Review on

Christian Beer, “Carbon-climate feedback higher when assuming Michaelis-Menten kinetics of respiration”

submitted to Earth System Dynamics

August 8, 2024

It is well known that even at the conceptual level there are large uncertainties in the representation of the global carbon cycle and its coupling with climate (Canadell et al., 2023, section 5.7, p. 769)[1]. To improve the reliabilty of coupled climate-carbon simulations, in particular the modelling of soil respiration is an active field of research [8, 7]. The study reviewed here contributes to this field by performing scenario simulations with a zero dimensional climate-carbon model (Lade et al., 2018) to investigate the effect of a modified representation of soil respiration on the future land carbon sink, the size of the carbon-climate feedback, and the “remaining carbon budget”, i.e. the amount of future anthropogenic emissions that will not compromise the 2 degree climate warming target. Here the original representation of soil respiration by a first order kinetics (FOK) is replaced by a Michaelis-Menten (MMK) type representation.

I agree to reviewer #1 that, qualitatively, the results from this study are rather foreseeable: Because the MMK model limits soil respiration when the land carbon storage grows beyond a certain amount, this model enhances the land carbon sink, whereby the remaining carbon budget gets larger, the land contribution to the carbon-concentration feedback gets more positive (there is more carbon stored per ppm CO_2 rise; β sensitivity), and the land contribution to the climate-carbon feedback gets more negative (there is more soil carbon to be respired per degree climate change; γ sensitivity). So the main advancement by this study would be its quantitative results (that are partly noted in the abstract). But I doubt that those numbers have any relevance (major comment 1 below) and, moreover, I don’t trust these numbers (major comment 2 below). In addition, as also noted by reviewer #1, the feedback analysis is rather unclear (major comment 3 below), and – if my reconstruction of its meaning is right – the feedback metrics used are not simply a measure of the carbon-climate feedback (as intended by the author; see title) but are mixing information on this and the carbon-concentration feedback.

Major comments

1. I fear that the quantitative results of this study are, if at all, only of academic interest. By the Michaelis-Menten type formula used by the author, soil respiration starts to saturate at large land carbon storage. The author motivates the usage of this model because “the underlying biochemical reactions are mostly enzymatic” (line 57), a rather incomplete justification. In fact, such a saturation appears because at large carbon storage also huge amounts of nutrients are bound in the organic soil molecules that are thus not available for the growth and maintenance of the microbials that mineralize them. These mineralized nutrients are at the same time necessary for the plants binding those nutrients to flourish. Accordingly, the growth of plants and the respiration of soils is

[1] Only references additional to those in the reviewed paper are listed below.

closely linked (see e.g. [9, chapter 10], [4]) so that the input to the land carbon stocks by photosynthesis (net primary productivity) is via the nutrient connex highly correlated with the loss of land carbon by soil respiration. This link, necessary to obtain realistic estimates of the feedbacks and remaining carbon budgets, is missing in the study reviewed here because only limitation to soil respiration is considered, but not a parallel limitation by plant productivity.

Accounting for this link leads to very different results: To account for nutrient limitation, the Earth system modelling community recently put a lot of effort in accounting for at least one such nutrient by complementing the carbon cycle dynamics with a description of the nitrogen cycle: 6 out of 11 Earth system models (ESMs) participating in CMIP6 were equipped with a nitrogen cycle (Canadell et al., 2023, p. 730). In a coordinated community effort, for the whole model ensemble the feedbacks (Canadell et al., 2023, section 5.4.5.5, p. 735) and remaining carbon budget (Canadell et al., 2023, section 5.5, p. 742) were analyzed, including a separate analysis of the effect of nitrogen limitation on the feedbacks (Arora et al., 2020, Fig. 5). It turns out that tendentially for the land component both the carbon-climate feedback (γ) and the carbon-concentration feedback (β) are reduced by accounting for nitrogen imitation, just the opposite of the study reviewed here (see also [10]). – Concerning the remaining carbon budget I just realized that a more comprehensive analysis of the effect of nutrient limitation in a particular ESM is currently under review in Biogeosciences [6].

2. Even if one ignores the previous comment, and insists to study the effect of limitation of soil respiration alone, I doubt that the simulation results can be trusted. Firstly, the author has without explanation modified the employed model by Lade et al. (2018) in a – as I think – unreasonable way: It belongs to basic understanding of the land carbon cycle that its long term dynamics is determined by the allocation into long-term stable organic componds, i.e. by net primary production (NPP), instead of gross primary production (GPP) that includes allocation to sugars that are fastly respired (see e.g. [5]). Accordingly, in the original model by Lade et al. (2018), NPP appears as input to the land carbon cycle (Lade et al., 2018, Eq. (2), but in the reviewed study the author has replaced it by GPP (Eq. (3), line 101). This replacement would be OK if in addition the model equation had been complemented by a term for carbon loss by autotrophic respiration, but this is not the case. Hence the carbon inputs to the model are overestimated by a factor two (GPP_0=113 PgC/a (table 1, line 144) vs. NPP_0=55 PgC/a (Lade et al., 2018, table 1). Together with the modified value for the pre-industrial land carbon, this modification results in a characteristic land carbon turnover time of $C_{L,0}/GPP_0 \approx 20$ years, while the respective turnover time in the original setup of Lade et al. is $C_{L,0}/NPP_0 \approx 34$ years – this is a serious modification of the dynamics of the model affecting how the carbon cycle reacts to the scenario forcing in the simulations, modifying in particular the distribution of carbon between the three compartments (land, ocean, atmosphere) in the scenario simulations and therefore also affecting the size of the feedbacks.

And secondly, the re-tuning of the model for the MMK model variant (the case with limitation of soil respitation) is rather unclear: The author notes that the parameter k of the FOK soil respiration term is obtained "following the same principle as in (Lade et al., 2018)" by setting pre-industrial soil respiration equal to pre-industrial plant productivity (lines 99-100) (but using GPP instead of NPP; see previous remark). While this is an adaptation to *pre-industrial* conditions, lateron it is remarked that "for the first-order kinetics approach (FOK)" (line 246) "model parameters [i.e. k] have been chosen to fit *historical* carbon and temperature changes ..." (line 245; emphasizing by me) – I guess the latter is wrong. But more importantly: Concerning the two parameters ν_{max} and K_M of the MMK soil respiration term it is remarked that they "are set such that MMK model results match FOK model results for the *pre-industrial* period" (lines 141-142; emphasizing by me). If this means that one sets following (Lade et al., 2018) as in the FOK case pre-industrial soil respiration equal to pre-industrial plant productivity, one had only one condition for two parameters, so that the problem to find ν_{max} and K_M is under-determined, meaning that the author's choice of them contains a subjective decision. Hence a clear explanation how the author comes to values of ν_{max} and K_M is missing. Particularly the value of K_M is crucial for the whole study as it determines at what value of the land carbon stock the limitation of soil respiration sets in, so that all results of the study depend critically on the choice of this value. I personally doubt that even if one uses besides pre-industrial also the histoical data, the values of the two parameters are sufficiently constrained to come to quantitatively reliable simulation results. This point of parameter estimation is so important for this study, that it needs a clear description of how it is done and a separate analysis how uncertainties in parameter estimation affect the study results.

3. Already reviewer #1 has tried to get clearer about the presented feedback analysis. If I understand it correctly, the feedback analysis is performed as follows. For each model variant (FOK, MMK) a simulation is performed with the fully coupled model (simulation 'on') and another one where climate change (represented by temperature change) is not affecting the soil respiration (simulation 'off'), realized by setting $Q_{10} = 1$. According to line 127 and the captions of tables 1 and 2 the feedback analysis is based on changes in *atmospheric* carbon stocks, which is a bit surprising as the the direct effect of the modified soil respiration is on the *land* carbon stock (as naturally assumed by reviewer #1). Calling the changes in atmospheric carbon in the two simulations by ΔC_A^{on} and ΔC_A^{off}, the strength of the feedback is then quantified in two ways, namely, as explained in line 127, by calculating the difference $D := \Delta C_A^{on} - \Delta C_A^{off}$ (listed in table 2) and by calculating the quotient $F := \Delta C_A^{on} / \Delta C_A^{off}$ (listed in table 3), called, following [2] and (Zickfeld et al., 2011), "feedback factor" in the reviewed study. In the 'off' simulation it is the land carbon-climate feedback that is switched off so that ΔC_A^{off} is the land contribution from the carbon-concentration feedback, meaning that D quantifies the effect from the carbon-climate feedback happening *on top of the carbon-concentration feedback.* And F is the factor by which carbon-climate feedback *boosts the carbon-concentration feedback.* Hence both metrics depend on the size of the carbon-concentration feedback as a reference and thus they are not a quantification of the carbon-climate feedback alone, as claimed by the author (title, abstract, captions of tables 1 and 2, conclusions). In addition, this reference is different for the two simulated model variants (FOK, MOK) so that the comparability of in particular the dimensional feedback metric D is questionable (F is non-dimensional).

In summary, if my interpretation is correct, the feedback metrics used are not (as intended by the author) independent measures of the carbon-climate feedback, but are of more complex nature. As already remarked by reviewer #1, this feedback could be more directly measured by the commonly used γ sensitivity (see e.g. (Arora et al., 2020)). And because the soil respiration affects the carbon-concentration feedback much more directly than the carbon-climate feedback, it would be natural to quantify it as well, which could be easily done by calculation of the commonly used β sensitivity (see e.g. (Arora et al., 2020)), which may be immediately obtained from the 'off' simulation.

Minor comments

- Title, abstract, and throughout: Whenever the author talks of respiration, *heterotrophic* respiration is meant (also called "soil respiration"), in contrast to autotrophic rspiration. This should be made clear in the title, the abstract, and elsewhere.
- The Lade et al. model is missing the temperature dependence of photosynthetic production. I don't think that a quantification of the carbon-climate feedback without that temperature dependence makes sense. At least it should be made clear that the study accounts (if at all) only for part of that feedback.
- Line 9 (abstract): The formulation "The epistemic uncertainty ... is unclear" a bit weird: I guess the author wants to express that because of epistemic uncertainty there is quantitative uncertainty whose size is unclear – hence it would be the quantitative uncertainty being unclear, not the epistemic uncertainty.
- Lines 13-15 (abstract): This formulation is misleading: taking it literally, the author suggests to improve our understanding of the model structure of Earth system models. But I guess the author wants to emphasize that we need to improve our understanding of heterotrophic respiration to improve its representation in Earth system models.
- Line 28: Why "in contrast"? In contrast to what?
- Line 35 (caption of Fig. 1) and line 39: Only the negative feedback may be termed "biogeochemical", the positive feedback is non-biological and mostly determined by physics (radiation in the atmosphere, temperature dependenc of reaction rates); for terminolgy see e.g. [3].
- Line 50: A better reference than (Zickfeld et al., 2011) on the methodology of separating climate-carbon feedbacks would be the original study [2] or the recent review [3].
- Line 85: Better: "atmospheric CO_2 concentration" instead of "atmospheric carbon content".
- Line 106: Should this read "1850" instaed of "1750"? If not: explain how the model is forced between 1750 an 1850.
- Line 117 (Fig. 2): Use "PgC/a" for the units noted in the ordinate labelling (as in

Fig. 3).

- Line 123: Concerning the methodology for determining climate-carbon feedbacks you cite (Zickfeld et al., 2011). Please give credit also to the inventors of this methodology [1, 2].
- Line 147 and Fig. 3: You use the term “changes” with two different meanings: Your “stock changes” are rates of stock change (fluxes), while your “temperature change” refers to a difference. It would be more clear to make this transparent in the text and Figure (as you did in line 164).
- Lines 157-160 (caption Fig. 3): I guess the solid lines show the results for the FOK model, but this is not noted.
- Lines 256 and 282: I do not see how the value of 35% – mentioned as key result of the study in the abstract – follows from the values in table 3. I guess its some kind of mean value, which would not make sense (see next comment).
- Line 234 (Fig. 5): This figure doubles the information from table 3. Moreover, it is inappropriate to show these data as box plots: Box plots are meant to display major characteristics of a statistical distribution, for which it makes sense to calculate mean value and percentiles. But the four scenarios, whose data are listed in table 3, do not form a statistical ensemble so that the results from the scenario simulations are not realizations of a random process which could be characterized by a statistical distribution.

References

[1] Cox, Peter M., et al., *Acceleration of global warming due to carbon-cycle feedbacks in a coupled climate model*, Nature 408 (2000) 184-187.

[2] Friedlingstein, P., et al., *How positive is the feedback between climate change and the carbon cycle?*, Tellus B55 (2003) 692-700.

[3] Heinze, Christoph, et al., *ESD Reviews: Climate feedbacks in the Earth system and prospects for their evaluation*, Earth System Dynamics 10 (2019) 379-452.

[4] Hungate, Bruce A., et al., *Nitrogen and climate change*, Science 302 (2003) 1512-1513.

[5] Prentice, I. Colin, et al., *Dynamic global vegetation modeling: quantifying terrestrial ecosystem responses to large-scale environmental change*. In: J.G. Canadell, D.E. Pataki, and L.F. Pitelka (Eds.), *Terrestrial ecosystems in a changing world*, (Springer, 2007), 175-192.

[6] De Sisto, Makcim L., and Andrew H. MacDougall, *Effect of terrestrial nutrient limitation on the estimation of the remaining carbon budget*, Biogeosciences Discussions 2023 (2023) 1-30.

[7] Sulman, Benjamin N., et al., *Multiple models and experiments underscore large uncertainty in soil carbon dynamics*, Biogeochemistry 141 (2018) 109-123.

[8] Todd-Brown, Katherine E.O., et al., *A framework for representing microbial decomposition in coupled climate models*, Biogeochemistry 109 (2012) 19-33.

[9] White, R.E., *Principles and Practice of Soil Science – The soil as a Natural Resource* (Blackwell, Malden, 4^{th} ed., 2006)

[10] Zaehle, Sönke, and Daniela Dalmonech, *Carbon–nitrogen interactions on land at global scales: current understanding in modelling climate biosphere feedbacks*, Current Opinion in Environmental Sustainability 3 (2011) 311-320.