

Review: Leaf Area Index identified as a major source of variability in modelled CO2 fertilization

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Synopsis:

In this paper, the authors run CABLE for seven C3 vegetation types, without nutrient cycling, and calculate CO2 fertilization for the RCP 8.5 scenario. CCSM simulations from 1901 to (the paper says 1910; I assume they mean 2010) holding carbon-climate feedbacks constant (driving the model with the averaged meteorology-I'm guessing average annual cycle, although the authors do not say) and feeding CABLE increasing CO2 concentration from the CCSM RCP 8.5 results.

They find that CO2 fertilization differs between PFTs, and decreases with time during the period 2011-2100. Fertilization is relatively constant both between PFTs and when the calculation is made on a per-unit leaf level, and shows much larger diversity both across PFTs and when the CO2 fertilization is calculated on a unit-leaf vs. integrated canopy basis. The authors close with the claim that simulated LAI is critical to the calculation of CO2 fertilization in climate simulations.

Review:

I have 2 major problems with this paper. Either one by itself, I believe, is fatal, but taken together I cannot make any recommendation for this paper other than rejection.

Problem #1: There is a rich body of literature from the FACE experiments that claims, pretty much unequivocally, that nutrient cycling and/or limitation becomes more and more important to CO2 fertilization as CO2 concentrations rise. Yet, in this experiment CABLE is run with nutrient cycling turned off!

Coskun et al. (2016) and references therein has a nice summary of both Free-Air CO2 Enrichment (FACE) as well as Open-Top Chamber (OTC) experiments. Smith et al. (2015) discusses the divergence between multiple models and a satellite-derived product that underscores the importance of the interaction between nutrient cycling and CO2 fertilization. Many of these studies focus on N limitation, although some research has indicated that P limitation is a factor as well (e.g. Hasegawa et al., 2016). These, and other studies, all conclude that understanding of CO2 fertilization requires taking nutrients into account.

Schimel et al (2014) demonstrate that, depending on the model, inversions can disagree on the location of the dominant location of the terrestrial sink (tropics or northern hemisphere). Quantification of the fertilization sink in a single model may bring information to bear on this uncertainty, as CO2 fertilization is really the only way to get a sink in the tropics, while other carbon-climate feedbacks (season lengthening, woody encroachment, forest regrowth) may also be possible at higher latitude. Studies that quantify CO2 fertilization may be able to shed

light on this discrepancy, although the interaction between higher levels of atmospheric CO₂ and nutrients must be considered.

I have to confess that I was very surprised when I read that the authors ran the version of CABLE without nutrient cycling included. I am not a FACE 'expert', but even I am aware of the amount of research that has concluded that nutrient cycling is critical to understanding ecosystem-level response to higher atmospheric CO₂. I found it very suspicious that nutrients were excluded from the study. Why, when there is this large body of work demonstrating the nutrient cycling is critical to understanding CO₂ enrichment, would nutrients be turned off in the model? The authors claim that nutrients were turned off for 'simplicity', but the obvious answer, and one that I suspect to be the truth, is that the authors did run CABLE with nutrient cycling, and model pathology and/or unrealistic results ensued.

It may have been possible to evaluate a nutrient run, even if the results were unrealistic, and evaluate how atmospheric CO₂ levels and nutrients interact in CABLE. The results may have provided an opportunity to evaluate or comment on the divergence of models in their predictions of atmospheric CO₂ levels and source/sink strength (e.g. Friedlingstein et al., 2006, 2014). By not including the critical nutrient interaction, I'm not sure that the results presented here give the reader any insight into how ecosystems might realistically respond to increasing future CO₂ levels in the atmosphere.

Problem #2: Without carbon-climate feedbacks and nutrient cycles, I don't think a model actually has to be run to determine CO₂ fertilization. You can probably perform the calculation directly from the equations in the code. Between models there will be some differences:

- Is the model an enzyme-kinetic model (Farquhar et al., 1980; Michaelis-Menten kinetics), or light-response (e.g. VPRM, Mahadevan et al., 2008)?
- how is stomatal conductance calculated? Does it use Ball-Berry, with a dependence on relative humidity, or Leuning, which uses VPD? How is transpiration coupled to photosynthesis?
- What are the parameter values for V_{cmax} for a given PFT?
- What determines phenology? Is allocation static, or, if it is dynamic, how does it change during the year and in response to what?

I believe it would be possible to determine the constraints on CO₂ fertilization for a suite of models without actually running any of them.

It is axiomatic that leaf-to-canopy scaling (LAI) is critical to total CO₂ fertilization amount. Every model that I am aware of calculates biophysics on a per-unit-area basis and then scales to the canopy level either by summing over sunlit/shaded leaves (and PFTs) or integrating from leaf to canopy scale along the lines of Sellers (1985, 1992)(OK, a gap model like ED2 may be a little different). Canopies with an LAI close to 1 (think of grasslands) will not see much difference from unit- to canopy-scale, more dense canopies (like forests) will.

If there is a large divergence between models in LAI (and GPP) for a given PFT, or if there is a large trend in one model's LAI for a given PFT during a climate run, then these might be valid topics of analysis. Finding that LAI is critical to canopy-level CO₂ fertilization (without nutrients being considered) does not really bring anything new to the field.

Sunlit and shaded leaf partitioning is fairly well-constrained and sunlit LAI can never get much above 1 to 1.5 or so even under the most direct-sun conditions. Solar angle and leaf angle distribution make it possible to exceed an LAI value of one. I know that CLM has had issues with shade leaf LAI becoming excessively large. The authors do not discuss total LAI in CABLE during their fertilization runs, and this makes me suspicious-if their shade-leaf LAI is becoming unrealistically large, that might be a reason why fertilization strength decreases with time; increase in the amount of sunlit leaf may result in large change in GPP, but once sunlit LAI is filled, any additional canopy growth will be as shade LAI, and GPP increase will be attenuated.

I just don't think there's anything new here. Without nutrient cycling the CO₂ fertilization results don't have much meaningful application, and the fact that leaf-to-canopy scaling is important has been known for a long time.

Specific comments:

- English prose and grammar, while readable, need attention. There are multiple places, too many to list, where errors exist.
- There is no explanation for what eCO₂ is (elevated CO₂). Don't assume all your readers know the definition.
- There is no definition of 'gamma' either.
- In many of the equations the equals sign is obscured. More effective spacing will make these equations easier to read.

References

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