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Climate
of the Past
Discussions

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

N. Bouttes et al.

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We thank reviewer #2 for all his comments. In the following we answer the major, minor and technical comments.

MAJOR COMMENTS

1. Brine parameterization

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

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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 this circulation to the one envisionned 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).

According to observations in the Southern Weddell sea, deep plumes can retain their core characteristics to depths greater than 2000 m (Foldvick et al., 2004). The flow can become occasionally supercritical which acts to limit exchange between

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the plume and its environment. It is thus possible for the sinking water from brine rejection to experience only little mixing while descending to the deep ocean. Yet no mixing at all is very improbable and the maximum value $frac$=1 is extreme and probably unrealistic. Lower values around 0.5 are more plausible and supported by the comparison between model results and proxy data. Indeed, the agreement between model and data for the deep Southern salinity, δ^{13}C (figure 8) and $\Delta 14$C (figure 11) is best for the simulations with $frac$ around 0.5 associated to the low diffusion profile Kz1.

In the manuscript we have added a discussion on the most plausible values for $frac$: "$frac$ values around 0.5 are the most plausible values as they are both supported by the comparisons of model results with data and modern observations. Indeed, comparisons between the modelled salinity and $\Delta\delta^{13}$C with data are in better agreement for $frac$ values around 0.5 with the low diffusion profile Kz1 (figure 8), as well as Δ^{14}C (figure 11). Such a $frac$ value is also close to 0.62 which corresponds to the observed ~62 % of the salt flux rapidly released by sea ice formation that is released out of the fjord in the Norwegian Sea (Haarpaintner et al., 2001)."

As pointed out by the reviewer, sea ice, which is calculated in the model, is an important component of the brine mechanism. A discussion on this matter has been developed in the response to reviewer #1 (question 1d) and a paragraph added in the manuscript on that subject.

The oceanic circulation is indeed modified by the sinking of brines which changes the density distribution in the ocean. The thermohaline circulation which depends on the surface density is hence altered. In the standard glacial simulation the atlantic meridional overturning circulation is slightly changed compared to the pre-industrial (figure 4). The upper branch of the circulation becomes shallower while the lower branch expands and becomes more intense. With the sinking of brines the ventilation of the deep Southern ocean is reduced leading to a decoupling of the deep and surface ocean as pointed out by the reviewer. The upper part of the Atlantic meridional circulation is further shoaled and the lower part expands above 3000 m depth. It is in agreement with the circulation showed in Shin et al., 2003, that also indicates a shoaled and less intense upper branch while the lower branch expands. It also agrees with the hypothesis of the role of a greater volume of bottom water with high carbon content (Skinner, 2009). We show that the deep stratification induced by the sinking of brines allows to isolate and expands the deep ocean which can store a greater amount of carbon, hence lowering atmospheric CO_2.

In the manuscript figure 4 has been changed to include the pre-industrial Atlantic meridional circulation to allow a comparison between the glacial simulations and the modern one. A discussion has been added to describe the simulated oceanic circulation:

“In the standard glacial simulation the atlantic meridional overturning circulation is slightly different from the pre-indsutrial one, with a shallower upper branch and more intense lower branch that penetrates farther north (figure 4 a and b). With the sinking of brines (figure 4 c and d) the enhanced vertical density gradient leads to a more reduced upper branch while the lower branch expands upwards. The ventilation of the lower branch is reduced resulting in a decoupling between surface and deep waters.”

2. Radiocarbon

I am surprised that radiocarbon (14C content of atmospheric CO2 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

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peculiar 'decoupled' circulation simulated here.

The 14C question has also been raised by reviewer #1, hence we takle this issue in the answers to the first reviewer. In the manuscript we have added a discussion on ^{14}C and two figures (figures 11 and 12):

“

Impact of brines and low diffusion on oceanic Δ^{14}C

The oceanic distribution of Δ^{14}C can be modified by both the change of circulation induced by the transport of salinity to the deep ocean and the direct effect of DI^{14}C transport during the sinking of brines. As for δ^{13}C, these two processes have opposite effects on the Δ^{14}C distribution: the change of circulation tends to increase the vertical gradient and lower δ^{14}C in the deep ocean while the transport of DI^{14}C brings DIC with high ^{14}C values from the surface to the bottom and increases the deep Δ^{14}C. The change of circulation is the prevailing effect and the deep Δ^{14}C values become very low (Figure 11 a, c and d). Yet only with very the very extreme and probably unrealistic $frac$ value ($frac$=1) can the circulation capture the increased deep-water ages present in the data.

The low diffusion enhances the vertical gradient as the deep ocean becomes even more isolated. The low data values can then be reached with lower $frac$ values. With very low diffusion profiles (Kz2 and 3, Figure 12), the deep water Δ^{14}C become too low showing that the diffusion should be lowered but not as much.”

3. comparison with OAGCM

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.

The simulations presented in this study are effectively different from the ones in Tagliabue et al., 2009, as justly highlighted by the reviewer. However the simulations involved in the comparison between CLIMBER-2 and the OAGCM are not the ones described in this study but the ones done with CLIMBER-2 that were presented in Tagliabue et al., 2009. They were realised with a similar forcing as for the OAGCM, i.e. by adding fresh water flux to the north atlantic in order to decrease the thermohaline circulation ans study the impact of a lower circulation on the carbon cycle. In the results the simulated d13C vs NADW intensity are similar in both models (figure 3 in Tagliabue et al., 2009). Similar deep-decoupled circulations as the ones obtained in this study were also obtained in OGCMs, as in Shin et al. (2003) for instance, but these simulations were not analysed to our knowledge in terms of the carbon cycle.

4. Salt and Salinity

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

"Salinity" and "salt" have indeed been used without enough caution. Following the reviewer's advice we have replaced salt by salinity in the manuscript when it refers to the tracer simulated in the model.

MINOR COMMENTS

5. Modern values

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 FRAC>0 in the model). This would affect the simulated circulation compared to the standard one, with impacts on 'tuning' parameters like vertical diffusivity Kz. 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.

As suggested by the reviewer modern values both from data and model simulations have been added to Figure 2. Concerning the sinking of brines in the modern ocean, it is probable that it should play a role, although minor compared to the glacial period. Indeed, the possibility for the very dense water rejected by sea ice formation to sink to the deep ocean greatly depends on local conditions, especially the topography. The latter is very different around Antarctica between modern and glacial periods. During the glacial period the Antarctic expands and covers the continental shelves. Simulatneously, because of the ice sheets shrinking the sea level fells. It results in a reduction of the volume of water above smaller continental shelves. The salty water is less mixed with surrounding water and can more easely sink along the continental slope. In modern conditions the continental shelves are bigger and the sea level higher resulting in a greater volume of water above the continental shelf. The salty water is then more mixed with the fresher surrunding water and is trapped at the bottom of the continental shelf. Hence as a first approximation we consider $frac$=0 for the modern ocean.

6. CO$_2$ for the radiative code

P.686 L.24-25 : "the atmospheric CO2 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 CO2 concentration is kept constant when calculating the radiative forcing, then carbon cycle and climate are not coupled i the model (as stated in the abstract P.682 L.20-21 and elsewhere in the text).

The CO$_2$ concentration used in the radiative scheme is not the one calculated in the carbon cycle model in order to obtain a coherent glacial climate and avoid complications, as done in previous studies (Brovkin et al., 2007; Bouttes et al., 2009). Hence the model is not fully coupled between the climate and the carbon cycle, it is only semi-coupled (the climate impacts the carbon cycle, but not the inverse). When the glacial CO$_2$ level is finally simulated it will be possible to fully couple the climate and carbon cycle. However it is worth noted that it is coupled within the carbon cycle itself, i.e. the three carbon reseroirs (ocean, atmosphere and terrestrial biosphere) are fully coupled, which is not always done in carbon cycle models.

In the manuscript we have deleted the reference to coupled climate carbon model as CLIMBER-2 is in fact used as a semi coupled model.

7. Vertical coefficient diffusion

*I am quite puzzled by the very high values used for the vertical diffusion coefficient Kz (2.E-3 to 6.5E-3 m**2/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 Kz (Kz0) increases with depth, whereas the stronger stratification in depth should call for a lower diffusion coefficient. Is there any simple explanation for this increase?*

There is a mistake in the units of the coefficient in Figure 7: it should be cm and not m. It has been changed in the mansucript.

The standard profile (Kz0) presented in figure 7 is the original profile of the model (Petoukhov et al., 2000). The deep profile increasing at depth is indeed puzzling, but the important part of the study relies on the difference between the profiles. A better modelling of the diffusion profile would parameterize the diffusion coefficient depending on the vertical density gradient (Marzeion et al., 2007). We have tested such a parameterization in CLIMBER-2 and preliminary results indicate that the oceanic meridional circulation does not change in the standard preindustrial and LGM

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simulations.

TECHNICAL COMMENTS

8. P. 682 l. 15

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

δ^{18}O inferred temperature has been added to the text: “The existence of such very dense waters has been inferred in the LGM deep Atlantic from sediment pore water salinity and δ^{18}O inferred temperature.”

9. P. 682 l. 24

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.

The last sentence of the abstract has been changed to: “The modeled glacial distribution of oceanic δ^{13}C as well as the deep ocean salinity are substantially improved and better agree with reconstructions from sediment cores, suggesting that such a mechanism could have played an important role during glacial times.”

10. P. 682 l. 25

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?).

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The δ^{13}C from the model is the one of DIC, the δ^{13}C from the data is measured from foraminifera shells. The latter reflects the δ^{13}C of the water in which this foraminifera has lived (Duplessy, 1984). This precision has been added to the manuscript: “The δ^{13}C, which is measured from the shells of foraminifera, reflects the δ^{13}C of the water in which this foraminifera has lived (Duplessy, 1984).”

11. P. 683 l. 3

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

It is indeed the surface ocean. It has been added to the text: “in the Southern Ocean surface”.

12. P.683 l. 7

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.

We use this definition of δ^{13}C because the $\times 1000$ coefficient accounts for the traditional unit used (‰).

13. P.683 l. 14

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).

As suggested by the reviewer we have replaced δ^{13}C by δ^{13}C$_{atl}$.

14. P.684 l. 5

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

“correctly” has been added.

15. P.684 l. 16

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?

Figure 4 shows that globally the meridional overturning circulation in the Atlantic is reduced with a higher $frac$ parameter and less exchange happen between intermediate and deep waters. The ocean is stratified because deep waters become denser and surface-intermediate waters lighter (which is now on Figure 1). As suggested by the reviewer, the ocean is indeed decoupled between the surface and the deep ocean. As explained in more details in question 1 we have added a discussion on the atlantic meridional circulation and the decoupling in the manuscript.

16. P.685 l. 15-16

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 t larger than today, but this does probably not contribute to the brine mechanism which is set up here. An alternative is stronger katabatik 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.

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Because of the glacial Antarctic ice sheet extent, sea ice formation happens closer to the break shelf. Besides, reconstructions of sea ice extent around Antarctica not only show a larger extent, but also a greater seasonality as the winter extent is larger but the summer one is not very different from the modern one (Gersonde et al., 2005). This greater seasonality should lead to a greater volume of released brines. As suggested by the reviewer the katabatik winds can indeed also play a role as it enhances the rate of sea ice formation and increases the amount of released brine.

17. P.686 l. 21
P.686 L.21: reservoirs

“reservoir” has been replaced by “reservoirs”.

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

“nutrient” refers to NO_3^- and PO_4^{2-}. It has been added to the manuscript: “nutrients (NO_3^- and PO_4^{2-})”

19. P. 687 l. 13-14
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?

The salt flux is referring to the the salinity rejected by sea ice formation. This sentence can be misleading and has been erased.

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

As suggested by the reviewers it has been replaced by “the brine formation does not affect”.

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

It has been done.

22. P. 688 l. 6
"processes"

It has been corrected.

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 DI^{13}C.

The ALK acronym has been precised in the manuscript and ^{13}C replaced by DI^{13}C: “dissolved inorganic carbon (DIC), alkalinity (ALK), nutrients, dissolved organic carbon, oxygen, DI^{13}C

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

It is the flux corresponding to the formation of sea ice, i.e. a flux from the new ice to the surface ocean. It has been added in the manuscript: “The flux (F_X) of any geochemical variable X rejected during sea ice formation to the surface ocean is then”

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 mode 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 vo ume of brines (salts) formed in the surface whereas it only controls th flux to the bottom cell of the model.

The sentence can indeed be misleading as the rejection of brines is always taken into account in the model contrary to the sinking of brines which is the subject of the study. It thus has been changed to the following sentence: “This fraction $frac$ can be set to 0 when none of the salt sinks to the bottom of the ocean (standard version of the model) and 1 when all the salt is used in the brine mechanism.”

26. P. 690 l. 5

"and THE MODEL does not include"

It has been done.

27. P. 690 l. 6

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"ATMOSPHERIC CO2". Here and elsewhere CO2 is used as a surrogate to 'atmospheric CO2' but this is not trivial (it could be the partial pressure of CO2 in seawater, as in P.692).

To make it clearer when it refers to atmospheric CO_2, CO_2 has been replaced by atmospheric CO_2 here and elsewhere.

28. P. 690 l.13

P.690 L.13: "data value" > observations; measurements; or something else

“The simulated oceanic δ^{13}C is also far from the data value” has been replaced by “The simulated oceanic δ^{13}C distribution is also far from the reconstructed one.”

29. P. 690 l.16

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, 49S, 3600m), whereas another site at a slightly lower latitude (Chatham Rise, 41S, 3300m) leads to a reconstruction of a far lower salinity of 36.2).

As noted by the reviewer the reference is missing, so we have added it with precisions on the core: “data value of around 37.1 psu measured in a sediment core (Shona Rise, 49°S, 3626 m depth) (Adkins et al., 2002)”.

The salinity reconstruction of Chatham Rise is indeed lower (36.2 compared to 37.1) though still high. But this site, in addition to being at a lower latitude and less deep, is situated in the Pacific basin, not in the Atlantic. In the simulations it corresponds to a transition zone (cf Figure 1) and the simulated salinity values simulated are generally in agreement with the data, so that it can not rule out most of the scenarios contrary to the salinity in the Atlantic (as shown on figure figures 2c and 8c of the manuscript and Figure 2 here) which is correctly simulated in fewer cases.

30. P. 691 l.15
"most of" : please give the approximate range corresponding to Figure 3

The approximate range has been added: “it accounts for the largest part of the entire brine induced drop (approximately 60 %, Figure 3)”.

31. P. 691 l.15-16
"indeed" > in fact

Indeed has been erased.

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.

To clarify, as suggested by the reviewer we have added “upper part”: “The upper part of the thermohaline circulation”. Yet the lower part is not increased, although the volume of this lower part is greater, as discussed in question 1 and in the related

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paragraph in the manuscript.

33. P. 691 l.28
"by the increased DENSITY"

“salinity” has been replaced by “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..."

The paragraph has been modified following the reviewer’s advice.

35. P. 692 l.7
"direct effect OF SALT SINK"

It has been added as suggested.

36. P. 692 l.9
"THE distribution of salinity, DIC and ALK..."

The sentence has been modifed as follow: “the distribution of salinity, DIC and ALK in the ocean is modified (especially the repartition between surface and deep waters)”

37. P. 692 l.14
"ATMOSPHERIC CO2"

“atmospheric” has been added.

38. P. 692 l.18-20
such a strong CO2 change forced by salinity (few ppm to 20ppm) is 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.

The change of 20 ppm of CO_2 due to the change of salinity in the surface corresponds to $frac$=1 which is extreme and probably irrealistic. With lower $frac$ values around 0.5 the CO_2 change is much less, around 8 ppm. As the salinity increases in the deep ocean it decreases in the surface (figures 2c and 8c in the manuscript and figure 2 here). The salinity change better agrees with the data for a medium value of frac around 0.5 and a lower vertical diffusion (Kz1). A higher $frac$ value or a vertical diffusion too reduced (Kz2 and Kz3) leads to salinity levels too high compared to the data.

39. P. 692 l.29
"Southern convection" should be clarified (ACC?)

The formation of Antarctic Bottom Water (AABW) is reduced in the simulations. It has been clarified in the manuscript as follow: “Southern convection (formation of Antarctic Bottom Water, AABW) is greatly reduced”

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40. P. 693 l.3+5
"With respect to ATMOSPHERIC CO2"

CO_2 has been replaced by oceanic pCO_2 as it refers to the oceanic changes observed on Figure 5.

41. P. 693 l.14, 15, 17
12C instead of d12C

δ^{12}C has been replaced by ^{12}C.

42. P. 693 l.23
nutrients

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.

To avoid confusion the sentence has been replaced by: "Hence the effect of the biological pump becomes more important compared to the mixing, deep waters have lower δ^{13}C while surface waters have a higher δ^{13}C and the vertical gradient increases."

44. P. 694 l.15-16

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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'.

In oceans models, because of the low resolution vertical diffusion is parameterized and it is often set to a constant value in time or space. However this is a simplification which does not take into account the spatial or temporal variations. In particular, stratification influences diffusion because more turbulent kinetic energy is required to move water across strong vertical density gradient (Gargett and Holloway, 1984). To take into account the vertical gradient a vertical profil of the diffusion can be used. But as the stratification can change due to variations of the density distribution the vertical diffusion profile should also change. To test the impact of such a change we modify the vertical diffusion profile. Further studies would require a parameterization of the diffusion in link with the density gradient (Marzeion et al., 2007). In the text “reality” has been replaced by “theory” as suggested by the reviewer.

45. P. 695 l.13
supports
Ithas been corrected.

46. P. 695 l.18
existed

It has been done.

47. P. 696 l.8-9

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"becomes stratified" > becomes MORE stratified

"becomes stratified" has been replaced by "becomes more stratified".

48. P. 696 l.11
"induced low diffusion": at this stage of the study, the lower diffusivity is prescribed, not induced

"induced" has been erased.

FIGURES

Figure 1
illustrative, but the figure lacks any quantitative information. May be replaced by a graph showing both surface and bottom density variations function of TRAC.

To make the figure more quantitative as suggested by the reviewer, we have added a graph showing the variation of the density difference between the deep and surface atlantic ocean as a function of $frac$ (Figure 1), wich illustrates the enhanced stratification induced by the sinking of brines.

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

We have specified which salinity is considered and added the data references: "salinity in the deep Southern ocean (Atlantic sector, 50 degrees South, 3626 m depth) as a function of the fraction of salt released by sea ice formation used for the brine mechanism (fraction of salt $frac$, $0 \leq frac \leq 1$).(...) Data are from Monnin et al., 2001; Curry and Oppo, 2005 and Adkins et al., 2002."

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

"S transport" has been corrected. "Atmospheric" has been added to CO_2 and $_{atl}$ to δ^{13}C.

Figure 4
*"thermohaline circulation" is too vague, what is shown is probably the meridional component of the stream function, in 1E6 m**3/s (Sv).*

"thermohaline circulation" has been replaced by "meridional overturning stream function".

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

It is the mean surface pCO_2 for the global ocean. It has been made more explicit in the caption: "Mean global ocean surface pCO_2 decrease". "drawdown" has been replaced by "decrease".

Figure 9

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

“scatter plots” has been replaced by “dots”. 2005 has been added to Curry and Oppo.

Interactive comment on Clim. Past Discuss., 6, 681, 2010.

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Interactive Discussion

Discussion Paper

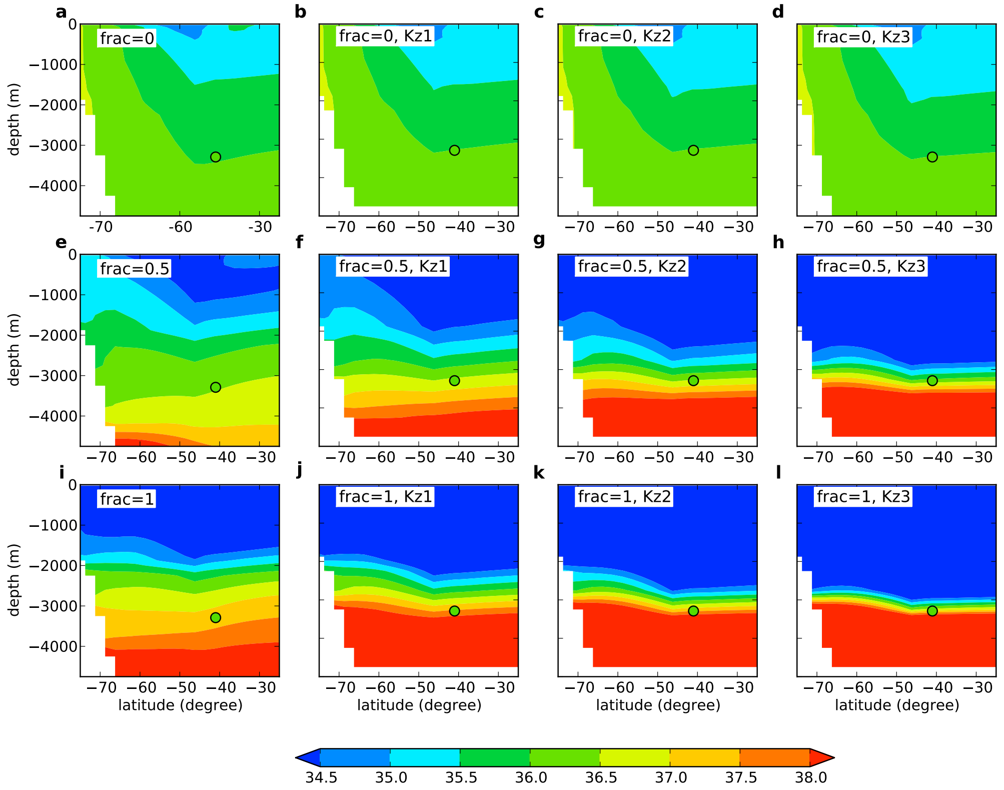

Fig. 1. Salinity distribution (permil) in the Pacific ocean as simulated by the model. The dot is the data from Adkins et al., 2002 (Chatham Rise, 41S, 3290 m).

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Interactive Discussion

Discussion Paper

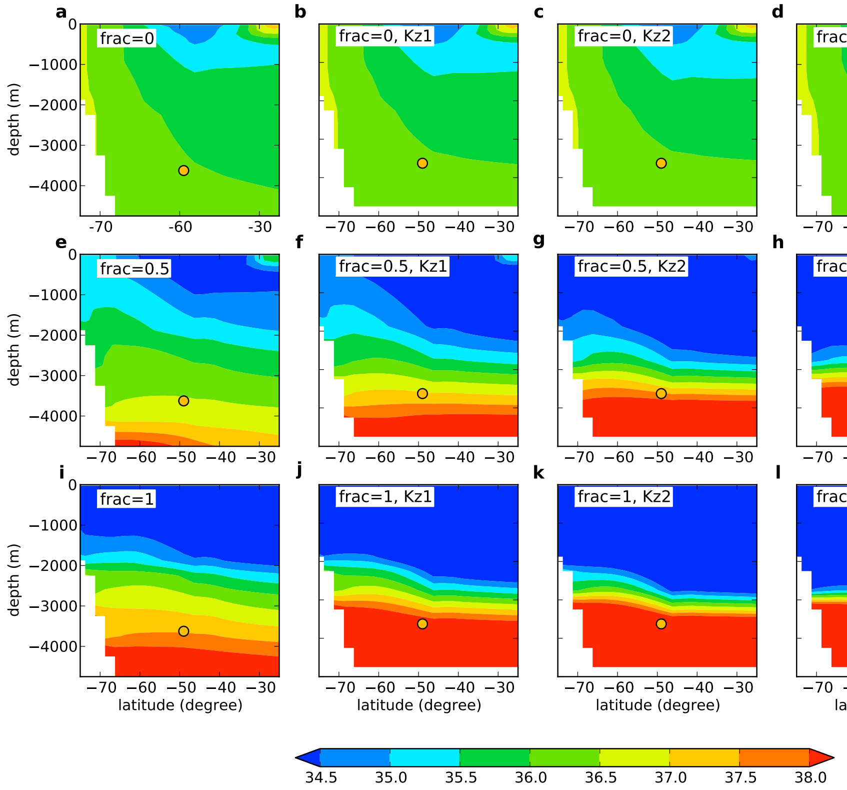

Fig. 2. Salinity distribution (permil) in the Atlantic ocean as simulated by the model. The dot is the data from Adkins et al., 2002 (Shona Rise, 49S, 3626 m).