

Interactive comment on “Impact of brine-induced stratification on the glacial carbon cycle” by N. Bouttes et al.

Anonymous Referee #2

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Impact of brine-induced stratification on the glacial carbon cycle

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I think this study brings new ideas on the glacial lower CO₂ level. By using a brine parameterization, the authors achieve to simulate an oceanic circulation which decouples the top ~2500m from the deepest part. This allows storing carbon in the deep ocean. Although the manuscript mainly deals with the impact of this circulation on the atmospheric CO₂, it is also interesting for this new type of glacial circulation. On the other hand, this study should clearly be considered as a sensitivity study, to be tested by 3D models. Both model and brine mechanism are crude so that it is difficult to me

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to evaluate the realism of this circulation. This manuscript is thus totally relevant to questions addressed by Climate of the Past, and i would recommend its publications. However, i think more informations and discussions are required to let the reader get a clear opinion on the results.

MAJOR COMMENTS

1. I see a potential problem with the brine parameterization. If i understand it correctly, it is designed to account for dilution of brines by surrounding waters, a dilution which is incorrectly simulated by models. High values (>0.5) of the transport coefficient FRAC are required to match LGM target values (eg., Figure 2). I am not a specialist, but I am a bit skeptical that brines could survive a dilution lower than 50% during their move from the surface to the deep ocean. Some hints are given in §2.2 based on studies in fjords, but it is not clear at which depths these salt fluxes have been evaluated and thus if such dilution could apply to a transport into the deep ocean. As argued at the end of §2.2, during glacial, an intense formation of sea ice at the margin of the Antarctic continental plateau may lead to low dilution of brines. However, this is a highly localized mechanism, probably similar to what is described today in fjords (§2.2). In the model, this parameterization is active wherever sea ice forms. This is the case around Antarctica where a larger extent of sea ice is imposed (or calculated?) in the model, but also probably (not documented here) in the North Atlantic and North Pacific. Hence, large amounts of salt are removed from the surface, even from open ocean regions. This probably prevents deep convection and may explain the decrease of the upper ocean (~2500m) ventilation (Figure 4). If this analysis is correct, it is surprising that the South Atlantic ventilation depends so much on deep convection, rather than on Westerlies. My point is that the strong impact on atmospheric CO₂ and oceanic d13C obtained in this study depends on the peculiar glacial circulation, which would merit more discussion. Some GCM studies (eg. Shin et al., 2003) also simulate a large increase in the deep ocean salinity, due to increased sea ice formation, but not a so strong decrease in the ventilation, especially in the north Atlantic. Also, how compares

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this circulation to the one envisioned by Skinner (2009)? I would suggest at least to give more quantitative informations on the glacial circulation, and especially on the Pacific one -which represents by far the largest volume of deep waters-, given that proxies of glacial ocean ventilation do exist to compare to (eg. Lynch-Stieglitz et al., 2007).

2. I am surprised that radiocarbon (^{14}C content of atmospheric CO_2 and DIC) is not used in this study to complement the other tracers. This is a very classic tracer of oceanic ventilation, for which several measurements exist, both for the atmosphere and the ocean at different depths (eg. Galbraith et al., 2007; Skinner et al., 2010). It is very probably implemented in CLIMBER-2, and would really help constraining the peculiar 'decoupled' circulation simulated here.

3. P.686 L.9-11 it is said that "the model compares favourably with a state of the art OGCM and gives the same response in terms of carbon cycle when the circulation is arbitrarily modified (Tagliabue et al., 2009)". It is not clear to me that the different circulations used by Tagliabue et al. (2009) compare with the ones simulated here. Especially, Table 1 in Tagliabue et al. show that decreasing NADW strength does not affect very much the strength of AABW, whereas Figure 4 of the present study clearly shows the contrary. As underlined by the authors, it is mainly the strong decoupling between the upper and lower ocean which helps match glacial proxies. I do not think that such a decoupling has been obtained by other dynamical models: again (cf. point 1) this should merit more discussion.

4. This comment is purely formal and has no impact on the scientific outreach of the study, but there is a confusion on salt and salinity which may be misleading. Salts are species (including gases, in theory) dissolved in seawater. Salinity was defined, before the Practical Salinity, as the mass of salts per unit mass of seawater (see, eg., UNESCO 1981). Hence, DIC and alkalinity contribute to salinity. In this study, 'salinity' is the model active tracer -along with temperature-, whatever its 'salt' composition. Authors should be careful when using these words, especially when writing (p.688,

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l.10): "same process as the release of salt for the other ions", and when opposing salinity to DIC and alkalinity.

MINOR COMMENTS

5. Because of the coarse resolution of the model and the level of parameterization, simulated values are not expected to match very localized measurements. This is why differences (here: LGM-modern) are usually preferred to absolute values. The key Figure 2 shows the success of the brine mechanism to correctly reproduce "LGM data", but to me it would be more credible if modern values were also represented. Further, to complement my point #1, the brine mechanism introduced in this study should also exist in the modern ocean, to a smaller extent (that is, with $\text{FRAC} > 0$ in the model). This would affect the simulated circulation compared to the standard one, with impacts on 'tuning' parameters like vertical diffusivity K_z . With a different set of such parameters, the standard LGM circulation would be different and the glacial carbon cycle as well. Again, I think the brine impact on the oceanic circulation requires a more complete discussion.

6. P.686 L.24-25 : "the atmospheric CO_2 concentration for the radiative forcing (190 ppm, not used in the carbon cycle part of the model" I am not sure what it does mean: if the atmospheric CO_2 concentration is kept constant when calculating the radiative forcing, then carbon cycle and climate are not coupled in the model (as stated in the abstract P.682 L.20-21 and elsewhere in the text).

7. I am quite puzzled by the very high values used for the vertical diffusion coefficient K_z ($2 \cdot 10^{-3}$ to $6.5 \cdot 10^{-3} \text{ m}^2/\text{s}$, Figure 7). These are about 2 orders of magnitude higher than those used in other ocean models, including EMICS (including the Wright & Stocker model which is the basis of CLIMBER ocean). There is probably a typo error in Figure 7, if not this is a problem for the circulation sensitivity. Also, Figure 7 shows that the standard K_z (K_{z0}) increases with depth, whereas the stronger stratification in depth should call for a lower diffusion coefficient. Is there any simple explanation for

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this increase?

TECHNICAL COMMENTS

8. P.682 L.15: salinity AND d18O of water are required to infer density (d18O allows to infer past water temperature)

9. P.682 L.24: "results" do not improve themselves (ie., the simulated distributions), rather these are the brine mechanism and decreased Kz which do improve the distributions.

10. P.682 L.25(and elsewhere) d13C of DIC. A more general comment is that d13C requires more explanation: it should be made clear what isotopic ratio R it is referred to, and especially that LGM data are measured on carbonaceous shells whereas the model simulates the ratio of DIC (and probably not of carbonaceous shells?).

11. P.683 L.3: "-2 to -6°C for the Southern Ocean SURFACE", i guess

12. P.683 L.7: this definition of d13C is not consistent with the values given elsewhere in permil. I beg the authors to use the standard definition which is simply R/Rref -1.

13. P.683 L.14: Dd13C is used in this study as a very specific difference (in the Atlantic, etc), i find this notation very confusing and suggest to add some suffix (_atl for instance).

14. P.684 L.5: "to correctly simulate"

15. P.683 L.16: I am not sure what is meant here, and throughout the text, by 'stratification'. For me, a "stratified deep ocean" means that vertical exchanges are very limited, but Figure 4 shows a quite intense overturning throughout deep Atlantic. Perhaps a more appropriate term could be that deep ocean is 'decoupled' from the upper part?

16. P.685 L.15-16: "increased because of enhanced sea ice formation" It is not obvious to me why sea ice formation would have been increased specifically ABOVE the shelf break. Reconstructions of the glacial extent of sea ice around Antarctica show it larger

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than today, but this does probably not contribute to the brine mechanism which is set up here. An alternative is stronger katabatic winds which would increase sea ice formation right at the shelf break. Maybe simulations of the glacial Antarctic climate could give some hints about this possibility.

17. P.686 L.21: reservoirs

18. P.686 L.26: What is meant by "nutrient"? Does it include DIC? (but not ALK?)

19. P.687 L.13-14: "total salt flux": which flux is it, where is it measured, at which depth; how does it apply to the 'brine mechanism' set up in this study?

20. P.687 L.23 the brines (...do not reach...) OR the brine formation does not affect

21. P.688 L.1: "assess"

22. P.688 L.6: "processes"

23. P.688 L.11: ALK as to be defined. 13C does not exist in seawater, please make it clear which species it refers to. It may be abbreviated as DI¹³C.

24. P.688 L.14&16: make it clear which flux it is (from where to where).

25. P.688 L.24-25: I think there is a confusion here between the specific "brine mechanism" set up in this study, which is the transfer of brines directly to the bottom of the ocean or "brine sink" (controlled by the FRAC coefficient), and the brine formation which exists in standard in the model as well as in the real world. A clarification is necessary in order to identify the different processes and their interplay: writing "FRAC can be set to 0 when no brine is formed" suggests that FRAC controls the volume of brines (salts) formed in the surface whereas it only controls the flux to the bottom cell of the model.

26. P.690 L.5: "and THE MODEL does not include"

27. P.690 L.6: "ATMOSPHERIC CO2". Here and elsewhere CO2 is used as a surro-

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gate to 'atmospheric CO₂' but this is not trivial (it could be the partial pressure of CO₂ in seawater, as in P.692).

28. P.690 L.13: "data value" > observations; measurements; or something else
29. P.690 L.16: "data value of around 37.1 psu." A reference is missing here, I guess this is the estimate by Adkins et al. (2002). The authors should underline that such a high value has only been reconstructed at one site (Shona Rise, 49°S, 3600m), whereas another site at a slightly lower latitude (Chatham Rise, 41°S, 3300m) leads to a reconstruction of a far lower salinity of ~36.2).
30. P.691 L.15: "most of" : please give the approximate range corresponding to Figure 3
31. P.691 L.15-16: "indeed" > in fact
32. P.691 L.18: "The thermohaline circulation is slowed" : this probably depends on the definition of the thermohaline circulation. At least AABW are increased (and maybe also the deep Pacific?), so maybe it should be clarified that the "upper part" of the thermohaline circulation is slowed.
33. P.691 L.28: "by the increased DENSITY"
34. P.692 L.2-4: suggestions: "THEIR surface concentrationS AND thus the biological activity" ... "less ATMOSPHERIC CO₂ is taken up BY THE OCEAN" ... "ADDITIONALLY, the transport of DOC is negligible COMPARED TO THE OTHER MECHANISMS..."
35. P.692 L.7: "direct effect OF SALT SINK"
36. P.692 L.9: "THE distribution of salinity, DIC and ALK..."
37. P.692 L.14: "ATMOSPHERIC CO₂"
38. P.692 L.18-20: such a strong CO₂ change forced by salinity (few ppm to 20ppm) is

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surprising, given that the global glacial-interglacial 1permil change modifies the atmospheric CO₂ by 10ppm. This means that the surface salinity decrease due to the brine mechanism is probably very strong. Again, it would be interesting to get some global figures about this change.

39. P.692 L.29: "Southern convection" should be clarified (ACC?)
40. P.693 L.3+5: "With respect to ATMOSPHERIC CO₂"
41. P.693 L.14, 15, 17: 12C instead of d12C
42. P.693 L.23: nutrients
43. P.694 L.1-2: "maintain their different values": this is a bit confusing since these 'values' are due to competing effects of biological pump vs. mixing, so that surface and deep waters have no 'specific values' of d13C.
44. P.694 L.15-16: a note is required here on what represents diffusion in an ocean model, since it is a completely different process than in the real world, due to the very low spatial resolution of models. "in reality it depends on the vertical profiles": in theory rather, or explain what means 'reality'.
45. P.695 L.13: supports
46. P.695 L.18: existed
47. P.696 L.8-9 (and elsewhere): "becomes stratified" > becomes MORE stratified
48. P.696 L.11: "induced low diffusion": at this stage of the study, the lower diffusivity is prescribed, not induced

FIGURES In general, I find the legends too suggestive and illustrative, and not descriptive enough. A too large part of the legends is a copy of the main text and does not bring new information.

Figure 1: illustrative, but the figure lacks any quantitative information. May be replaced

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by a graph showing both surface and bottom density variations function of TRAC.

Figure 2: clarify which salinity it is referred to; give references for the 'data'.

Figure 3: "S tranSport" ; "ATMOSPHERIC CO2"; here typically the notation Dd13C is not clear enough (see point 13).

Figure 4: "thermohaline circulation" is too vague, what is shown is probably the meridional component of the stream function, in $1E6 \text{ m}^3/\text{s}$ (Sv).

Figure 5: "Oceanic surface pCO2": where, of the global ocean? Drawdown > decrease (drawdown would be for the atmospheric CO2)

Figure 9: "scatter plots" > dots; add 2005 to Curry & Oppo

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