Review on

Robert J. Allen, "The Biogeophysical Effects of Carbon Fertilization of the Terrestrial Biosphere"

submitted to Atmospheric Chemistry and Physics (ACP).

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In simulations where the radiative effect of rising CO_2 is switched off, nevertheless a warming is observed. This warming is a consequence of the reaction of the biogeochemical reaction of the land biosphere on rising CO_2, namely the physiological effect of CO_2 fertilization. This goes along with a closure of stomata and thus reduced transpiration, as well as an increased plant productivity typically leading to an increased growth and thus a reduction in albedo. These effects – reduced transpiration and reduced albedo – together with their consequences (e.g. changed cloudiness) explain the warming observed in those simulations as is known at least since the pioneering study by Bala et al., 2006, soon followed by investigations with other models (e.g. [1, 2] and Cao et al., 2010). The new study by Allen re-investigates this behaviour taking advantage of the recent CMIP6 multi-model ensemble. I don't have a complete overview on the literature on the subject, but I suspect that the new aspects of this study are the comparative investigation of results from a rather large ensemble of models, and methodologically the usage of an energy balance decomposition as main tool.

Overall the study is well written, I like in particular the rather concise summary in the "Conclusions" section. Nevertheless, the mentioning of so many numbers in the text and the usage of so many abbreviations makes reading tedious. I personally would have preferred to leave numbers to tables or illustrative figures and restrict the text to qualitative discussions – I don't think that in this way the scientific content would deteriorate. Moreover, I am not really sure whether all the figures are really needed, in particular since from the many geographic details seen in the maps almost nothing is discussed. Maybe a visualization of the size of the different terms of the energy balance would be more illuminating than the many maps. But this is largely a matter of style to be decided by the author.

As far as I see, the results of this new study only confirm the insights obtained in previous investigations. Nevertheless, I think that because of the large range of models included in the analysis the paper could get an important reference on the subject. But for this a thorough review of the results of previous studies on the subject and also a comparison with their results should be added. Otherwise I fully support the publication of this study.

Major comments

1. The abstract should be more concise: Why do we need another study on the biogeochemical effects of carbon fertilization? What is specific about this study? I think one should also mention that an energy decomposition is used to disentangle the different effects on the surface energy budget.

2. The text in line 86ff leaves the impression that the author is the first to find biogeophysically induced global warming. But this is not the first study quantifying the climate change caused by the physiological effects of CO_2 on plants (see above). Results of these and similar studies should be reported in the introduction. Moreover, the literature references on the subject should be completed (two additional references are found below, but I guess one finds more if one follows the citations).

3. Completely missing is a comparison of the results of this study with those obtained in earlier studies on the same topic.

3. Names for the energy balance terms are not explicitly introduced and moreover those names are not consistently used. E.g. the term from latent heat is called "LH SEB term" in the text, "Latent Heat" in the titles of Fig. 3 and Supplementary Fig. 8, and "LH" in Supplementary tables 4 and 5. Because most of the paper centers around the discussion of these terms, I suggest that names for the different terms are explicitly introduced early in the paper, maybe already when the energy balance equation is introduced in section 2.2.

4. Today all data sets have a unique identification by a DOI. Such DOIs should be provided for all data sets used.

5. The four paragraphs in lines 473-529 are discussing exclusively material from the Supplement. In order that the main text is self-contained this material should either be included in the main text or that whole text passage should be moved to the Supplement.

Minor comments

- First sentence of abstract is inappropriate here: Here it is stated that CO_2 fertilization is a "significant source of uncertainty in future climate projections". This is indeed correct, but the sentence leaves the impression as if the study could contribute to a reduction of this uncertainty. But this is not the case, so that the study cannot be motivated in this way.
- Line 49: Preposition missing: "...associated <u>with</u> ..." or "...associated <u>to the</u> ...". But even better would be to name also the direction of causality, e.g. "...increasing atmospheric CO_2 concentrations <u>lead to</u> carbon fertilization ...".
- Line 64: I find it a bit misleading to denote the cooling induced by enhanced carbon storage as a "biogeochemical" effect. I would say that the cooling is caused physically (its a radiative effect) even though its induced biogeochemically.
- Line 85: What does "Climate effects include ... as well as the drivers" mean? What "drivers" are "climate effects"?
- Lines 137-138: Something is wrong with the grammar.
- Line 138: The word "including" is superfluous since all eight models are explicitly named.
- Lines 206-208: What means "not clear model differences"? It seems that the author expects that the dynamic vegetation modules have a particular impact on the changes

in LAI, or what is the reason to consider here in particular the two models with dynamic vegetation?

- Line 304: The wording "more efficient stomata" is a bit weird, more efficient are not the stomata but is the water usage.
- Lines 317-320 (similarly lines 344 and 488): I am not sure what exactly the reported "results ... across models" are. I can imagine that the author has analyzed the correlation between transpiration and vaporation for each model separately, but if so I had expected that the author reports a mean correlation value *plus* a range indicating the spread across the different models, but in the text one finds only single numbers for the global and tropical regions.
- Line 334: The remark that increases occur "also" over land is only understandable when consulting te respective figure in the Supplement: Indeed from that figure it is visible that significant changes are mostly found over oceanic regions, which explains the "also". But the main text should stand for its own without such a consultation.
- Lines 337-342: I cannot really follow this passage: What is called here the "former case" doesn't look for me like a "feedback" via the atmosphere but simply as a consequence of the changed temperature gradient due to the surface warming. And concerning the "latter case", the author seems to think that the mentioned "compensation" would be a process different from the mechanism described in the "former case", but I doubt that this can be justified by physics: "compensation" is not really a physical process but a consequence of the underlying processes, that surely obey energy conservation.
- Line 378-381: The formulation "In other words ... " is indicating that the subsequent claim that "the decrease in water vapor over land as dictated by the Clausius Clapeyron equation ... is muted by the decrease in latent heat flux" could be concluded from the foregoing remarks. But to do so one had in addition to know how strongly humidity changes per Kelvin in the simulations in order to compare with the Clausius Clapeyron prediction of 7%/K. But such a number is missing in the text.
- Line 420: What is the abbreviation "SOA" standing for?
- Lines 479-480: I do not see how from the spatial correlations between the total change in the energy balance and the individual LW_{clear} term it can be concluded that the changes in the LW_{clear} term is "largely a feedback" to the surface warming? I think that the strong correlations indicate only that the changes are a reaction to the warming. That a feedback on surface temperature is involved leading to mutual adjustment of temperature and the size of the LW_{clear} term seems plausible independent of the value of the correlations, but the correlations give no indication on the importance of the feedbacks in determining the size of the LW_{clear} term.
- Line 533: Typo: "biogeophysCIal".
- Lines 536-538: I think that this conclusion is wrong: From the mere fact that the 1%CO2 simulations from which TCR is determined contain the processes of the 1%CO2-bgc simulations from which the biogeophysical warming induced by CO_2 fertilization is derived, one cannot even expect that TCR and biogeophysical warming are positively correlated. Reason: First, the temperature rise in the 1%CO2 simulations is only slightly larger than in the radiatively coupled simulations 1%CO2-rad (where

biogeophysical warming from CO_2 fertilization is absent) (see e.g. Arora et al., 2020), meaning that TCR is mostly determined by the radiative effect of CO_2. But the biogeophysical warming from CO_2 fertilization arises from very different processes, mostly (as explained in the paper) from transpiration and albedo changes. So I do not see why a correlation between the two quantities should be "not necessarily unexpected". I think such a correlation is rather unexpected.

- Lines 538-539: In continuation of the previous comment: The remark that "the causes of larger TCR ... also operate in the 1PCTCO2-bgc runs" is only partially true, as the dominant effect determining TCR, namely the radiative CO_2 effect, is missing in the 1%CO2-bgc simulation. Hence, this remark also doesn't help to understanf why a positive correlation is found between TCR and biogeophysical warming from CO_2 ferilization.
- Lines 543-544: In continuation of the previous comment: The mere fact of a statistical correlation between TCR and biogeophysical warming from CO_2 ferilization (as exemplified by the mentioned three models) doesn't mean that this warming and TCR are "related" – where I suspect that "related" is meant here in a causal sense.
- Lines 544-546: In continuation of the previous comment: From the foregoing considerations based on correlations one can thus not conclude "the importance of the biogeophysical warming associated with carbon fertilization effect to intermodel variation of TCR". Nevertheless, I would surely agree that the considered biogeophysical warming contributes to TCR (and its intermodel variation as discussed in lines 547-555). But this can be concluded without reference to the diagnosed correlations (which, having value 0.55 (see line 531), is anyway rather weak). Hence, because the dominant mechanisms determining the considered biogeophysical warming and TCR are different, I think that this correlation is incidental, even though it is reported to be significant. Accordingly, in my opinion, this positive correlation should at most be mentioned as a curiosity.
- Lines 600-603: As explaind above, from the diagnosed statistical correlation one cannot conclude that "the causes of intermodel TCR variation ... are also responsible for some of the intermodel spread in the biogeophysical temperature response under carbon fertilization."
- Supplement Line 390: Typo: correLAtions instead of correALtions.
- All figures showing spatial distributions: As is often done, the author sets a dot into grid cells where the result is significant. Thereby the color impression gets for significant grid cells darker and a comparison with the color in the color bar gets biased. A better practice would be to set dots where results are not significant and a comparison with the color bar is anyway futile.

References

[1] Boucher, O., Jones, A., & Betts, R. A. (2009). Climate response to the physiological impact of carbon dioxide on plants in the Met Office Unified Model HadCM3. Climate

Dynamics, 32, 237-249.

[2] Cao, L., Bala, G., Caldeira, K., Nemani, R., & Ban-Weiss, G. (2009). Climate response to physiological forcing of carbon dioxide simulated by the coupled Community Atmosphere Model (CAM3.1) and Community Land Model (CLM3.0). Geophysical Research Letters, 36(10).