Review “Conditions for Instability in the Climate-Carbon Cycle System”

In this paper the authors use a conceptual model and a more complex model to investigate possible climate carbon cycle instabilities. They find that whether such an instability can occur depends on the equilibrium climate sensitivity of the model and the strength of CO_2 fertilization. This paper is a very interesting read, using some really cool methods and producing some interesting results. I believe the paper will make a valuable contribution to the field and is very suitable for the journal. I believe major revisions are in order before it can be published though. I’ve listed some main comments/concerns regarding the paper, followed by a few more specific comments.

Main comments

1. Framing of the work:
    a. Currently, part of the framing is that some Earth System Models (ESMs) have a high Equilibrium Climate Sensitivity (ECS), which might result in climate-carbon cycle instabilities in these models. I think how it is framed in the introduction is fine. However, I’m not sure whether how it is treated in the penultimate paragraph of Section 4 (lines 294 – 298) works that well, since if ESMs would suffer from these type of instabilities, they would be tuned such that the instabilities wouldn’t occur. From a steady state perspective, I do not expect ESMs to have these instabilities. From a transient perspective then it might be relevant. I suggest reframing the conclusions in Section 4 to take this into account.
2. Conceptual model
    I think the use of a conceptual model can be very powerful, however, I think in this paper the description of the model and the assumptions should be extended.
    a. Upon skimming the Cummins et al. (2020) paper, I realized that T_1 and T_2 are actually supposed to be temperature anomalies. Is this correct? This is not mentioned in the text I think. Obviously, this is extremely important to be able to properly interpret the results.

    b. It is unclear to me to what extent the model is based on previous work. To what extent is the model original to this paper? From what I understand now, (part of) the model is indeed based on some earlier work. I suggest making this connection clearer.

    c. I suggest explicitly stating that the temperature state variables and the carbon state variables are not representing the same ‘box’.
    d. I suggest explicitly stating the units of the state variables.

e. How much carbon is there in the system, i.e. what is the sum of equation 2? Are the results sensitive to this quantity? I'd say this is especially important when put in the context of anthropogenic emissions that would raise this quantity on the timescales assessed in this paper.

f. The assumption for the no temperature sensitivity in soil carbon stems from the Varney et al. (2023) paper. Skimming through this paper, I suspect the main motivation for this assumption is found in Fig. 10c where it shows that more than 50% of the changes can be explained by changes in CO_2. Do I understand it correctly that this means that the rest of these changes are related to climate change? Looking at Fig. 10 these can still be relatively large for some models. Is this all temperature or also other changes in the climate system?

g. I am not satisfied with how the assumption of no temperature dependency in the solubility of CO_2 in the ocean is treated. I find this a rather strong assumption without citing previous work or giving a good indication on why this assumption is okay to make.

   From my perspective, writing in a temperature dependency for k should be doable, plus it would add an additional positive feedback to the system. E.g. use the equation of Weiss (1974) for K_0, which as I understand it should be the 1/k parameter in your model. An assumption for salinity needs to be made, which I would say is more valid than the no temperature dependency assumption made now.

   However, if T_1 and T_2 are indeed temperature anomalies then adding the temperature dependency might be a bit more difficult. Looking at the values of k, k_1 and k_2 they appear to be taken at a T_0 of 10°C, so one possibility is to use $T_0 + T_1$ in the Weiss (1974) equation as temperature. Though since T_1 also represents the atmosphere this might also not be a valid assumption.

h. As far as I understand it now, the carbonate chemistry is solved for by assuming alkalinity is equal to carbonate alkalinity. It is not clear in the text that this is assumed. Furthermore, the implications of this assumption are also not mentioned. For example, pH values are typically 0.15 – 0.20 lower using this method compared to more sophisticated methods (Munhoven, 2013). I suggest being clearer about this assumption and the implications of the assumption.

i. Do the uptake rates in the ocean also capture processes related to the biological and carbonate pumps?

j. What I think should be made more explicit is for what timescales this model is valid. This is also relevant for simulations with different C_A* as shown in Fig. 5.

k. How I interpret the model is that C_A* does not necessarily represent pre-industrial CO_2 concentrations but the stable CO_2 concentration on a certain timescale. Am I correct in this? If so, I suggest clarifying this.

l. Currently there is only a few sentences on the assumptions in the discussion. I think the assumptions and the implications of these assumptions, as well as the timescales involved in this model, should be more thoroughly discussed in Section 4.

3. Description IMOGEN/JULES
   a. I think the description of the model setup, including assumptions, especially with regards to the carbon cycle model, could be more extensive. For me specifically I would like to know more how IMOGEN works and whether the coupling between IMOGEN and JULES is in one or both directions.

4. Results
   a. It is also stated in Section 4, but I think it would be good to also note it in Section 2 that the limit cycle shows behaviour in which the model assumptions, including the timescales resolved, are not valid anymore.

   b. I think it is very important to very explicitly spell out the physical mechanism behind the instabilities and the limit cycle in the conceptual model.

   c. JULES is a more sophisticated model, if the mechanism in JULES is similar to the mechanism in the conceptual model, the results will be much more powerful. However, the mechanism in JULES is not really discussed. Is the underlying mechanism that causes the instability in JULES the same as in the conceptual model? This comparison is essential for me to lend credibility to the results of the conceptual model.

   d. There is no discussion about how certain it is that the JULES simulations above 11K ECS are actually unstable. In the conceptual model there are internal oscillations on longer timescales than the duration of the JULES simulations. Would it be possible to extend one of the simulations with e.g. another 5000 years to be a bit more certain that the model is moving towards a runaway state?

5. Discussion
    a. As mentioned in the previous comments, I would like a more thorough discussion on the assumptions in the model and their potential effect on the results and conclusions. Also, a discussion on the used parameter values from Table 1 would add value I think (how certain/realistic are these values? How sensitive is the model to their values?).

    b. What is still missing, in my opinion, is how these results compare to what is found in the literature. In the introduction already a few studies were named. Studies focusing on the marine carbon cycle are for example Rothman (2019) and Boot et al. (2022). There could also be a connection made to paleo events, e.g. the Paleocene-Eocene Thermal Maximum (PETM). For some more conceptual work see e.g. Arnscheidt and Rothman (2021). The literature mentioned here are just suggestions and do not have to be included.

    c. The CO_2 fertilisation effect plays a central role in the results, but I didn't see a reference to what is actually realistic. Do we know what is realistic? Do we know what the values are for ESMs?

Specific and technical comments

1. Figure 1: 'Increased CO_2 solubility' should be 'Decreased CO_2 solubility' I guess. You could also include ocean acidification in there as a positive feedback.
2. Line 23: I suggest rewriting this sentence, specifically the 'even here' part.
3. Line 42: Would it make sense to mention quantitative results from the Cox et al. (2006) study here?
4. Line 53: 'and and'.
5. Figures (general): all figures (except Fig. 1) miss certain text on the labels and the tick marks.
6. Figure 3: I suggest mentioning explicitly which panel (top or bottom) represents without CO_2 fertilisation and including CO_2 fertilisation. I also suggest switching the order of the two for two reasons: (1) the case with CO_2 fertilisation is mentioned first in the text, and (2) in Fig. 4 it is also switched, i.e. first with fertilisation then without.
7. Line 206: I guess the reference should be to Figure 3 not Figure 2.

8. Figure 5: As mentioned earlier, I would not call C_A* 'pre-industrial' but something like steady state concentration (though C_A is I think not defined as a concentration).
9. Figure 6: Can you explain a bit more what the dotted line is, and how it is determined? I'm not sure whether it is necessary to include an elaborate discussion in the text or caption, but I'd like to know a little bit more about it.

References:

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- Munhoven, G.: Mathematics of the total alkalinity–pH equation – pathway to robust and universal solution algorithms: the SolveSAPHE package v1.0.1, Geosci. Model Dev., 6, 1367–1388, https://doi.org/10.5194/gmd-6-1367-2013, 2013.
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