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Climate of the Past Discussions

Interactive comment on "Impact of oceanic processes on the carbon cycle during the last termination" *by* N. Bouttes et al.

Anonymous Referee #1

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This paper describes simulation experiments with the CLIMBER-2 EMIC model in order to understand carbon cycle dynamics over Termination I. Here, simulation results are compared with atmospheric CO_2, atmospheric $\delta^{13}CO_2$, and deep ocean $\delta^{13}C$ in the Southern Ocean evolving over time between 20 and 10 kyr BP. In principle this is still a topic not finally understood and therefore a welcome contribution well fitted within the scope of the journal.

1 General appearance

In general terms the papers is written in a way which is very often not exact in the details and therefore the written text needs a major improvement. This manifests for example in the way how changes over time are described. The authors try to simulate forward in time the changes in the carbon cycle, thus they evolve their model from LGM to the beginning of the Holocene implying a rise in CO_2 over time. However, very often this is confused with a decrease in CO_2, which is correct in absolute terms with respect to today, but which is in the context of forward modelling not correct. Which makes things very difficult is that it is not consistently wrong, but mixed up. Further below I list examples which should be revised accordingly.

This was about wording and readability. Other points for improvements concern rather vague explanations in the way how changes are described. For example, in the introduction it is said that the glacial state was -2 to -6°C colder in the Southern Ocean. But what variable is meant here? SST? Deep ocean temperature? Surface air temperature? Another example: It is often written about the "increase of the terrestrial biosphere". The is no such thing, it probably means that the C content of the terrestrial biosphere increases, but this should then also be written down. Unfortunately, this consists throughout the MS and need a major improvement to get the article in an acceptable shape.

2 Major suggestions concerning the content

1. The novel aspect of this study is the use of an EMIC transient over time with an interactive carbon cycle. Similar experiments so far were focusing on box modelling approaches. However, the authors are not addressing two recent studies under discussion in this same journal, which are:

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- Deep ocean ventilation, carbon isotopes, marine sedimentation and the deglacial CO2 rise T. Tschumi, F. Joos, M. Gehlen, and C. Heinze Clim. Past Discuss., 6, 1895-1958, 2010
- Glacial CO2 cycle as a succession of key physical and biogeochemical processes V. Brovkin, A. Ganopolski, D. Archer, and G. Munhoven Clim. Past Discuss., 7, 1767-1795, 2011

While the Tschumi paper seemed to be very likely published in a revised version (according to the online discussion), not a lot can be said about the status of the Brovkin paper, however as there the same model is used - CLIMBER-2, I think the approaches of both papers should be discussed widely within the present paper. While the Tschumi paper analyses the impact of different processes on the carbon cycle starting from a constant climate (comparable to sect 3.1 of the Bouttes paper), Brovkin simulates a full glacial cycle (120 kyr). Both papers make strong cases about how their approaches can explain some fraction of data-based changes over the glacial/interglacial cycles without the apparent major process of the Bouttes approach (brine rejection). This should be discussed as wide and as far as possible. I acknowledge, that both papers are not finally published, but since they are discussed already the authors should address how their findings can be compared with them. Discussion papers available online here opens for the possibility to include comparing discussions of papers on the same subject much earlier than when submitted to more classical journals without this open discussion section. This should be seen as opportunity to speed up the scientific discussion.

2. The authors claim, that they for the first time have an EMIC simulation forward in time with an interactive C cycle. They state insolation, ice sheet extent and atmospheric CO_2 content as only forcing of the model. If the CO_2 is now calculated interactively it implies that for those results that do not meet the measured values the forcing would be smaller than for those scenarios where CO_2 perfectly

matches the data. This would have feedback effects, e.g. small change in CO_2 leads to smaller change in global temperature, which again leads to a smaller CO_2 change in the ocean carbon uptake/release (via solubility pump) or C storage on land. To give the reader a possibility to estimate how large these feedback effects might be, it is desirable to have the relative size of the forcing explained, e.g. insolation, ice sheet, CO_2. Furthermore, what about the radiative forcing of other two greenhouse gases CH_4 and N_2O which contribute to a forcing which is about 30% of that of CO_2?

3. I acknowledge that the C cycle is in focus here. But to set the overall model performance into context to data it should be shown, how temperature evolves over time. It should briefly be expanded on the climate state at the end of the initialisation (after 50 000 yr) to see the general performance of the model. This is in detail probably written in other papers, but to make this paper here more independent especially the distribution of temperature should be analysed. Is the model in a similar state than in a previous application, analysing an LGM to be -5.8 K cooling than preindustrial times (*Schneider von Deimling et al.*, 2006)? Furthermore, how is temperature evolving over time in the interactive final run? As the only non-C cycle aspect the change in ocean circulation were plotted in Fig 5 and 12. However, they are hardly explained in the text, which should be expanded. Furthermore, if the stratification is changed in the Southern Ocean by the brine process, it would be good to see HOW stratification is changed. Or should that be seen as AABW in Figs 5 and 12?

4. In their ultimate last scenario the authors calculate with interactive C cycle and include all the three processes introduces before, which are iron fertilisation, brine rejection and a stratification-dependent diffusion (sect 3.3). However, in all previous investigations (sec 3.1 and 3.2) without interactive C cycle all three were never used together, they were either analysed individually or in pairwise combinations (brine+iron, brine+diffusion). This is most confusing. If the combination

of all three processes are the ultimate suggestions of this papers to explain the data this should also be investigated without interactive C cycle coupling and from steady state climate.

5. As the authors try to make statements for transient simulations including the comparison with proxy data some more care is necessary concerning the dating of the different data sets. The authors show ice core CO_2 data from two studies (Monnin, Lourantou). However in both study different age models are used for presentation of the data which leads to an offset in the start of the CO_2 rise by some centuries. This might in detail not be resolvable in the simulation results, but for comparison and completeness the used age model should be stated. Similarly, the iron fertilisation is said to follow dust transport according Wolff et al. (2006). In this paper ice core data from EPICA Dome C are plotted on an older age model, EDC2, while things look slightly different on the new age model EDC3 (*Parrenin et al.*, 2007). Thus, for completeness it first needs to be known which age model is used, and second it should at best be the same (and probably the most recent) one as the one used for the CO_2 and $\delta^{13}CO_2$ record. Additionally, why do the authors say they synchronise iron fertilisation to dust transport, when in the Wolff paper iron fluxes (not only dust fluxes) itself are given. This should be revised accordingly.

6. Going in the same direction, the way how the brine scenario is forced (changes in the respective parameter "frac" starting at 18 kyr BP either abrupt or linearly) is only weakly motivated. In explaining the brine process in Fig. 7 I had the impression the maximum brine rejection should be during the termination with smaller values during LGM and during interglacials. This is not the case, brine is only reduced from LGM values. Furthermore the timing seemed to be arbitrary. Thus some more statements if and how this process might be coupled to sea level evolution are wanted and might in the end lead to a different timing!

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7. It was chosen to simulate the last glacial/interglacial transition without abrupt climate changes connected with the Heinrich 1 event and the Younger Dryas. It is therefore a priori clear that the dynamics contained in the atmospheric $\delta^{13}CO_2$ record can not be matched. As written at least twice throughout the text the $\delta^{13}CO_2$ record of Lourantou contains a w-shape. Lourantou et al. already provided a lot of evidences that this might be caused by the rapid climate changes during Termination I. To streamline the whole paper the authors might consider to drop the whole discussion of the $\delta^{13}CO_2$ record. This might especially be important when the discussion and results sections are extended as suggested here.

8. The paper misses a final analysis in which the contribution of various processes over time are disentangled. The authors finally agree on a best guess scenario, but how much of the changes in ocean $\delta^{13}C$ and atmospheric CO_2 is due to a) the physics (solubility pump (changes in SST), ocean circulation (including the brine and diffusion mechanism), b) biological pump (iron fertilisation, but also changes in marine biota due to changes in climate), c) terrestrial C uptake. I realised this kind of fractional analysis was presented for LGM steady state in Bouttes et al. 2011 GRL, Fig.3. So the authors might consider if this figure might be extended towards a time-dependent version. One might then learn, which processes change first and initiate the whole changes in the C cycle.

3 Minors in chronological order

1. Intro and results: Evolution of the depth gradient in $\delta^{13}C$ in the Southern Ocean is published in *Hodell et al.* (2003).

2. Intro, 2nd paragraph page 1890: Here, the thinking in temporal evolution of processes and in changes from present day is mixed up. They write about a "CO_2

increase" (thus thinking in the evolution of CO_2, not in the change from present days). But CO_2 INCREASES due to a reduced (not enhanced) biological pump, CO_2 should rise (not drop), sea ice over Termination I is reduced (not increased). It is impossible to list all confusing statements here, thus this is illustrative to make the authors aware how they mix up both ways of describing the changes. A careful revision of the whole text for these things is necessary.

3. Section 2.2.1: Proxy-based evidences of a changed biological pump highly depend on the location, e.g. north or south of the polar front, see *Kohfeld et al.* (2005). Is this relevant here?

4. Section 2.2.2 and 2.2.3: In explaining your scenarios your should also mention, which is the chosen parameter value used here for frac and α and how the chosen value refer to the previous studies (2011 in GRL), e.g. were they taken as the best guess from previous simulations, or were they fitted / tuned especially for the dynamics during Termination I investigated here?

5. Results for constant climate (sect 3.1): I think the way you calculate the return to near equilibrium by reaching 95% of the equilibrium values is maybe too simplistic. For $CO_2$95% of the equilibrium value of about 257 ppm is 244 ppmv (–13 ppm). The CO_2 anomaly in the iron fertilisation scenario is only 29 ppm. Thus by a rise of only $29 - 13 = 16$ ppm this equilibrium threshold is crossed. Furthermore it lead to an analysis where the equilibrium is already achieved, while changes are already underway, e.g. in the linear scenarios of IRON or BRINES (equilibrium reached after 4400 yr, but changes is for 5000 yr). I therefore suggest to choose an even stricter threshold (e.g. 99%) or you define the reach of equilibrium with respect to the CO_2 anomaly. E.g. in IRON CO_2 initially is reduced by 29 ppm, so calculate until 95% from this anomaly is again gained back, thus when CO_2 rises by 27.6 ppm after the start of the change in the forcing.

6. Another example of sloppy writing: Beginning of sect 3.1.3 it reads: "The brine

sinking mechanism leads to a larger atmospheric CO_2 increase than iron fertilisation (Fig. 2b) with a change of 40 ppm". This is in detail not correct. The brine sinking mechanism leads to a larger DECREASE in CO_2 than iron fertilisation. Or: The STOPPING of the brine mechanism leads to a larger INCREASE in CO_2 than the STOPPING of the iron fertilisation.

Same paragraph: "The response of the system to the abrupt halt of brine sinking takes more time than iron fertilisation. " Iron fertilisation itself does not take time, CHANGES in the process and its effect of CO_2 might take time.

7. page 1899, or throughout the results: "upper $\delta^{13}C_{\text{ocean}}$" or "deep $\delta^{13}C_{\text{ocean}}$" is not a proper wording, it should be "upper ocean δ^{13}C" and " deep ocean δ^{13}C".

8. page 1900: "When the stratification collapses the diffusion coefficient progressively increases." This needs explanation.

9. sect 3.2: The complete description of the forcing scenarios should be move to sect 2, only results should be seen here.

10. page 1902, line 1: "the continental shelves are covered simultaneously". simultaneously with what?

11. page 1902, first paragraph. The description including the impact of northern hemispheric ice sheets on sea level needs some refinements.

12. page 1902, lines 22-25: Effect of sea level on nutrients on biological pump versus effect of sea level on salinity: These statements are too general and need some numbers from the analysis of simulation results.

13. throughout the results: It is not necessary to describe the colour and shape of the figs in the text, it is enough if they are properly label ed in the figs themselves.

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14. page 1903, lines 20-30: details on the sediment core were already given previously, thus can be omitted here.

15. Throughout the results and all Tables and Figs: Ocean δ^{13}C: sometimes it is labelled $\Delta\delta^{13}C$ (thus changes in the gradient), sometimes deep ocean δ^{13}C in the Southern Ocean. It is not clear to the reader if it always refers to the same things (and is incorrectly named sometimes) or if really two different things are meant here. Please carefully crosscheck (e.g. Tab 1 vs Tab 2 to start with).

16. Fig 14: x axis is reversed with respect to all other figs. Thus, to be consistent I suggest to also let time run from left to right here. Fig 14c is named $\Delta\delta^{13}C$, but is in the description only changes in deep ocean $\delta^{13}C$ in the southern Ocean, and not in the gradient, please revise/correct.

References

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