Notification to the authors:
Please ensure that the colour schemes used in your maps and charts allow readers with colour vision deficiencies to correctly interpret your findings. Please check your figures using the Coblis – Color Blindness Simulator (https://www.color-blindness.com/coblis-color-blindness-simulator/) and revise the colour schemes accordingly. --> Figs. 2, 4, A2

We changed colors on Figs. 1,2,4,6,7,A1 following the color code for each ESM used in the recent publication in GMD by Sanderson et al. (2025), https://doi.org/10.5194/gmd-18-5699-2025. This keeps consistency between flat10 papers, assuming that color code in Sanderson et al. (2025) is in line with colour vision deficiencies approach. The figure A2 was already published by Winkler et al. (2024) with used colors, we cannot really modify them.

Referee #1 (Vivek Arora)

This is a well-written and interesting manuscript presenting a novel analytical framework for analyzing coupled carbon–climate simulations. The approach is creative, the results are relevant, and the manuscript merits publication. I have provided several detailed comments in the annotated PDF, but here I summarize my major points:

We thank Vivek Arora for constructive and helpful comments on the manuscript, including detailed comments in the annotated PDF (see our response below).

1) I suggest using delta (Δ) notation for variables such as temperature and carbon pools in the ocean, land, and atmosphere. This would make it clear that the quantities represent changes from pre-industrial values.

We were initially undecided whether to use the Δ notation or not. The delta notation is used in the carbon feedback community (Friedlingstein et al., 2006; Arora et al., 2020), but since we use equations for the energy budget and ocean heat uptake, we prefer to follow an approach used in climate studies (Held et al., 2010; Gregory et al., 2009, 2024) where the temperature and carbon variables are for anomalies from the pre-industrial state. This makes the equations shorter and we explain that variables are starting from initial values of zeros.

2) In eqn (3) it becomes clear only later in the text why *(k–1)* is used instead of *k*. This reasoning can be explained upfront.

Agree, we now explain earlier in the text that we need it for simplicity in the equations.

3) Equation (4) is certainly a crude approximation. I feel this needs to be acknowledged a bit more openly with a few references. For example, see Gillett (2023) (https://www.nature.com/articles/s41467-023-42111-x).

Indeed, it is a crude approximation, however it also in line with analysis of Bronselaer and Zanna (2020) who argued regarding TCRE: “This emergent linear relationship is driven by the ocean, and the linearity arises in part from the ocean processes that are responsible for the uptake of heat and carbon”. We noted that Gillett (2023) operated with annual fluxes, while we as well as Bronselaer and Zanna (2020) analyze cumulative fluxes. We added these references into discussion.

4) Consider revising the titles of Sections 2.1 and 2.2 to better reflect the distinction between the two. 2.1 uses linear approximation for F = f(Δ CO2) whereas 2.2 does not. Fortunately in the case of section 2.1 linearity leads to an analytical solution but linearity does not always guarantee an analytical solution.I have made some suggestions in the PDF, but more descriptive titles could be chosen. Figure 4 panels (a) and (b) could be retitled similarly.

We agree and rename the sections as suggested. We also renamed labels on figure panels to "Linear forcing" and "Logarithmic forcing".

In addition, an extra panel showing the actual airborne fraction from the flat10 simulations would help readers directly compare the analytical solutions to the model airborne fraction results.

Apparently, the panel 4(b) shows the model airborne fraction, because we adjected the numerical model parameters in such a way that the model solutions fit the observed CO2 (Fig. 6). Therefore, suggested panel will be almost a copy of the 4b figure and we think we don't need it in the paper.

5) The asymptotic airborne fraction of ~0.3 in Figure 4a contrasts with observation-based estimates of ~0.5. In Figure 4b, the model-mean airborne fraction appears to rise toward ~0.5 after about 100 years. Including actual model-simulated airborne fraction in a panel (c) could clarify how well the analytical approaches capture this.

The airborne fraction on Figure 4b is actual model-simulated airborne fraction (see response to the point 4).

Also, is an airborne fraction ~0.5 is an emergent property of the real Earth system?

This is possible for decadal timescale. On centennial to millennial timescale, the airborne fraction of pulse emissions gets smaller and smaller, see eg Archer et al. (2009). For constant emissions, it depends on time scale as the airborne fraction is increasing with time (Fig. 4b).

6) I can't help comparing Figure 4a, b show airborne fraction (AF) under continued emissions to Figure 5a of Torres Mendonça et al. (2024) (https://bg.copernicus.org/articles/21/1923/2024/) which shows the response to a pulse emission. I realize the distinction between continued and pulse emissions. Can this distinction be made explicit so that readers don't directly compare Figure 4a to figures similar to Figure 5a of Torres Mendonça et al. (2024).

We agree that the study by Torres Mendonça et al. (2024) is not helpful in the context of our analysis because their approach on Fig. 5 is about time scale of the carbon cycle response to a pulse emission and not continuous emissions. We replaced the reference with the C4MIP study by Arora et al. (2020).

7) In Section 2.1 (Equation 13), AF = 1 at $t = 0$, which makes sense for an instantaneous pulse but seems less realistic for continuous emissions. This should be clarified. Again including actual model AF in a new panel 4(c) would be helpful.

AF = 1 at $t = 0$ because the emissions in flat10 are added directly to the atmosphere, and it takes few decades for land and ocean carbon cycle to catch up with the rising atmospheric

CO2 concentration. The actual model AF is the same as on panel 4(b) because the atmospheric CO2 dynamics is captured well in the simple model simulations with logarithmic forcing. We added these points into the text.

8) On p. 12 (lines ~185–200), processes that slow carbon uptake at higher CO_2 for land and ocean are discussed. However, the analytical model is unaware of these processes in Section 2.2. So how does AF actually increase in Figure 4b?

The model is indeed unaware about biogeochemical processes, but it uses semi-empirical relationship between cumulative uptakes reported on Fig. 1. Together with a linear relationship between ocean carbon and heat uptakes and logarithmic dependence of radiative forcing on CO2, this captures the AF increase with time on Fig. 4b.

9) The discussion on page 13 (lines ~220 onward) is insightful but would be stronger if introduced earlier. Also, note that TCRE is constant in Section 2.1 (where F is linear function of atmospheric CO_2 change) but also in Section 2.2. It appears some loose ends need to be tied here.

We moved this discussion from conclusions to the section 2.2, and added that the TCRE is constant also in the case of analytical solution.

Overall, this is an interesting manuscript and additional clarifications will allow readers to gain insight into the underlying properties which lead to emergent behaviour even in this simple framework.

Thank you.

Here are our replies to comments in the annotated PDF:

Page 2-3, edits and corrections: accepted

p.3 Eq.2: we prefer to follow Gregory et al. (2024) notation style without deltas (see our response above)

Eq. 3: the rational for (k-1) is explained

Eq 4: we prefer not to use deltas, see our rational above

Figure 1: for comparing with historical carbon budget, we use historical S2 simulation from TRENDY experiments (Sitch et al., GBC, 2024). In this S2 simulation, CO2 and climate evolve over the historical period, while the land cover stays at its pre-industrial level. This S2 simulation is reported as terrestrial carbon sink in the Global Carbon Budget (Friedlingstein et al. 2024) “The terrestrial sink is estimated from the average of 20 Dynamic Global Vegetation Models (calculated from simulation S2)”: https://globalcarbonbudgetdata.org/downloads/jGJH0-data/Global_Carbon_Budget_2024_v1.0.xlsx

We added an explanation into the paper.

Eq. 5: we prefer to explicitly mention time (t) in the right part of the Eq. 5, as it is used later for the analytical solution.

Fig.2 caption: ZJ is explained (zeta joules).

Table 3, “r” in linear and logarithmic approximations: we use constant value r=5.35 W/m2 for both approximations and all models (reported in the Table A1), therefore there is no added value in reporting “r” for every model in the Table 3.

l. 139, ramp-down parameters for MPI-ESM will be reported in the text of the section 2.3. The approach captures well the ramp-down part with constant emissions but not the transition period from +10 to -10 PgC/yr emissions. This is a limitation of our approach and we added it to the discussion.

Section 3: we agree that flat10 helps to evaluate the framework, but the analytical framework is rather independent from flat10 runs. Text corrected.

Line 164: the details of the ACCESS model are provided by the ACCESS modelling team. The N and P limitations contribute to the saturation of land carbon uptake in CABLE, but the main reasons are the response of heterotrophic respiration and optimal temperature of photosynthesis in tropics, as explained in the text. We added a sentence on the P limitation and a reference to Ziehn et al. (2021).

Line 170: suggested remark is added.

Line 176: We prefer to discuss reasons for adjusted parameters in this section when we discuss the Figure 6.

l. 189-197: The analytical model is indeed unaware about biogeochemical processes behind land and ocean carbon uptake, but it uses semi-empirical relationship between cumulative uptakes reported on Fig. 1. Together with a linear relationship between ocean carbon and heat uptakes. This is enough to capture the AF increase with time on Fig. 4, right.

l.211: “natural” carbon cycle means the carbon cycle without landuse fluxes.

l. 218: agree with “variables” instead “dimensions”.

l. 219-224: We updated the main text where we discuss the Figure 4.

Fig. A1, caption: corrected.

I will be happy to read a revised version of this manuscript.

Refs:

Archer, D., Eby, M., Brovkin, V., Ridgwell, A., Cao, L., Mikolajewicz, U., Caldeira, K., Matsumoto, K., Munhoven, G., Montenegro, A., Tokos, K., 2009. Atmospheric lifetime of fossil-fuel carbon dioxide, *Annual Reviews of Earth and Planetary Sciences,* 37, 117-134.

Bronselaer, B., Zanna, L. Heat and carbon coupling reveals ocean warming due to circulation changes. *Nature* 584, 227–233 (2020). https://doi.org/10.1038/s41586-020-2573-5

Gregory, J. M., C. D. Jones, P. Cadule, and P. Friedlingstein, 2009: Quantifying Carbon Cycle Feedbacks. *J. Climate*, 22, 5232–5250, https://doi.org/10.1175/2009JCLI2949.1.

Held, I. M., M. Winton, K. Takahashi, T. Delworth, F. Zeng, and G. K. Vallis, 2010: Probing the Fast and Slow Components of Global Warming by Returning Abruptly to Preindustrial Forcing. J. Climate, 23, 2418–2427, https://doi.org/10.1175/2009JCLI3466.1.

Referee #2

Summary

The paper proposes two tractable models that allow modelling the response of the globally averaged temperature to carbon dioxide forcing. The authors use an energy balance model to derive a model that is in agreement with the hypothesis of linear response of global mean annual temperature to cumulative carbon emissions. The authors derive an analytical solution from the differential equation of the EBM allowing to represent atmospheric carbon content Ca(t) and global annual mean temperature T(t). This requires assuming a linear relationship between carbon content relative to (preindustrial) equilibrium and greenhouse effect radiative forcing as well as a linear relationship between ocean heat and carbon uptake. A numerically solvable model is proposed to when the standard log relationship empirically demonstrate by Myhre et al. The results of the simplified model are compared with a set of eight 3D ESMs. These results show that within a range of annual emissions the linear response T(t) to cumulative emissions obtained by simplified models are in good agreement with those of the set of ESMs chosen.

General comments

The manuscript is written in a clear fashion. The research question proposed, the methodology and results obtained are clearly presented and the assumptions taken are well argued. The results obtained by the paper provide a useful tractable model that can be used for research in areas beyond the climate sciences, for instance in economics and finance where tractable models are needed for coupling with the state-of-the-art models used. The paper is worthy of publication.

We thank the reviewer for the positive view on usefulness of our approach.

It could could however be improved by addressing the following points:

1. The results presented show that the analytically solvable model is in agreement with the results obtained with the ESMs up to a point. Although mentioned, could the authors further stress the range of cumulative emissions where the analytically solvable model is in good agreement and when it is no longer reliable?.

The analytical solution is valid for several decades, as long as the CO_2 increase is not that strong and a linear approximation of radiative forcing dependence on CO_2 is quite precise. After several decades, the linear approximation results in too strong heat and carbon uptake and therefore too small airborne CO_2 fraction.

2. While the authors focus on the flat10MIP experiment to carry out comparison and validation of their model that supports the TCRE heuristic of linear response to cumulative emissions, it would be useful for the reader to (briefly) mention that the climate system is inherently nonlinear and that the linearity assumption holds within a certain range of temperature and carbon which is still uncertain today.

Thanks, this is really important point. While climate system is nonlinear, ESMs on the global scale behave linear. We added a paragraph on that in the conclusion section.

Several studies also cited in the same AR6 report (section 7.4.3.1 State-dependence of Feedbacks in Models) stress the limits of this proportionality relationship which no longer holds at higher atmospheric carbon levels and global temperature.

The range of 1000 PgC emissions in flat10 experiments roughly corresponds to 2°C degree global warming. Beyond that, we cannot conclude much from the flat10 experiments.

3. It would be useful the understand whether (and why) the subset of 8 ESMs used in flat10MIP are representative of the over 45 models included in IPCC reports and participating in CMIP experiments. Do the 8 models cover the spread of CMIP6 models? If not, several of the conclusions should be tempered to note that they appear robust across the set of tested models under flat emissions.

Not all 45 models in CMIP6 could be run in the emission-driven mode. We can only answer on the question what is going in the flat10 models, we will make it more clear in paper. In addition, the analysis of concentration-driven model results in C4MIP gives an idea about few other ESM.

4. Concerning the discussion made and the justification to exclude ACCESS from the analysis of the climate-carbon dynamics, could the authors detail whether ACCESS uses a different land surface model than the 7 other ESMs to support the hypothesis made?

The ACCESS model is special regarding the land uptake and we tried to explain it in the text. See our response to the comments of Vivek Arora.

5. In Table 3, the authors present “adjusted parameters” for analytical and numerical solutions. Could the authors further comment (already started in lines 175-179) on the large wedge between estimated and adjusted parameter for certain models (UKESM, CESM2) in contrast with the good fit with MPI-ESM?

We started our analysis with MPI-ESM and when used the same methodology for the other models. One issue with adjusting parameters was the initial period of few decades, when the climate feedback parameter taken from 4xCO_2 ESMS simulations deviates from the later period. The second is the non-linear relationship between carbon and heat uptake in the ocean. These both results in a need for the parameter adjustment.

6. Could the authors elaborate on the reason for only showing results with the MPI model for the CDR experiments in section 2.3. Is it a question of data availability which may be? If so, please do say so, and otherwise could results with all other models be also presented to support the claim made.

It is not a problem of the data availability; we just use one model as an example to illustrate the linear behavior in the ramp-down mode. We think it is enough for this purpose, but if the reviewer insists, we can plot results from the other flat10cdr simulations in the Appendix.

Detailed comments

- The size of the x- and y-axis labels and ticks should be increased for readability (as as well as the font size of the legends in each of the subplots across figures).
- done
- In Fig 6 the label should be NorESM2-LM not NORESM2-LM
- corrected
- Fig A3: Could the authors write in the figure text what GCB stands for?
- GCB stands for Global Carbon Budget, we explain this now in the text
- Across the whole article, I read "GCB" in the legend of figures, however it should be "GCP" I think.
- "Global carbon budget" is a product of the Global Carbon project, we explain this in the text.
- Fig 2 text could eliminate "analogous to Figure 1"
- done
- In Fig 7 it would be useful to use different colors for the flat10 and the cdr experiments that look very similar.
- colors are modified
- Also, why is there the bisectrice in the left panel and not on the right panel.
- Left panel has the same units for both axes, therefore the 1:1 line is appropriate. It is not a case for the right panel.
- There seems to be a typo in Winker et al (2024) in Appendix A2 – which should be Winkler.
- Thanks, corrected.
- Line 60, "zeroes" should be "zeros"
- done
- Line 107: "Because the later term is proportional" should be "Because the la**tt**er term is proportional"
- We think it is either "the later term" or "the latter". We replace it with "the latter".
- The title of section 2.1 should be "Analytical solution for **the** dynamical system"
- The title is modified