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Interactive comment on "Leaf Area Index identified as a major source of variability in modelled CO_2 fertilization" *by* Qianyu Li et al.

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Li et al. use the CABLE model to explore the role of LAI in variability in the CO2 fertilisation response. The analysis has some interesting aspects which I'm sure will be of interest to the modelling community, in particular I thought fig 5 was interesting. However, I think the manuscript could be carefully revised for greater impact and insight. I have a number of specific points below but also 4 key issues with the analysis as presented:

1. I don't understand the logic of using a model which simulates N and P cycles and then switching this functionality off to understand the CO2 fertilisation response? In my eyes, this is one of the great strengths of this model. So to not compare C against N

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and P, or C against N, is a missed opportunity. Whilst I'm realistic enough to envisage the authors won't rethink this strategy, I do feel this requires some further justification.

2. It is stated that CABLE is largely RuBP-limited (line 179) and this point is given no further analysis. This is interesting and it isn't clear why this would be the case? Do the authors envisage that this is also true of other models? I would suggest it isn't but would be keen to read the authors thoughts on this. Surely this shapes the analysis (responsiveness to CO2)? So it warrants more than a single sentence that simply says "not shown" ...

3. The paper is about CABLE but surely the aim is to make the result general (otherwise the title would have the word CABLE...)? However, I wonder if I was developing JULES or CLM, (etc) what my take home messages would be? The authors urge other modelling groups to repeat their analysis, but could they also make suggestions as to the implications for other modelling groups? How do these results help us to understand model responses to CO2? The CMIP5 concentration-carbon feedback factor?

I didn't take much in the way of insight from the current section on this topic, i.e. section 4.3. For example, the authors assert that "It can be inferred that normalized leaf-level ðİŻ¡ values would diverge little across different land surface models as long as they use ...". Is that true? If the models had different levels of water stress (which they almost always do) they would get very different values of Ci even with the same model assumptions. As the authors also show, leaf temperature affects gamma_star, so I see no reason to assume that models would predict similar leaf temperatures. Leaf temperature itself is dependent on a whole range of assumptions. I've never seen any evidence that models with different architectures, with different assumptions about leaf-to-boundary conductance, etc, would predict similar leaf temperatures. If the authors disagree they should support these assumptions. The authors cite the Hasegawa et al study as an example of a consistent result of their conclusion. But wouldn't a number of the other model CO2 papers that point to marked divergence argue otherwise. My sense is their conclusion here is too simplistic.

The authors argue for the importance of LAI but don't really consider the role of allocation or turnover in great detail. Surely this is the key reason different models arrive at different LAI values? Even if you ignore changes in allocation/turnover due to CO_2, this impacts on the scaling terms that the authors focus on.

4. The results are considered on a PFT level, but presumably they vary in interesting ways within a PFT (i.e. in space). Would this be worth showing or exploring further?

Specific comments =================

- Line 43: Could you explain the CO_2 fertilising effect further? The text as written expects the casual reader has significant background knowledge for the second sentence of your manuscript.

- Line 48: 4 or 4.5? What does that mean, do you mean 4 to 4.5? How can it be OR?

- Line 49: the reference to the Smith et al. paper ignores a technical comment on this paper: De Kauwe et al. (2016). Satellite based estimates underestimate the effect of CO_2 fertilization on net primary productivity. Nature Climate Change, 6, 892-893. This is important as the authors are using this study to leverage their question. See also point on line 340.

- Line 51: it isn't "reality" - the satellite estimates are also model estimates.

- Line 54: "increasing temperature in models" why is temperature being introduced as a factor here? Isn't the focus solely on the CO_2 fertilisation effect rather than the than carbon-climate feedback factor? There are further studies cited in this paragraph which should be removed if the focus of this paper does not consider the carbon-climate feedback factor.

- Line 67: Despite models using apparently similar photosynthesis models, Rogers et al. (A roadmap for improving the representation of photosynthesis in Earth system models. New Phytologist, 213, 22-42.) showed some important differences. It would be worthwhile highlighting this study in the context of the section of the text.

- Line 72: what does carbon storage have to do with this sentence?

- Line 76/7: seems a narrow characterisation of the literature, the De Kauwe et al. 2014 study that the authors cite, explored these issues in depth.

- Line 81: Why would a high "basic" (delete basic) NPP necessarily lead to tropical regions having the highest stimulation by CO2? Wouldn't the opposite be expected? These regions have a high LAI and so would predominantly be light-limited and so have a more limited capacity to respond to CO2? Either way, the authors need to expand on this assertion.

- Line 89: Improved on what?

- Line 124: The assumption that Jmax25 = 2 x Vcmax25. Did the authors consider varying this assumption? Other models would make quite different assumptions about this ratio.

- Line 155: is there a citation, web link for "Community Climate System Model (CCSM) simulations"

- Line 168: the definition of S (line 171) needs to be moved up to this line.

- Line 215: just to clarify when the authors say total carbon storage - do they mean the soils too? Or just the plant? Or just the foliage pool? The equation isn't very clear. This also makes Fig 1 hard for me to interpret as I'm unclear what is being shown, I'm going to assume it is total plant carbon...

- Fig 1. Does it make sense to normalise these PFT lines? The authors say they decline but the magnitudes differ, the point is that the initial starting points are different too. This makes it hard for the eye to gauge.

- As a general comment the results need work, particularly in terms of transition text. For example 3.1 talks about the temporal trend in Bcpool and then switches immediately to the Ci/Ca ratio in 3.2? It is hard to follow the logic of the transition, is there is meant to be any connection for the reader?

- What is the point of Fig. 2? It isn't clear what this figure has to do with the story of the paper?

- The text around line 261 which refers to Fig 4 could do with further explanation. I personally don't find this particularly surprising, but the reader isn't offered much as the way of explanation. Presumably the change in slope as you move from B_GPP to B_NPP relates to respiration assumptions and then to B_cpool, allocation/turnover assumptions? I think the authors could go further in assisting the reader with interpretation. As currently written, the text simply highlights that the slope changes.

- I think figure 5 is very interesting.

- Line 290: I think this discussion of Fig S5 is interesting but I'm not sure I follow the interpretation? The LAI is the emergent outcome of the model assumptions - 1 leaf, 2leaf, multi-layer. Of course this assumption will lead to differences? But why you do the analysis on the leaf-level? Surely you're interested in the emergent outcome - the LAI. Most likely I simply misunderstood this point but I think it could also be explained further as it seems like an important point the authors are making.

- Line 295: I don't fully follow that interpretation? Your differences in Ci/Ca were small across PFTs? And the differences in leaf temp would be expected between PFTs? Certainly, fig 2 doesn't show any within PFT variation.

- Line 362: This is an assumption of the model and might not necessarily be true!

Technical corrections =====================

- Abstract: "vegetation types is 0.15-0.13", presumably you meant 0.13 to 0.15? Also, why don't the other variables (e.g. BetaGPP) have ranges too?

- First line of the introduction, makes no sense. You can't start a sentence with Terrestrial carbon sink and then a comma.

- Line 45: In Coupled -> In the Coupled
- Line 138: In CABLE model -> in the cable model

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