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.

We will 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 will add 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 will rename the sections accordingly and consider renaming labels on figure panels.

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 gets smaller and smaller, see eg Archer et al. (2009).

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 will make a better explanation in the text

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.

We will clarify this.

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 will improve the discussion section.

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) will be 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 will add 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 will be 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 which we will add to the discussion.

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

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.

Line 170: suggested remark will be 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-emperical 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 will update 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.