . .] at a potential future”? If you are trying to estimate a future state of a variable, it is by definition non-measureable?

P1, L7: “greening sensitivity to the CO_2 forcing” . . . but also temperature, right? (Methods).

P1, L18: Is the value of the GPP enhancement from this study or from Winkler et al. (in revision)?

P2, L4: “can have substantial uncertainties” → remove can. They have.

P2, L8: I’d move the “large-scale climate modes” to the paragraph about natural variability a few lines below.

P2, L12: “aims is to explore” → “aims to explore”

P2, L21: “namely, AS a method. . .”

P2, L24: In theory, could another relationship (non-linear) be used?

P2, L27: what do you mean by difficult to observe? Cox et al. (2013) used two variables that are relatively well observed (CO_2 growth rate and tropical temperature).

P2, L32: What do you mean by “confirmed”?

P3, L17: “2xCO_2 world”: you mean in model simulations, not in CO_2 enrichment experiments, right?

P6, L2: Here you mention that you also use precipitation to derive ω, however later you mention only CO_2 and GDD0 were used. If you don’t use, can you justify the exclusion of precipitation (non-significant trends? Non-significant effects?)

P7, L4-5: can you provide any lines of evidence to justify the assumption (non-model based).

P8, L4-5: What do you mean by “difficult to measure”? It’s already repeated 2 times before.

P8, L6-9: What evidence do you provide for this? CO_2 enrichment experiments contradict this assumption.

P8, L15: “large area” $\rightarrow$ “large-scale”?

P8, L16-32: This is somewhat confusing since up until now you mention that you will analyse NHL. Please reformulate before in other to make clear that first you look at global values, and then focus on NHL (and provide justification to do so).

P8, L19-21: How much does GDD0 contribute to ω in the tropics? Can the low sensitivities in the tropics be due to your choice of temperature variable? I do not expect GDD0 to be a relevant temperature variable in the tropical band. . .

P9, L2-3: Indeed, but perhaps this is because of your inadequate choice of predictor for temperature (GDD0, rather than annual T, or some other metric)?

P9, L8-9: not the overall forcing, just two components of the forcing (CO2 and temperature). Please show the time-series of ω.

P9, L17: “all pixels”: of the globe, or just NHL?

P9, L26-29: Where do you show the corresponding increase in plant productivity? Where can I see that the distribution is approximately the same for the two variables? And if you have this data, where do you get GPP from, models or observations? Can you plot the GPP distribution for the same choice of pixels?

P10, L3-8: Is this also valid for ESM outputs?

P10, L19: What do you mean by “LAImax sensitivity cannot be accurately estimated irrespective of the window length”.

P10, L20-21: Do you mean the signal to noise ratio of ω? Unfortunately you don’t show the time-series, so it’s hard to follow.

P10, L23-26: But, in theory, that’s the aim of the EC method. Do you mean that before considering using a given EC, one should evaluate the stability of the sensitivities?

Printer-friendly version

Discussion paper

P10, L29-30: It's not really shown in Figure 4.

P11, 4-6: Very good way to pose the question. But can you answer this in a pure model world? I'm not fully convinced.

P11, L8-10: Before (and after) you always use 7 models. It's not clear which model set is being used for which analysis. Are you using only 3 models to constrain future GPP changes? This does not seem consistent with Figure 6.

P11, L11-18: Not that surprising since all models are based in some way or another in the Farquhar photosynthesis model, which for the ppm ranges of 1xCO_2 and 2xCO_2 can possibly be approximated by a linear function, and in DGVMs the allocations schemes to leaves are strongly coupled to GPP (e.g. models don't simulate well non-structural carbon reserves, or changes in allocation)? Also, if models prescribe fixed LAImax (as some do), then this will strongly depend on the chosen model parametrization.

P11, L18: Why not call it simply "thought experiments" or "conceptual experiments", for non-german readers?

P11, L21: What do you mean by LAI? Annual values? Growing-season average? And why not LAImax?

P11, L24: ". . . responses" → add something like "of GPP to CO2 and of LAI to GPP" for clarity.

P11, L26 – P12, L5: Why did you choose Scenario 3? Scenario 4 in Figure A2 is much more plausible (GPP saturating for high levels of CO_2 because of basic physiology (Farquhar)).

P12, L9-10: "timing of saturation": where can we see this?

P12, L20: what LAImax are you referring to here? I assume you used AVHRR, since you explained (well) why MODIS is not suitable. But you need to clarify.

P12, L24-26: in the model world. You need to discuss whether observations support this.

P12, L26: I assume you mean "LAImax sensitivities" to ω. Is this simulated ω or ω from observations? Over which period? If it is simulated ω you need to show how ω from historical simulations compares with ω from observations.

P13, L14: do models simulate compositional changes in these simulations? I.e. do they all include dynamic vegetation changes?

P13, L34: But observations seem to point out that climate change (warming and drying) probably cancels out the CO_2 fertilization effect (Penuelas et al., 2017), because of processes not well simulated by CMIP5 models - climate extremes, particularly heatwaves, mortality, disturbance – and further reinforced by nutrient limitations (also not simulated by most CMIP5 models).

P14, L8-9: Can you provide references for this?

P15, L2: is this an original result from this manuscript or from Winkler et al. in revision?

References

Arora et al., Journal of Climate (2013) 26, doi: 10.1175/JCLI-D-12-00494.1
Cox, et al. Nature (2013) 494.7437, 341.
DeKawe and Medlyn (2014), New Phytrologist, 203, 883-899.
Friedlingstein et al., Journal of Climate (2014) 27, doi: 10.1175/JCLI-D-12-00579.1
Körner et al. (2005), Science, 309, 5739.
Norby et al. (2005), PNAS, 102,50.
Mystakidis et al. Global Change Biology (2016) 22, 2198–2215, doi: 10.1111/gcb.13217
Schimel et al. PNAS (2015), 12, 2.
Smith et al. (2016) Nature Climate Change, 6, doi: 10.1038/NCLIMATE2879
Terrer et al. (2016) Science, 353, 6294.
Wenzel et al., J. Geophys. Res. Biogeosci., 119, 94–807, doi:10.1002/2013JG002591
Wenzel et al., Nature (2016), 538, 499-501.
Peñuelas et al. (2017) Nature Ecology and Evolution, 1, 1438-1445, doi: 10.1038/s41559-017-0274-8
Zhu et al. (2016) Nature Clim. Change, 6, 791–795.

Interactive comment on Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2018-71, 2018.