Review of “Asymmetry in carbon cycle feedbacks and transient climate response under positive and negative CO_2 emissions” by Chimuka and Zickfeld.

This is a clear and well written manuscript that explores the symmetry of response of the carbon cycle under positive or negative emissions of CO_2. This is an important question, and one of increasing interest as both science and policy are required to think more about aspects of overshoot and reversibility. Much more is known about the climate system in a steadily warming world than one where CO_2 removal leads to cooling. This manuscript contributes to a research agenda to better address this issue.

The paper uses the UVic earth system model of intermediate complexity. This is a model widely used in the literature and suited to exploring this question. While the results from this model are interesting, and the analysis sheds light on processes involved, I find the value of the paper is more in terms of testing an experimental design which could be adopted more widely by other models. We know that such responses are model-dependent and even the sign of behaviour of terms such as ZEC can vary from model to model. So it is not so much that these results are definitive, but that they lead the way for other models to follow and for a wider research initiative into symmetry and reversibility.

We thank the reviewer for the positive and constructive comments.

I have some minor comments which I list below which I hope the authors find useful to clarify some points.

I do, though, have a major comment about the design and intent of the paper. The more I think about this question, the more I realise that there is a difference between “symmetry” and “reversibility”. It may feel nuanced, but I believe it is important.

The difference is that “symmetry” - as defined in this study - is asking the question “do we see the same response if we go upwards from a starting point or downwards from that same point?”, whereas “reversibility” would ask “do we see the same response going up from one point to another as we do going back down from the second point back to the first?”

Previous studies of reversibility (e.g. Boucher et al 2012, or CDRMIP simulations) have addressed the question of “going back down again”, BUT they have been contaminated by the problem that they have very large legacy effects on the way down because the simulation follows immediately from very strong upwards forcing and is not in steady state. The current authors know this well and discuss it in detail in Chimuka et al (2023). The present study therefore represents a novel experiment design and analysis by equilibrating at a higher level (here 2xCO_2), thus avoiding the legacy response in the negative emissions phase. I think this is vital and a very nice experimental design.

However, if the question of interest is whether or not we can recover a previous climate state by the same path in reverse (i.e. does TCRR = TCRE?) then I am not sure that the current experiment answers exactly that question. The asymmetry found in this paper may be because the system response differs above 2xCO2 from below. For example, if TCRE is state-dependent (e.g. lets assume the TCRE gradient gets less steep at higher warming), then we would expect TCRE measured above 2xCO2 to be lower than TCRE measured up to 2xCO2. This would manifest as an asymmetry about 2xCO2 – that's true. But it does not necessarily imply that reversibility is along different lines. TCRR and TCRE could still be identical to each other. Does that make sense?

A more direct answer to the reversibility question would be to compare your ramp-down experiments here with the ramp-up from 1xCO2 up to 2xCO2. So you can measure a TCRE going from 1xCO2 up to 2xCO2, and then a TCRR going back down (but from an equilibrated state at 2xCO2). I think this then answers the reversibility question.

I think you must have such experiments, but I realise that requesting them (and new analysis) to be added here is a big task. As the manuscript stands it does indeed answer the stated question regarding "symmetry". But my challenge to the authors is whether or not that is really the target question? Given that work like this will undoubtedly be influential in experimental design for CMIP (CMIP7 and beyond), then making sure that we address the correct questions is as important as making sure we design the experiments to answer them. One option is to keep the present manuscript and be clear that the question asked is on symmetry but does not necessarily reflect reversibility. This opens up the opportunity for a follow-on paper on reversibility. Or a second option would be to add to the current study an analysis of positive vs negative emissions up vs down between the same points.

I appreciate a request for extra analysis is never welcome, but I hope the authors see this a constructive suggestion.

Chris Jones

We thank the reviewer for these important comments on the distinction between symmetry and reversibility. We have included a few sentences in the introduction to clarify the difference. As the reviewer correctly points out, our experimental design is best suited for answering the question on symmetry. By diagnosing positive and negative emissions from the same equilibrium state, we investigate whether the climate impact of a given amount of emissions can be balanced by an equivalent amount of removals.

With that said, we do see the policy relevance of exploring the reversibility question by comparing our ramp-down experiments to a 1% yr^{-1} ramp-up from preindustrial to 2xCO_2 and had already conducted the required model simulations. Therefore, we have included an additional ***Section 3.4: Quantifying Carbon Cycle Feedback and TCRE Asymmetry under Positive and Negative Emissions Applied from Different Equilibrium States*** *with two additional experimental designs based on suggestions by the reviewer. Overall, we find that the magnitude of carbon cycle feedback asymmetry is much smaller (by an order of magnitude) in* *this experimental design, except for the ocean, where the magnitude of asymmetry more than doubles. The smaller magnitude of asymmetry arises because temperature change is more symmetric and the range of CO_2 concentrations in both trajectories is the same. The sign of the TCRE asymmetry reverses and the sign of the carbon cycle feedback asymmetry is positive for land feedbacks and negative for ocean feedbacks.*

Minor comments/suggestions:

- Have you thought about pitching the experiments in emissions rather than concentration forcing? I have been a proponent of using concentration-driven runs to diagnose the alpha/beta/gamma feedback metrics because they are in units of "per ppm" so prescribing concentration as the boundary condition makes sense. But for metrics like TCRE/TCRR which are "per GtC of emission", then prescribing emissions makes sense as a boundary condition. Then it is relatively easy to prescribe simulations with symmetric positive/negative emissions. The consequence here of imposing symmetric % change in concentration is very unsymmetric emissions rates (your fig 1b).

  This is something we considered in the course of our study. We opted to use concentration-driven simulations so that we could quantify feedbacks using the integrated flux-based feedback framework and maintain consistency with the feedback framework, ensuring comparability between the Jones & Friedlingstein and the direct approach to quantify TCRE. However, one thing we neglected to mention in the manuscript is the time points at which we evaluated TCRE/TCRR. Unlike the feedbacks parameters, which were evaluated at $3xCO_2$ and $1xCO_2$ (yr 43) for both positive and negative emissions simulations, we chose to compare TCRE vs TCRR at the same level of diagnosed emissions for both the 1% yr^{-1} and flat10 simulations.

  For example, in the positive emissions simulations, diagnosed cumulative emissions are approximately 800 PgC, whereas cumulative diagnosed negative emissions are -970 PgC in the negative emissions simulation. Therefore, we chose to evaluate TCRE at 800 PgC from the positive emissions simulation, and TCRR at -800 PgC from the negative emissions simulation. In addition, we also evaluated TCRE and TCRR at +/-800 PgC from the flat10 simulations for consistency in the rate-dependence analysis.

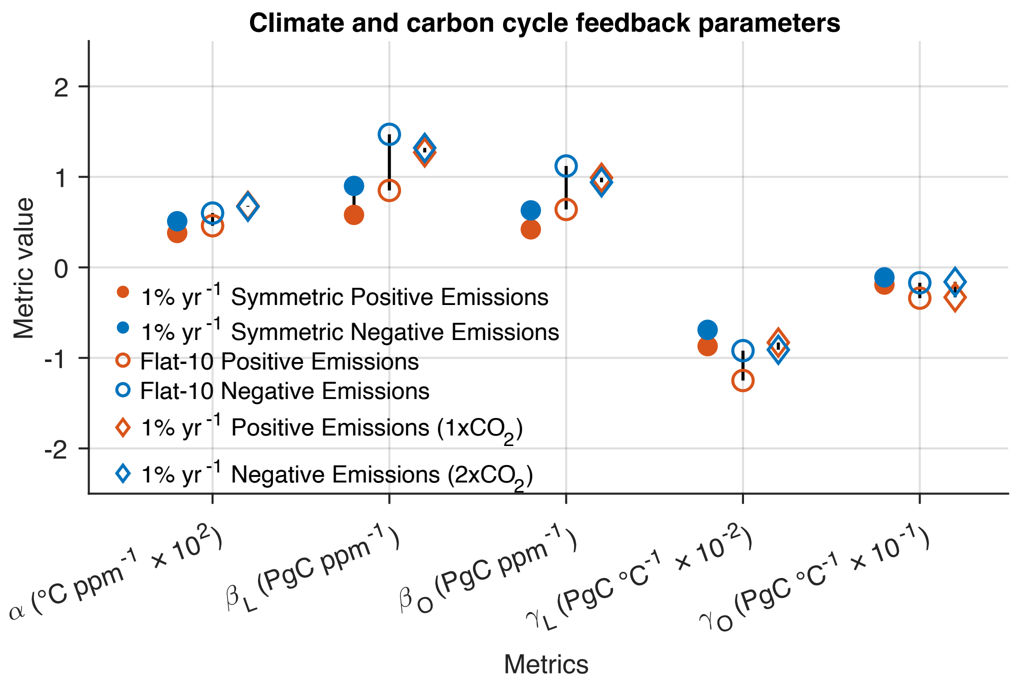
- The fact that it only needs 43 years to reach 3xCO2 feels very short – are you comfortable this is long enough for a reliable signal? Potentially should we look at lower rates and longer runs? If this experiment was adopted for example in ESMs with significant internal variability would this be long enough to robustly diagnose T and carbon fluxes?

  We agree that 43 years is a relatively short time period, especially for ESMs. This was partly the motivation for additionally exploring feedback and TCRE asymmetry from the flat10 simulations as those metrics are evaluated after 100 years. Based on our rate-

dependence analysis, the sign of the feedback and TCRE is independent of rate of emissions (and therefore, time of model integration), and the magnitude exhibits little to no dependence. We do, however, agree that simulations with a longer time of integration, such as the flat10 simulations, would be better suited for multi-model analyses.

- Figure 5 is useful – can you include alpha on here too?

  Figure 5 has now been updated to include alpha. We have also added results from the new Section 3.4 as well. Please see the updated figure below:



- Sec 3.3 on quantifying the asymmetry and using Jones & Friedlingstein framework. I think you could be clearer here that the J&F framework is simply a way to break open TCRE to see which components matter – but yes it might be approximate. The delta-T/CE and delta-T/CR remain the correct answer and the actual definition of TCRE and TCRR (you say "conventional calculation" – I would make this stronger and say "known values" or similar). Just to be clear that J&F isn't a rival definition, just a means of understanding

  Noted. We have changed the wording to "direct calculation".

- Fig 6 – instead of plotting these vs time, I wonder if plotting vs Emissions is more relevant? We know the diagnosed emissions are not symmetric so we wouldn't expect the T vs time to be the same even if TCRE=TCRR. So maybe plotting in emissions space here makes more sense

  We thank the reviewer for their comment. As mentioned in a reply to a previous comment, we plotted TCRE and TCRR against time, but evaluated the metrics at the same level of *cumulative diagnosed emissions. We also chose to plot the remaining metrics against time because they track CO_2 concentrations (which are symmetric) rather than cumulative emissions (which are asymmetric).*

Citation: https://doi.org/10.5194/egusphere-2025-6405-RC1

Review of "Asymmetry in carbon cycle feedbacks and transient climate response under positive and negative CO2 emissions"

Overall assessment:

The study devises a novel experiment design to assess the asymmetry in positive and negative TCRE. This is done by spinning-up an Earth System Model (ESM) at 2X pre-industrial CO2 concentration and carrying out positive and negative emissions experiments from climate equilibrium. Biogeochemically and radiatively coupled CO2 experiments are conducted to decompose the components of the carbon cycle feedbacks. The paper finds that there are substantial asymmetries in positive and negative TCRE driven by complex Earth system responses. Overall the paper is well thought out, thorough and a valuable contribution to negative emissions research. I recommend that the paper undergo minor revisions.

We thank the reviewer for their positive comments.

General Comments:

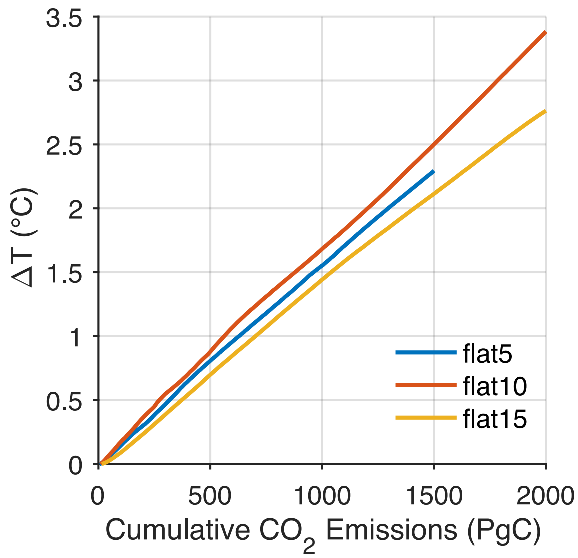
(1) While the need for a new experiment design to quantify asymmetry in TCRE is clearly explained in the introduction, you do not give a clear rationale specifically for your new experimental design. Why is starting simulations from 2X CO2 a good idea? Why not start at pre-industrial conditions?

While dropping down to zero CO2 concentration would clearly cause catastrophic strain on any ESMs numerics, you need to explain to the reader why this is a bad experiment design, and why this justifies starting from 2X CO2.

We chose not to initialize simulations from preindustrial because the symmetric negative emissions simulation would span a range of CO_2 concentration of 0 – 280ppm, with most of those CO_2 concentrations below policy relevant levels and could cause instabilities in numerical simulations. Furthermore, this choice allows for comparability to previous work (experimental design from Chimuka et al., 2023). In particular, the symmetric negative emissions simulation from $2xCO_2$ to $1xCO_2$ here, which represents the response to negative emissions alone, can then be directly compared to the declining limb of the CDR-reversibility experiment (from $2xCO_2$ to $1xCO_2$), which represents the response to both prior positive emissions and the subsequent negative emissions. An expanded discussion sections details results from this comparison. We plan to provide a stronger justification for the experimental design in the revised manuscript.

(2) You need to establish that TCRE exists in 2X CO2 world (I highly suspect it does). To do this simple conducted flat-5, flat-10 and flat-15 experiments from the 2XCO2 equilibrium state to 2000 PgC, and make sure that the cumulative emissions vs. temperature curve is linear and path-independent. You can place the figure in the supplementary and add a sentence or two in the main text saying that you checked this.

We thank the reviewer for this note. We can confirm that the TCRE relationship is linear and shows little path-dependence for the 2xCO_2 state. The requested figure is included below and will be added to the supplement.



Specific comments:

Line 25: Change to either 'year 2100' or '2100 CE'

Corrected.

Line 112: 'several millennia' be specific, give actual numbers.

We have made this more specific (6000 years).

Line 124: Explanation here can be improved. It is unclear what 'symmetrically' means in this context.

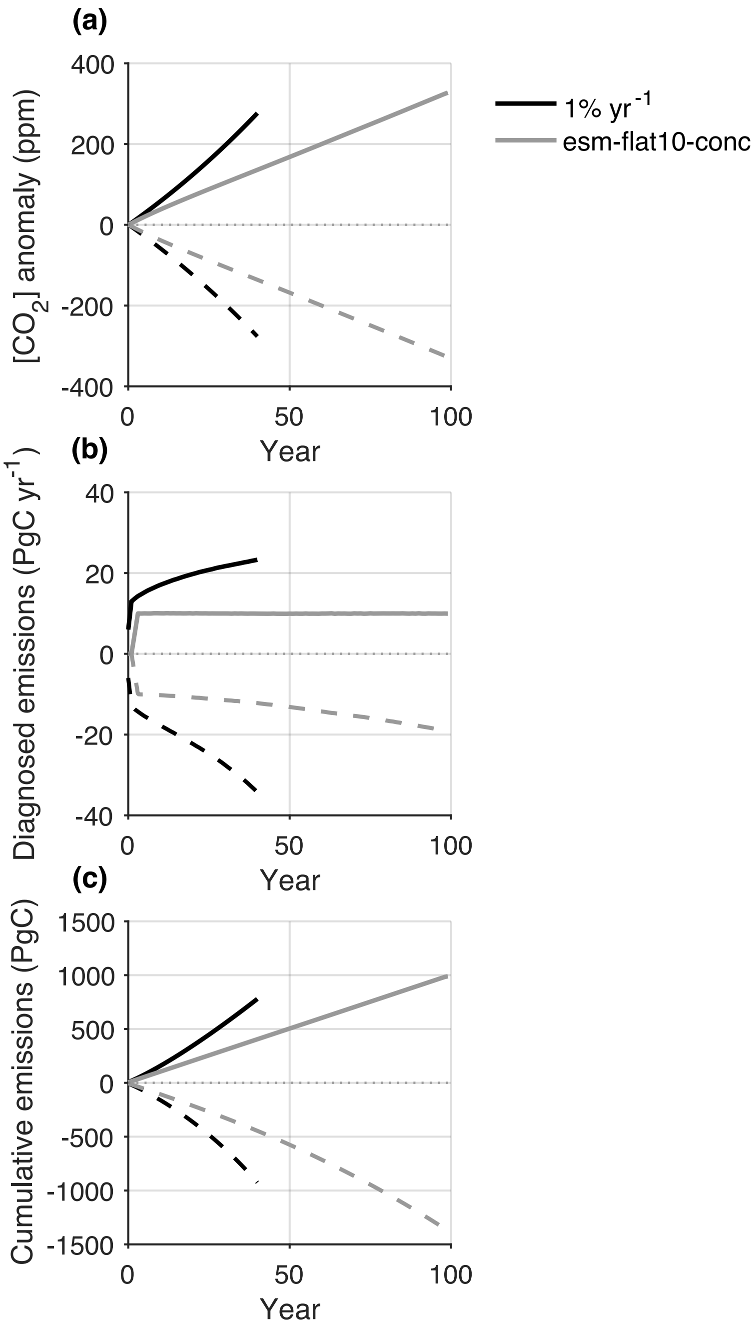
We have removed this word in this line, and kept in in line 127, where we refer to the symmetric CO_2 trajectories.

Line 130: Change 'real world' to 'natural world'

We have corrected this wording everywhere in the manuscript.

Figure 1b: What are there tails on the flat-10 experiment?

We noticed that we had the axis shifted forward by a year. The figure has now been corrected and included below. We thank the reviewer for pointing this out.



Line 183: Should 'modes' be 'models'? If not a typo please explain more.

Modes here refers to the three fully coupled, biogeochemically coupled and radiatively coupled modes; the last two modes are important for disentangling carbon cycle feedbacks. We will clarify this in the manuscript.

Line 260: Please write our 'positive emissions' and 'negative emissions'. 'PE' and 'NE' are not improving the readability of the paper.

We have changed this everywhere in the manuscript.

Line 261: Change 'logarithmic' to 'approximately logarithmic'

Corrected.

Figure 4: Please write out all of the abbreviations in the figure legend.

We have updated the figure legend accordingly and included it below.

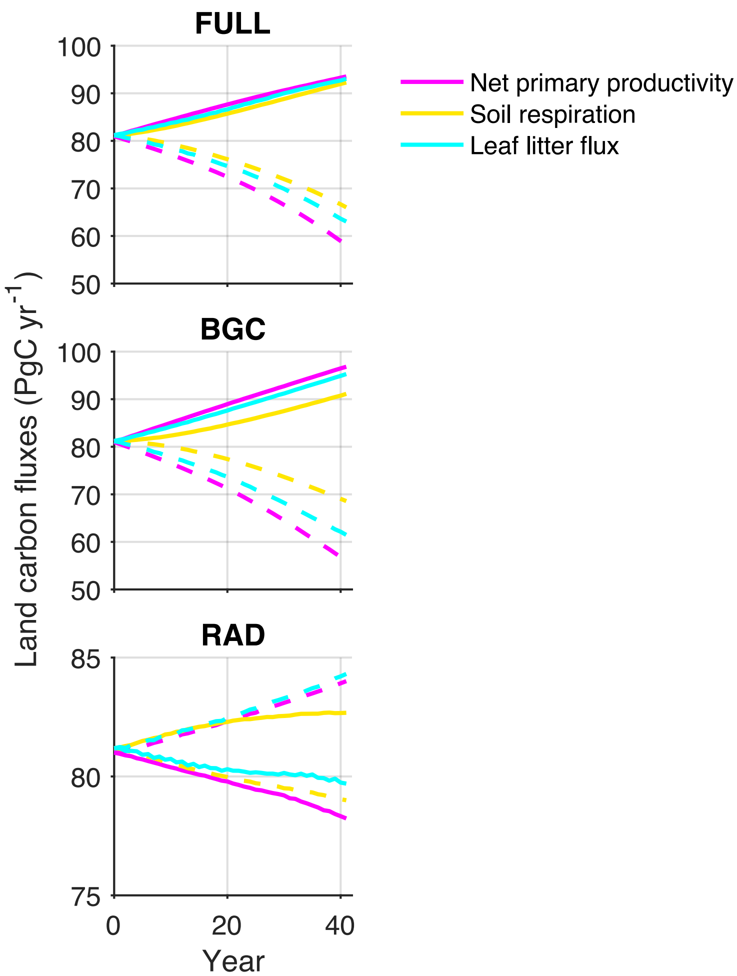


Figure 5: I would suggest using Kelvin units in place of ºC. Have C for carbon and ºC in the same units can lead to confusion.

Figure 6a: Again Kelvin would be preferable. 'TCRE/TCRR' could be written 'TCRE or TCRR' to avoid readers interpreting the former as a division operation.

We thank the reviewer for these comments. We chose to keep ºC for consistency with the original feedback framework and past carbon cycle and TCRE research studies. We have, however, clarified the meaning of the units in case of confusion in the captions for Figures 5 - 7.

We have also replaced all instances of TCRE/TCRR with TCRE or TCRR.

Line 490: Write out RF and AF. Many readers will read RF as 'radiative forcing'

We have corrected this wording everywhere in the manuscript.

Line 496: Change 'real world' to 'natural word'

We have corrected this wording everywhere in the manuscript.

Line 503: Change 'real world' to 'present day' or 'pre-industrial world'. Many paleoclimate states have been close to 2XCO2.

Corrected.

Line 518 to 520: There are versions of the UVic ESCM with fully coupled nitrogen cycle (De Sisto et al. 2024). This should be mentioned here.

Corrected. We thank the reviewer for this note.

References:

De Sisto, M.L., C. Somes, A. Landolfi, and A. H. MacDougall, 2024: Projecting atmospheric N2O rise until the end of the 21st century: an Earth System Model study, Environmental Research Letters, (12), 124036

Citation: https://doi.org/10.5194/egusphere-2025-6405-RC2

This is a review of the manuscript entitled “Asymmetry in carbon cycle feedbacks and transient climate response under positive and negative CO2 emissions” by Rachel Chimuka and Kirsten Zickfeld. The authors aim to quantify asymmetries in the land and ocean carbon feedbacks and TCRE/TCRR, and test their initial state and rate dependency. They firstly look at the symmetry in response to positive emissions on a 1pctCO2 trajectory from 2 to 3xCO2 and the response to negative emissions from 2 to 1xCO2 (PI). They find that there are asymmetries in the feedback parameters and TCRE and attribute those to the land/ocean processes underlying the changes in carbon inventories and the atmospheric fraction as well as climate sensitivity.

The paper is well written and clear in its methodology and results layout. The approach is novel and UVic provides a testbed to assess its usefulness. To that extent I do not have major comments. I would like to highlight a few points, some of which the authors already address, perhaps a little in passing, so I would welcome some comments on those points.

We thank the reviewer for the positive feedback.

The UVic model and its low-complexity structure with regards to the atmospheric component is casting some uncertainty at how much these results are robust and generalizable. Some missing processes here with regards to the physical climate, e.g. storm track representation, biases in air-sea fluxes, overly stable AMOC, etc make in the sensitivity of the feedback parameters to changes in the climate somewhat uncertain. I would like to know what the feedback parameters & TCRE are in this model compared to the CMIP6/5 ensemble.

We thank the reviewer for this important note. Unlike the feedback parameters computed from CMIP5 and CMIP6 ensemble, our feedback parameters are computed from the 2xCO_2 equilibrium state, and therefore, are not directly comparable because feedbacks are state-dependent. We have, however, in previous work used the same version of the model to compute feedbacks from simulations initialized from a preindustrial state, and compared these metrics to those from CMIP5 and CMIP6 (Chimuka et al., 2023). We have included the table from that paper below. We find that our feedback parameters are generally situated well within the CMIP5 and CMIP6 ranges.

Table 1: Comparison of carbon cycle feedback parameters from the ramp-up phase of the “CDR-reversibility” experiment to CMIP5 and CMIP6 model means and mean±1 standard deviations from Figures 5, 6 and Table 1 in Arora et al. (2020), calculated at the time atmospheric CO_2 concentration quadruples (4xCO_2) and doubles (2xCO2) respectively. Model means and mean±1 standard deviations for models without a nitrogen cycle are shown in parentheses. Feedback parameters were calculated using the FULL -BGC approach (see Eq. 7), consistent with the BGC-COU (T*=0) approach used in calculating feedback parameters in CMIP5 and CMIP6. Note that there is a small discrepancy with the BGC-COU approach used to calculate the values in Table A1 of Arora et al., 2020, which allows for T*≠0.

Feedback Parameters (taken at 4xCO_2)	**“CDR-reversibility” ramp up**	**CMIP5**	**CMIP6**

β_L (PgC ppm^{-1})	0.96	0.91 ± 0.48 (1.2 ± 0.3)	0.96 ± 0.39 (1.2 ± 0.5)
β_O (PgC ppm^{-1})	0.88	0.81 ± 0.07	0.78 ± 0.07
γ_L (PgC °C^{-1})	-121.5	-54.7 ± 36 (-75.4 ± 23.9)	-42.7 ± 47.2 (-63.8 ± 70.5)
γ_O (PgC °C^{-1})	-22.7	-16.3 ± 3.5	-16.4 ± 4.6
Feedback Parameters (taken at 2xCO_2)	**"CDR-reversibility" ramp up**	**CMIP5**	**CMIP6**
β_L (PgC ppm^{-1})	1.27	1.15 ± 0.63	1.22 ± 0.40
β_O (PgC ppm^{-1})	0.99	0.95 ± 0.07	0.91 ± 0.09
γ_L (PgC °C^{-1})	-83.5	-37.01 ± 25.48	-34.1 ± 38.39
γ_O (PgC °C^{-1})	-3.30	-9.42 ± 2.70	-8.59 ± 2.9

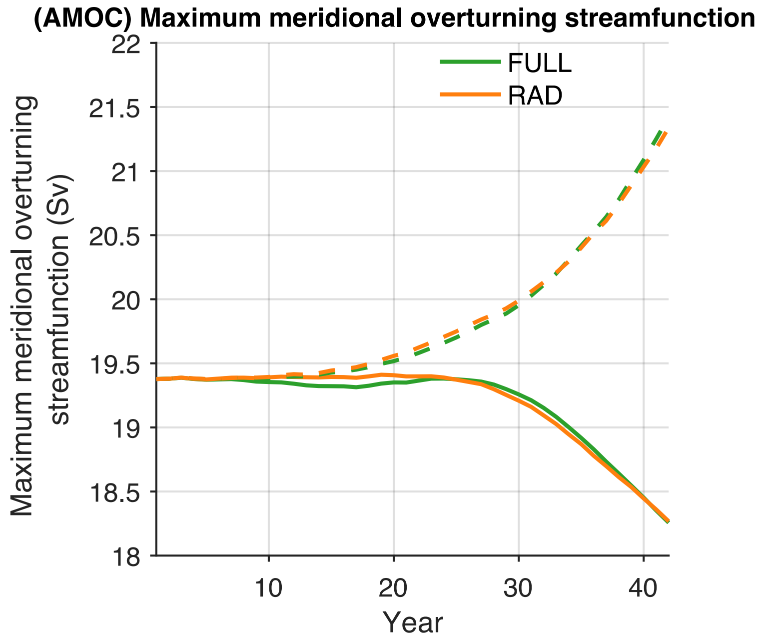
I also wonder how the relative significance of β and γ estimates under fast or slow warming rate as shown in Fig. 5 would compare given the spread of these Arora et al 2020 Fig. 5 -even though they are computed at different levels). In other words, the rate-dependency (especially for the climate-carbon feedback) may not be conclusive if the spread of all the CMIP6 models is considered. Perhaps in subsequent applications of this framework the 3 to 4xCO2 pathway is chosen to make this comparison more straight forward with, say Arora 2020.

Similarly, a future application of this framework could be to start the esm-flat10-conc simulations from PI to 1000PgC to make the comparison with temperature and land/ocean carbon responses comparable to other models that performed the flat10 simulations (as in Sanderson et al 2025).

We thank the reviewer for these comments and respond to both comments below. This is an important consideration and underscores the importance of multi-model analyses to better assess uncertainty in the rate-dependence results. We opted to initialize simulations from the 2xCO_2 equilibrium state in our framework, so that our negative emissions simulation would span a CO_2 concentration range relevant to present day and ensure numerical stability. However, before we ran flat10 simulations from 2xCO_2, we ran flat10 simulations from preindustrial with this version of the UVic ESCM, and ensured consistency with results from Sanderson et al. (2025).

I am somewhat perplexed about the AMOC in Fig. S1.2. First, maybe a more conventional notation can be used, such as 18Sv rather than 1.8 x10-7 m3/s.

Corrected.



Secondly, it seems that at 2xCO2 equilibrium, the AMOC is quite strong. I suppose it weakened with prior emissions (if I judge from the response to subsequent positive emissions) but recovered during the equilibration period? While this makes sense, I am wondering if that maybe masking the response to positive emissions. Typically, the air-sea flux of CO2 saturates somewhere between 2 and 3xCO2 forcing on the 1pct scenario (at least in most models), but if the ocean is left to equilibrate, is the air-sea flux further reduced during the equilibration period? Maybe that is why the ocean sink changes so much slower than the land sink in Fig. 3b and picks up again as the AMOC responds to positive emissions (delayed response compared to the negative emissions). The ocean resists taking up more carbon but gives it up easier.

We thank the reviewer for this comment. During the 2xCO_2 spinup, the AMOC weakens until it stabilizes at a weaker intensity and the air-sea flux declines until it stabilizes around 0 PgC yr^{-1}. From our state-dependence analysis, we see that the higher the initial state, the smaller the magnitude of the asymmetry for beta. The non-linearity in the buffer capacity results in the ocean losing carbon easier than it takes it up, as the reviewer states, and this non-linearity becomes less nuanced with higher initial states.

It should be noted also, that equilibrated simulations at high levels of CO2 are difficult to do in ESMs, due to computational cost, if that framework were to be adopted. And that brings me to my last comment.

If mitigation is to occur, it will probably happen on a transient trajectory rather on an equilibrated trajectory (as in the simulations here). How do we translate the responses of an equilibrated system to those of a transient system which experiences negative emissions. I understand the framework needs equilibrium states to produce "clean" feedback parameters, free from the system's inertia, but we will have to account for this in the actual system's response to CDR, for example. Perhaps using this framework in conjunction with Chimuka 2023? Some discussion on these last two points would be very helpful.

We agree that negative emissions will likely be applied from a transient state as in the CDR-reversibility simulation, for instance. Our goal with designing the experiment was to quantify symmetry, that is, determine whether feedbacks under positive emissions would be of equal magnitude and opposite sign to those under negative emissions, which, as the reviewer stated, required "clean" feedback parameters, free of climate system inertia effects. However, we can see the importance of building on previous frameworks, especially when the previous framework (Chimuka et al. 2023) is easier to reproduce in ESMs. We have, therefore, expanded the discussion comparing this framework to that from Chimuka et al. (2023) to highlight the differences between the two frameworks, and highlight what we learn from this new approach. Furthermore, we also compare our ramp-down experiments to a 1% yr^{-1} ramp-up from preindustrial to 2xCO_2 and have included an additional section ***3.4: An Alternative Experimental Design for Quantifying Carbon Cycle Feedback and TCRE Asymmetry*** *based on two alternative experimental design. Overall, we find that the magnitude of carbon cycle feedback asymmetry is smaller in this experimental design. This arises because temperature change is more symmetric and the range of CO_2 concentrations in both trajectories is the same. The sign of the TCRE reverses and the sign of the carbon cycle feedback asymmetry is positive for land feedbacks and negative for ocean feedbacks. Our conclusion also now includes recommendations for use of the two experimental designs covered in our study.*

Overall, I think this is an important contribution that aims to untangle the system's responses to positive and negative emissions, TCRE and TCRR.

We thank the reviewer for this positive comment.

Citation: https://doi.org/10.5194/egusphere-2025-6405-RC3

Review of "Asymmetry in carbon cycle feedbacks and transient climate response under positive and negative CO_2 emissions" by Chimuka and Zickfeld

The authors present novel simulations with an Earth system model of intermediate complexity, that allow to determine the asymmetry of carbon cycle feedbacks and TCRE/TCRR under positive and negative emissions. They investigate the rate and state-dependency of their results with additional simulations. They find large asymmetries in all of the metrics investigated and a clear rate-dependance. The manuscript is well written, the experimental design is novel and very relevant to address some of the knowledge gaps surrounding carbon dioxide removal (CDR). I did not find any major problems with the manuscript, and I would recommend publication in Biogeosciences after a few comments and suggestions have been addressed by the authors.

We thank the reviewer for their positive comments.

General comments:

I was a bit disappointed about the discussion related to a comparison of results to the previous study by Chimuka et al. (2023). The authors use only two short sentences on this, stating that the results are inconsistent. Given that the same model was used by the same authors, I would have been curious to learn more. Particularly, since I believe that the setup of Chimuka et al. 2023, i.e. running a zero-emission simulation as a reference to single out the committed changes from previous positive emissions, is much better suited for multi-model studies with full ESMs. I agree that the set-up with a 2x-CO2 reference state used in the present study is a very clean experimental design, but I doubt that CMIP modelling groups will be willing to do a full >1000 year spinup at 2xCO2 as a prerequisite for these experiments. Therefore, it would be extremely valuable to learn more about the reason why the results from these two kinds of experiments are different, and I would encourage the authors to add a more details on this in the Discussions section.

We thank the reviewer for this key point. We can see the importance of building on previous frameworks, especially when the framework (Chimuka et al. 2023) is easier to reproduce in ESMs. We have, therefore, included additional text in the Discussion section comparing this framework to that from Chimuka et al. (2023) to highlight the differences between the two frameworks:

4 Discussion

Carbon cycle feedbacks under negative emissions have previously been quantified from CDR reversibility simulations (CDR-MIP: Keller et al., 2014), in which a phase of atmospheric CO_2 decline (negative emissions) immediately follows a phase of CO_2 increase (positive emissions). In these studies, the magnitudes of both feedbacks are smaller under negative emissions because of climate system inertia (Schwinger et al., 2018; Chimuka et al., 2023). Although other studies using preindustrial as the reference year for quantifying feedbacks under negative emissions *show that both feedbacks become larger under negative emissions, they also show the same lagged carbon pool response (Melnikova et al., 2021; Asaadi et al., 2024). Chimuka et al. (2023) proposed an approach to correct for climate system inertia using zero emissions simulations. Compared to the framework used here, there is a difference in the initial state, the range of CO_2 concentration changes, configuration of simulations, and linearity assumptions, and these differences have various implications. First, feedbacks are smaller under positive emissions here than in Chimuka et al. (2023) because the initial state here is higher (2xCO_2 vs pre-industrial). At a higher initial state, the sinks are more saturated, and therefore, the sink sensitivity to changes in CO_2 concentration and the temperature response to a given CO_2 concentration are both smaller. Second, although the CO_2 concentration change in both frameworks is the same (~280ppm), carbon fluxes and pools exhibit different magnitudes of asymmetry due to different ranges of CO_2 concentration in the two frameworks. In this study, the two symmetric simulations span CO_2 concentrations between 2xCO_2 to 1xCO_2 and 2xCO_2 to 3xCO_2, whereas, in Chimuka et al. (2023), the range of CO_2 concentrations is between 1xCO_2 and 2xCO_2. As a result, the two frameworks show different magnitudes of temperature asymmetry due to the logarithmic relationship between CO_2 and temperature change. Furthermore, non-linearities in net primary productivity, soil respiration, buffer capacity and the solubility pump impact carbon cycle dynamics differently in the two simulations.*

After correcting for inertia, Chimuka et al. (2023) found a larger climate-carbon feedback and smaller concentration-carbon feedback under negative emissions, whereas we find a smaller climate-carbon feedback and larger concentration-carbon feedbacks under negative emissions in this study. The two studies suggest opposite signs of asymmetry due to comparison to different magnitudes of feedbacks under positive emissions. When feedbacks here are compared to those from the ramp-up phase of the CDR-reversibility simulation (as in Section 3.4), results are more consistent with Chimuka et al. (2023): we find larger land feedbacks parameters and smaller ocean feedback parameters under negative emissions. Despite discrepancies in sign of asymmetry, our results are qualitatively consistent with Chimuka et al. (2023): feedbacks under negative emissions are larger in our approach than in the standard CDR-reversibility approach. The difference, however, the extent to which feedbacks are larger. In this study, feedbacks under negative emissions are generally smaller than those from the inertia-corrected framework due to limitations in the correction approach related to linearity assumptions, irreversible vegetation shifts and different simulation configurations (concentration-driven CDR-reversibility simulations and emissions-driven zero emissions simulations, compared to concentration-driven simulations here) (Chimuka et al., 2023). The benefit of the novel approach used in this study is that it eliminates climate system inertia by prescribing both CO_2 trajectories from the same equilibrium state, allowing for a more accurate quantification of carbon cycle feedbacks under positive and negative emissions.

I was also a bit confused about the definition of the sign of asymmetry. I would suggest to help the reader by including a definition in terms of an Equation. Also, currently, the definition is somewhat hidden in section 3.2.1. I would suggest to move the definition to the beginning of section 3.2 (also since it applies to both land and ocean).

We have now added a definition of asymmetry in equation form in the Methods in Section 2.3.4. We also include a reminder of this definition at the beginning of the land and ocean carbon asymmetry sections.

2.3.4 Determining the sign of asymmetry

As aforementioned, all metrics were computed under positive and negative emissions and compared to determine the magnitude and sign of asymmetry. The sign of the asymmetry was determined as follows:

Where 'X' denotes β_L, β_O, γ_L, γ_O, TCRE or TCRR under positive ('pos') or negative ('neg') emissions:

[13] $$asymmetry = X_{neg} - X_{pos}$$

Therefore, the asymmetry is positive (negative) when a given metric is larger (smaller) under negative emissions.

Specific comments:

line 129-130: "that cannot yet be achieved in the real world" This sounds as if the authors would be confident that these levels of negative emission could be achieved in the future. I'm not sure this is intended, and I would suggest to reword this sentence.

We agree – that was not the intended meaning. We have clarified this in the text by removing the word "yet".

line 160 and 164: "twice the preindustrial CO_2 concentration for both simulations" This doesn't hold for the $3xCO_2$ and $4xCO_2$ simulations, so maybe better "the CO_2 concentration of the respective equilibrium state from which the simulations were branched off" or similar?

Corrected as suggested.

line 203-204: "However, this warming is considered negligible in this feedback framework". Arora et al. 2020 and Asaadi et al. 2024 (both cited) have looked at the assumption $\Delta T_{BGC}=0$ and found that the effect is indeed relatively small.

These citations have now been added.

Equations 7 and 8: I guess $\Delta T_{BGC}=0$ is assumed also here, too (as in Equations 3 and 4)? I would suggest to adjust Equations 7 and 8 accordingly to not confuse the reader.

We have included a note to clarify that $\Delta T_{BGC} = 0$.

lines 238-239: "All changes in variables were calculated relative to the $2xCO_2$ (twice the preindustrial CO_2 concentration) equilibrium state." See my comment above, this doesn't hold for the 3x and $4xCO_2$.

Corrected.

line 261-262: The logarithmic relationship is not only used by UVic, but widely used in climate sciences (Myre et al. 1998; Etminan et al. 2016).

Cited. We thank the reviewer for these references.

line 345: Here you could in addition cite Schwinger et al. 2014 (which is already cited before, note that it is missing in the list of references)

We have added the citation and included the reference in the bibliography.

line 355-361: This paragraph is largely repetition of what has been said before for the FULL, because the difference between FULL and BGC is so small. I would suggest to shorten this.

We thank the reviewer for pointing this out. We have shortened this paragraph to the key differences between FULL and BGC.

line 373: Delete "land" before "climate-carbon feedback"

Deleted.

lines 388-389: I cannot follow this argument. A larger temperature change under negative emissions means that the land and ocean carbon cycle have actually seen these colder temperatures. A smaller gamma means that land and ocean have reacted less to this cooling per degree of cooling compared to warming. I do not see that the way of calculating would "favour a smaller gamma under negative emissions". Please double check this logic.

We thank the reviewer for this comment. The land (ocean) carbon changes in the positive and negative RAD emissions are small and comparable in magnitude, and therefore, the key differentiator in the magnitude of gamma for land (ocean) is the asymmetry in the temperature response. We have changed the wording to make this clearer.

line 396: The results on the rate dependency (for positive emissions) are consistent with the early results of Gregory et al. 2009 (cited), this could be mentioned.

We have now cited Gregory et al., 2009.

line 405: As far as I can see from Fig. S1.3 is at least the sign of the asymmetry the same for both land and ocean across all equilibrium states?

This is correct. We have now clarified this in the manuscript.

line 474: The correct reference is "Schwinger and Tjiputra (2018)"

Corrected.

line 504: Isn't the sign of the asymmetry robust, as seen from Fig. S1.3?

This is correct. We have now clarified this in the manuscript as well.

References:

Etminan, M., G. Myhre, E. J. Highwood, and K. P. Shine (2016), Radiative forcing of carbon dioxide, methane, and nitrous oxide: A significant revision of the methane radiative forcing, Geophys. Res. Lett., 43, 12,614–12,623, doi:10.1002/2016GL071930.

Myhre, G., E. Highwood, K. Shine, and F. Stordal (1998), New estimates of radiative forcing due to well mixed greenhouse gases, Geophys. Res.Lett., 25(14), 2715–2718, doi:10.1029/98GL01908.

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