Biogeosciences Discuss., community comment CC1
https://doi.org/10.5194/bg-2022-168-CC1, 2022

Comment on bg-2022-168

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Community comment on "Quantifying land carbon cycle feedbacks under negative CO_2 emissions" by V. Rachel Chimuka et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-168-CC1, 2022

The authors explore carbon cycle feedbacks under an idealized 1%CO2-CDR overshoot scenario using an intermediate complexity model UVic ESCM and introduce a novel approach that uses zero emissions simulations to reduce the climate system inertia when quantifying feedback parameters during the ramp-down period.

I and other co-authors of a closely-related study (Melnikova et al., 2021, hereafter M21) would like to draw the authors' attention to our study as it may have been overlooked when the authors say:

L85: "Our study complements the only existing study on ocean carbon cycle feedbacks under negative emissions (Schwinger & Tjiputra, 2018) by exploring the behaviour of these feedbacks on land."

It would be interesting to see a comparison of the analysis of the carbon cycle feedbacks under the idealized 1%CO2-CDR scenario with SSP5-3.4-OS scenario, and I would be pleased to provide the data if the authors are interested.

Particularly, in M21 (section "4.2. The Peaks of Land and Ocean Carbon Uptakes"), we discuss the balance between GPP and TER that could be useful for the proposed analysis by the authors on balance between NPP and soil respiration.

Most importantly, the conclusions of this new study sound somewhat opposite to the conclusions of M21 where we stated that: "The carbon cycle feedback parameters **amplify** after the CO_2 concentration and temperature peaks … so that land and ocean absorb more carbon per unit change in the atmospheric CO_2 change (stronger negative feedback) and lose more carbon per unit temperature change (stronger positive feedback) compared to if the feedbacks stayed unchanged". In contrast, the study by Chimuka et al. concludes on a "***reduced*** *carbon loss due to the concentration-carbon feedback and reduced carbon gain due to the climate-carbon feedback."*

I am curious about what drove the discrepancy in the conclusions and encourage the authors to add some discussion that could be useful for the scientific community and could prevent any confusion about the conclusions.

While I am not sure for the reasons that drove the discrepancy, I speculate it could be (i) the methodology used to calculate the feedback parameters (i.e., in this study, "Feedbacks under negative emissions are computed at the return to preindustrial levels (end of ramp-down phase) using changes in carbon pools, atmospheric CO2 concentration, and surface air temperature computed relative to the time of peak atmospheric CO2", while M21 computed them relative to piControl). In fact, for our M21 analysis we considered using (1) piControl, (2) time of CO_2 and temperature peaks, and (3) "new equilibrium state" at the end of the simulation. However, we chose (1) because using (2) in the more "realistic" SSP scenario would result in too small values of ΔCO_2 and temperature during most part of the ramp-down phase, resulting in ill-defined quantities. Besides, UVic ESCM shows no lag between the peaks of CO_2 concentration and global surface temperature but it is not the case in some of the more complex models (e.g., Boucher et al., 2012, shows a lag of temperature peak over the ocean; in M21, the lag of temperature peak is up to 30 years, depending on the ESM). The discrepancy in conclusions of the two studies could also be due to (ii) the proposed method to remove the impact of climate inertia by using additional zero-emission simulations. Finally, (iii) the discrepancy could root in the difference between the idealized 1%CO2-CDR and SSP5-3.4-OS scenarios (e.g., due to scenario dependency of feedback parameters). I suggest adding discussion on this matter, especially in terms of the implications of translating the conclusions from the idealized scenarios to the more socially-relevant ones.

Other comments are :

L341: "Surface air temperature remains relatively constant in the BGC mode. In the FULL mode, the land switches into a source of carbon after missions cease, consistent with the behaviour of the UVic ESCM in the Zero Emissions Commitment Model Intercomparison Project (ZECMIP)"

Yes, but there is a variety of responses among models in ZECMIP. The UVic's behavior in ZECMIP is somewhat different from the majority of models (see figures 2.d and 3.a of MacDougall et al 2020). Could some discussion be added?

Also, we would appreciate seeing a comparison of the 'standard' ☐ and γ (under 1%CO2 experiments) by UVic to the CMIP6 ensemble in a table or figure to get a better idea of where this version of UVic stands.

L426: "Models without a nitrogen cycle exhibit greater land carbon gain under positive emissions relative to other CMIP5 and CMIP6 models, that is, the concentration-carbon feedback parameter is more positive (Table S2). They also exhibit greater carbon loss under positive emissions, that is, the climate-carbon feedback parameter is more negative."

I am concerned that the authors ignore that the climate-carbon feedback may be both positive (i.e., amplifying climate change) and negative in the colder regions.

L12: "This study investigates land carbon cycle feedbacks under positive and negative CO2 emissions using an Earth system model"

The fact that UVic is not an ESM but EMIC should be made clear throughout the manuscript.

I hope these comments are useful.

Irina Melnikova, with the inputs of co-authors of Melnikova et al. (2021)

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