**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 CO2 increase is not that strong and a linear approximation of radiative forcing dependence on CO2 is quite precise. After several decades, the linear approximation results in too strong heat and carbon uptake and therefore too small airborne CO2 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 will stress this in the paper.

3. 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.

4. 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.

5. 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.

6. 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 4xCO2 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.

7. 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
- Thanks, corrected
- Fig A3: Could the authors write in the figure text what GCB stands for?
- GCB stands for Global Carbon Budget, we will explain this 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 will 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 will be 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"
- done
- The title of section 2.1 should be "Analytical solution for **the** dynamical system"
- The title will be modified