Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-272-RC1, 2019

Biogeosciences
Discussions

Interactive comment on "Low sensitivity of gross primary production to elevated CO_2 in a mature Eucalypt woodland" *by* Jinyan Yang et al.

Simone Fatichi (Referee)

simone.fatichi@ifu.baug.ethz.ch

Received and published: 24 August 2019

Overall Review

The article presents a detailed exercise of upscaling photosynthesis from the leaf scale to the stand level for the climatic conditions and vegetation distribution corresponding to the EucFACE experiment (forest stand dominated by Eucalyptos tereticornis) and thus it includes ambient and elevated CO2 (eCO2) scenarios. Upscaling leaf-level response to tree and forest stand scale is a long-standing problem in biogeoscience and while it has been tackled in various ways in the literature, the study presented here is innovative for the thoroughness and level of detail included in the analysis. Furthermore, the analysis is carried out for ambient and eCO2 conditions using a terrestrial

Printer-friendly version

Discussion paper

biosphere model (MAESPA) that represents explicitly each tree and solve the canopy using multiple layers and accounts in each layer for multiple points representing radial variability in incoming light. The study also accounts for the acclimation response of photosynthesis and it is strongly constrained by observations, which is rarely the case in other similar studies. The study convincingly shows that a strong increase in leaf-level light-saturated photosynthesis (+33%) under eCO_2 reflects in a minor increase in stand level GPP (10%) because of the prevalence of electron transport limitations in photosynthesis and to a minor extent downregulation of photosynthetic capacity due to the leaf acclimation to eCO_2. Results also show a large uncertainty in computing GPP at the stand level when a small area (corresponding to a CO_2 enrichment ring) is considered. While upscaling photosynthesis at the forest stand scale is not a new task, the way this problem is solved here, represents a scientific advancement because it is presented in the context of a FACE experiment and provide a number of interesting discussion points on mechanistic model parameterization and uncertainties (e.g., the role of the curvature for electron transport, the $J_{c,max}/V_{c,max}$ ratio, photosynthesis acclimation, forest stand heterogeneity). It is clearly shown that translating leaf-level responses of CO_2 effects to the ecosystem scale is very misleading and most important the study provides mechanistic explanations for the differences. The search for the reasons and the clear explanations provided concerning subcomponents of the photosynthesis model (e.g., Rubisco vs. electron transport limited, or acclimation of photosynthetic capacity) represents an innovative approach, which I did not see before in the literature. For these reasons, beyond the importance of estimating GPP in ambient and eCO_2 conditions that will serve future studies in the context of the EucFACE experiment, the article represents an important piece of work for the mechanistic understanding of ecosystem responses to elevated CO_2.

The article is overall very well written and presented. In summary, I think the manuscript is making an important contribution to the field and I sincerely congratulate the authors for this nice piece of work. In the following, I just have a number of minor comments that can be helpful to improve further the presentation of this work.

Sincerely,

Simone Fatichi

Minor comments

P.2 Line 34-37. The difference between canopy scale "direct response" of +11% and the mean actual response of 6%, while very clear in the manuscript, it is not so clear at the abstract level. Maybe introducing the concept of "uncertainty" associated with the variability across rings or something associated to the "actual field response" according to the experimental configuration may help.

P.3 Line 51. The "hence" here is out of place, because the causality is not straightforward. An increase in carbon uptake does not necessarily lead to an increase in the amount of carbon stored in the ecosystem. The authors are well aware of this. Something like "which in turn could potentially increase..." will be more correct.

P.3. Line 57. A short overview of main disagreements between various studies is provided in Fatichi et al. 2019.

P.3. Line 57-68. I think this paragraph would benefit from referring to the estimates of global terrestrial C sink. While the attribution of the land C-sink is still debated, an average C-sink of 20–30 g C year-1 m-2 over vegetated land in the last decades is not a detail in the overall story about eCO2.

P. 4. Line 80. While, practically, I would agree in defining the response of GPP to eCO2 an upper bound. Theoretically, this is not a limit, if for some reason, plants in eCO2 conditions will be able to do maintenance with half of the respiration costs, then the NPP response could be larger than the GPP response. I think a "reference value" is more correct than an "upper-bound".

P.4. Line 115 and P.6 Line 161 and 166. Yang et al. 2019 is missing from the reference list, overall, I would avoid referring to papers, which are not published.

P.4 Line 116 and P.6 Line 170-171. I would not mix the "meteorological forcing" with the "model parameterization". The two aspects are different from a modeling perspective, one represents the inputs to the model, the other (e.g., physiological and structural attributes) represents model parameters, or prognostic variables if these are time-dynamics and computed in the model. One can use the same model parameterization with different meteorological inputs and the other way around.

P.6 L.173. Figure 2b. I am strongly encouraging to avoid using a linear interpolation for soil moisture values at least in its graphical representation. Soil moisture temporal dynamics have a fast and strong response to rainfall events. Linearly interpolating biweekly value is creating a misleading perception of the real temporal dynamics of soil moisture. I would prefer to have just the points when the soil moisture values have been collected rather than the current representation where raising and descending soil moisture dynamics are often unrealistic.

P.7. Line 220. Just a suggestion. Maybe the Fig. S1 could be included in the main manuscript.

P.9 Line Table 1. I know that in literature it is quite typical to report μmol only. However, this is not very precise, especially when we are dealing with photosynthesis. I would suggest to explicitly say μmol of what, e.g., μmol-CO2, μmol-H20, μmol-electrons or better μmol-Eq. as in the original Farquhar et al. 1980.

P.10. Line 286-290. Did you check if with MAESPA you get the same +33% of leaf-level photosynthesis if you simulated the same environmental conditions of the 600 A-Ci curves? Very likely, yes, because these are used to estimate the photosynthesis parameters, but just as a double check.

P.10 Figure 6 and 7. In Fig.7 is reported incident PAR and in Fig. 6 absorbed PAR, even though one refer to the stand scale and the other to the leaf-level, I think it would have been better to use either absorbed or incident PAR in both of them for comparison.

P. 11. Line 318. From the Supp. Material, the curvature for electron transport θj is also used as curvature and for overall photosynthesis (Eq. S8). These two values are typically different in models (e.g., Bonan et al 2011). This needs to be specified in the manuscript as well. The reference θj =0.85 is typically assumed for the curvature and for the overall photosynthesis, rather than for the curvature of electron transport, which is typically lower in some models (0.7, Bonan et al 2011, Fatichi et al 2016). This needs to be discussed.

P. 12. Line 377. I am honestly impressed by the inter-ring differences in GPP. I think these are mostly related to the relative small size of the rings. Or better, the size is quite large in comparison to experimental capabilities but relative small to average forest stand heterogeneities.

P.12 Line 388. Renchon et al 2018 is not in the reference list.

Eq (S3) The denominator should be Ci+2Γ rather than Ci+ Γ (e.g., Wang and Leuning, 1998, Dai et. 2004, Bonan et al 2011);

References

Bonan, G. B., P. J. Lawrence, K. W. Oleson, S. Levis, M. Jung, M. Reichstein, D. M. Lawrence, and S. C. Swenson (2011), Improving canopy processes in the Community Land Model version 4 (CLM4) using global ux _elds empirically inferred from FLUXNET data, Journal of Geophysical Research, 116 (G02014), doi:10.1029/2010JG001593

Dai, Y., R. E. Dickinson, and Y.-P. Wang (2004), A two-big-leaf model for canopy temperature, photosynthesis, and stomatal conductance, Journal of Climate, 17, 2281-2299.

Farquhar, G. D., S. V. Caemmerer, and J. A. Berry (1980), A biochemical model of photosynthetic CO2 assimilation in leaves of C3 species, Planta, 149, 78-90.

Fatichi S., C. Pappas, S. Leuzinger, J. Zscheischler (2019). Modelling carbon sources and sinks in terrestrial ecosystems. New Phytologist, 221:652–668, doi:10.1111/nph.15451

Fatichi S., S. Leuzinger, A. Paschalis, J. A. Langley, A. Donnellan Barraclough, and M. Hovenden (2016). Partitioning direct and indirect effects reveals the response of water limited ecosystems to elevated CO2. Proceedings of the National Academy of Sciences USA, 113(45) 12757-12762, doi: 10.1073/pnas.1605036113

Wang, Y.-P., and R. Leuning (1998), A two-leaf model for canopy conductance, photosynthesis and portioning of available energy I: Model description and comparison with a multi-layered model, Agricultural and Forest Meteorology, 91, 89-111.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-272, 2019.