Reviewer Responses

Reviewer comments are in red. Our responses are in black. Quoted text that are changes to the manuscript are *italicised*.

General comments

This interesting manuscript describes a simple/conceptual model, and secondarily the intermediate-complexity JULES model, to explore in what circumstances carbon cycle instabilities can occur. The results are interesting—instability depends on ECS and CO2 fertilization—and the authors nicely document that the two models produce qualitatively similar results, although computational limits preclude a full comparison. The methods are generally clear and well written, and this is appropriate for EGUsphere and will be of interest to a wide audience.

There are some significant problems. First, there's very little acknowledgement of previous research and similar studies—the discussion is particularly inadequate in this respect. Second, this is not a mathematical journal, and I think it would be worth defining many terms and methods more fully, i.e. clarifying the methods in many places. There are some structural problems. Finally, some aspects of the methods and study are unclear.

In summary, this is an interesting experiment that will be of wide interest, but the current ms needs major revisions to acknowledge previous literature; make it more appropriate for the journal's readership; and improve the structure and flow of the text.

We thank Reviewer Two for a careful review of our paper. As well as responding to specific comments below, we have updated the manuscript to define more mathematical terms and rewritten the discussion to acknowledge other studies.

Specific comments

Line 4: perhaps clearly define what you mean by "unstable"

In the introduction we add the text: "*instability […] in which small perturbations to atmospheric carbon grow rapidly*"

11: a final sentence would be useful, laying out implications of this study

We add *"Given the apparent stability of the climate-carbon cycle, we can view these parameter combinations as implausible."*

Introduction: have no other papers explored this since Cox 2006?

To the best of our knowledge, no other papers have explored the respiration-warming instability since Cox 2006. A google scholar search gives 30 articles which cite Cox 2006 (including this manuscript), however these articles are focused on the sink to source transition rather than the instability in Cox 2006. However, there are other works on carbon-cycle instability more generally. We add to the discussion: *"That climate-carbon instability can play a role in Earth system, specifically paleoclimate, dynamics has long been appreciated. For example Saltzman 1988 showed how unstable interactions between physical climate and ocean carbon uptake could lead to oscillations resembling the Pleistocene glacial-interglacial cycles. More recently, Boot 2022 showed how ocean circulation can induce a Hopf bifurcation in the carbon cycle, potentially explaining some CO2 variability. On longer timescales, Hulse 2025 demonstrated that different carbon cycle processes could 'out-compete' the silicate weathering thermostat, leading to significant cooling. The carbon cycle may also be excitable (Rothman 2019), meaning small perturbations lead to large responses. Extreme fluctuations can be seen in isotopic records which show a heavy-tailed distribution (Arnscheidt 2021). Carbon cycle instability can even be seen in high CO2 exoplanets (Graham 2024). This paper (and Cox 2006) is unusual in that it studies an instability in the interaction of the terrestrial biosphere with the physical climate."*

69: "fitted to the outputs of more"

Changed

72: "II"?

This is a capital pi (for **p**roduction). To make it clearer we are using this for NPP as a variable name, we change this line to: *"Net Primary Production (NPP), denoted by Π,"*

132: What's the total quantity of carbon in the simple model system, and at steady state how much is partitioned into atmosphere, land,and ocean? How does it compare to e.g. GCP2024 numbers?

The stores of atmospheric and land carbon are free parameters of the model; they are set to pre-industrial values. However the total carbon is dominated by the ocean carbon which is a function of the ocean parameters. Our ocean model parameters were chosen to get the historical ocean carbon sink within observational estimates. As our ocean carbon model has only two boxes, it is not suitable to simulate the size of the total ocean carbon store on multi-millennial timescales. However, as it is the ratio of the carbon stored in the atmosphere to the carbon stored on the land that appears in equation (13), the stability of the system is not dependent on the absolute size of the total ocean carbon store. To clarify this we add: *"We fit to the ocean uptake rather than the total ocean carbon as the response to elevated CO2 plays an important role in the system's stability. As our ocean carbon model has only two boxes, it is not suitable to accurately simulate the size of the total ocean carbon store on multi-millennial timescales."* and *"As it is the ratio, rather than the sum, of carbon stored in the atmosphere to*

carbon stored in the land that appears in equation (13), the stability of the system does not depend straightforwardly on the total carbon store."

157: this isn't a specialist journal, so it seems important to define what a Hopf bifurcation is; same comment re "bifurcation diagram" in line 200 and "eigenvalues of the Jacobian" in line 208. Some readers will understand, many will not

We add: *"a Hopf bifurcation, which means the system transitions from a steady state to an oscillatory state."* and *"plotting the system's equilibrium state against a varying parameter, known as a bifurcation diagram"* and "*eigenvalues of the Jacobian, which are the solutions to equation 9.*"

222-250: belongs in the methods

Rather than having a separate methods section, we treat the simple model and JULES in their own self contained sections. We have restructured the JULES section and used subsections to make that section clearer.

262-274: this just restates previous material; remove or greatly condense

The discussion has been rewritten.

The discussion is completely inadequate; in particular, I doubt that no one else has performed exercises like this in the last 20 years (see #3 above), and it's important to compare and situate these results in the context of current scholarship. Currently there are almost no literature citations.

We have rewritten the discussion. In particular we have improved the discussion of model assumptions and limitations as well as adding in a discussion of other carbon cycle instabilities (see prior response)