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Climate of the Past Discussions

Interactive comment on "Glacial marine carbon cycle sensitivities to Atlantic ocean circulation reorganization by coupled climate model simulations" *by* M. O. Chikamoto et al.

Anonymous Referee #2

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Major comments

This study evaluates the effect of changes in ocean circulation, sea ice and solubility in a GCM under LGM climate conditions, on the carbon cycle and pCO2 in an offline biogeochemical model. Previous studies on this topic have typically used box or intermediate complexity models, so the study is timely and will be of interest to the readership of Climate of the Past. I think this study should be published, but I have some comments on the interpretation of the results that must first be addressed.

In general I felt that the experiments merit a better analysis analysis and discussion. Sections 3 to 5 are very descriptive and a bit listy, and would benefit from a more careful guiding of the reader to the main findings of this study. Furthermore I felt that some of the most interesting questions that this study could address were not properly discussed. For example, the discussion of circulation changes and carbon storage at section 5.1 is probably the most exciting aspect of this GCM study and this section does not do it justice. For instance, the topical question of the balance between increased AABW formation vs. Southern Ocean stratification in controlling pCO2 is not properly discussed. I also wanted to know more about the causes of the circulation changes; AABW changes do not have to be driven by NADW changes. The discussion of preformed nutrient contents should be expanded to consider potential changes in productivity in source regions, for instance as a result of Fe fertilisation (see Hain et al. 2010).

I also felt the discussion of pCO2 changes due to sea-ice, another major aspect of this study, was lacking. I wanted to know more about "capping" of CO2-rich waters by sea-ice, a topical question which has had relatively little testing. How is permeability parameterised? And biological productivity? And how sensitive are some of these parameters, and thus why are pCO2 changes (for LG-is) less dramatic than those of Stephens and Keeling.

1274, 18: The fact that the solubility experiments don't include CO2 solubility change due to whole ocean salinity increase is an important caveat, so be more clear with this discussion, and in the initial description of the factorial solubility experiments. This does seem a significant shortcoming of the solubility factorial experiment, though it is hard to think of a way to include this effect without potentially changing circulation - does circulation show a significant change when a uniform salinity increase is applied? And how is the 10 ppm increase (line 19) assessed? However it is interesting that the PI-sl and LG-sl experiments give values in such close agreement: given that PI-sl has LG SSS but PI interior sal, and that LG-sl has PI SSS, but LG interior sal, it seems that the effects of salinity on air-sea gas exchange vs. whole ocean CO2 solubility are roughly equal. This should be discussed.

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The same argument applies to the circulation experiments, where the switch from PI to LG circulation states will include +10 ppm pCO2 due to increased salinity. This masks the true effect of 'circulation', in terms of changing water mass dominance and movement. For instance when comparing experiments LGb-oc (PIb circ) to LGb (LGb circ), the switch to LGb circulation will also include +10 ppm pCO2 increase due to higher salinity. As the pCO2 difference between the experiments is 264 –> 260 (-4 ppm), yet the salinity increase should have raised pCO2 by +10 ppm, the effect of this circulation change is really -14 ppm. Investigating the effect of circulation change on pCO2 is a major aim of this paper, so this issue should be considered for each circulation state, and these results discussed.

1276, 24: this analysis of surface DIC, ALK, pH and CO2 is interesting, but I don't think the most interesting aspects of it are fully discussed, and the current interpretation seems at odds with the other experiments. The fact that the high surface ocean pCO2 at high latitudes under LGb circulation does not lead to significantly increased atmospheric CO2 is attributed to sea ice cover. However if glacial sea ice is the key factor in preventing high latitude CO2 escape, then we would expect that reduced (PI) sea ice cover with LGb circ, as in experiment LGb-in, should cause an increase in atmospheric pCO2. Instead, the opposite effect is observed, with reduced sea ice causing a decrease in atm pCO2. This highlights the main point that these figures seem to make: that surface pCO2 in these zonal averages is a relatively unimportant predictor of atm pCO2 change. The most important factor controlling atm pCO2 seems to be the reduced surface DIC (and increased deep DIC storage), not the increased surface pCO2.

13C and 14C provide some of our best constraints on LGM circulation and carbon cycling. These tracers are included in the model (1265, 21) and should be plotted and compared to the data to allow evaluation of the LGb state.

Be more clear in distinguishing LGa vs. LGb experiments and "glacial conditions".

Points arising in text

Abstract

11: what reconstruction of DIC? Only d13C of DIC exists, or hypotheses about DIC gradients.

1. Introduction

This section would benefit from a more clear description of how circulation change may cause CO2 change. In particular it would be good to explain the concept of preformed nutrients vs. DIC in different deepwater formation regions; and spell out hypotheses involving different Southern Ocean processes, like the potential importance of the biogeochemical divide (e.g. Marinov 2008), and the potential difficulty of having a more stratified Southern Ocean, yet more AABW formation. Clear description of these hypotheses here would also benefit the discussion.

1262, 25: There are several candidates (or There is likely to be more than one cause of...)

1263, 15: Saltier bottom waters don't necessarily imply reduced ventilation. Instead could discuss that increased vertical salinity gradient may imply increased stratification (see Lund et al. 2011) or that 14C implies reduced ventilation (e.g. Skinner 2010).

1263, 17: be more clear - is it the increased density, or increased volume, or both?

1263: reference data that show evidence for an altered glacial circulation regime; almost all papers referenced are modelling studies. Obvious choices would be Curry and Oppo 2005 or Lynch-Steiglitz 2007.

1264, 7: would be good to state explicitly that sea-ice may make CO2 go up or down and that you will test this.

1264, 11: reference a box model study. In particular would be good to reference, and compare results with the study of Hain et al. 2011, which also pulls apart different

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components of glacial CO_2 change in a box model.

1264, 14: outin –> out in

1264, 21: future carbon cycle is not discussed, so don't mention here.

2.

1265, 21: 13C and 14C are mentioned here - sections of these tracers should be shown.

1266, 10: would be good to discuss why this warm bias might occur

1266, 26: is it the cooling itself, or brine-rejection, or P-E changes, that leads to increased AABW formation?

1267, 4: prognosticate doesn't seem the right word - it typically means to foretell or prophesy. Maybe diagnose?

1267, 5: considering a closed system carbon cycle, the difference between these experiments is interesting in its own right - 400 GtC is 2/3 the PI atmosphere. How big a change in CO_2 is seen if the cooling of 'b' is applied to an 'a' experiment with non-restoring CO_2?

1267,11: how long are factorial experiments run for?

1267, 14: would be helpful to the reader to insert the sentence at line 1268, 6 in here. Likewise move the sentence at 1268, 28 to line 1268, 11.

1267, 15: make it clear here, as discussed in 4.1, that PI SSS is only used in the air-sea gas exchange parameterisation and that the whole ocean salinity is LG.

1269, 2: would be more clear to say at the start of section 2.3 that you do the factorial experiments for the a and b configurations, and then drop these letters from the rest of the description (i.e. "LG-sl is identical to LG, ...")

3.

1269, 13: this is confusing - surely slower settling should give shallower regeneration, and result in higher nutrient contents in the upper ocean? Is this a far-field effect?

1269, 19: or remineralisation.

1269, 20: change "less affects"

1269, 24: any idea why the fluxes are larger than observations?

1270, 7: 'lower' or 'more negative' would read easier than 'smaller'

1270, 12: good that preformed nutrient content is assessed; would be good to include more in discussion of LG results.

1270, 28: 'equal' would be better than 'uniform'

Section 3.2 is not well structured - jumps between different parameters and experiments.

1271, 10: 'minimum' seems an odd word choice - the flow presumably this means maximum flow, but with the Northward direction giving it a negative value? I think maximum would be more clear.

1271, 23: how does this compare to pore water estimates (Adkins et al. 2002)?

1272, 7: I don't understand the statement about DIC 'delivery' - why does forming more low DIC water in the Atlantic mean that Pacific water DIC should increase? As an alternative, perhaps NPIW formation is more sluggish, allowing more DIC to accumulate in this region?

1272, 10: would be good to also have the equivalent basin DIC inventories quoted for LGb, as for the LGa experiment. This would also clarify the statement that 'carbon inventories are reduced': if the total DIC inventories are reduced in each basin, then (given a closed ocn-atm system) atmospheric CO2 must have risen?

1272, 19: why do nutrients increase in upwelling zones? In the Pacific this seems likely to be related to higher nutrient concentrations in the subsurface, and is likely to be linked to the increased intermediate depth DIC.

1272, 20: change to 'nutrients are supplied from'

1272, 24: to what region does this export flux production apply?

1272, 27: by what processes does reduced NADW lower surface nutrients? Could this not be due to reduced mixing of nutrients trapped in dense AABW, or less nutrients in better ventilated intermediate waters?

1273, 3: reference a data compilation (i.e. Kohfeld et al. 2005) as well as model studies.

1273, 3: confusing that increased air-sea flux actually means increased sea to air flux - make this clear.

1273, 10: I don't think that these changes in preformed nutrient fraction or the ~20 ppm changes in pCO2 should really be classed as "considerable". Also, although I like seeing the changes in preformed nutrient fraction, I don't think these should be classed as strictly "biological". They could be achieved with fixed productivity regimes, but by forming more deep water through the inefficient Southern Ocean pathway relative to the efficient North Atlantic (e.g. Toggweiler et al. 2006).

4.

1274, 4: true that the response is non-linear. However this is not in itself an explanation of why LGb is more sensitive. These variations in sensitivity under different boundary conditions are an interesting feature of this study and deserve more discussion.

1274, 18: see major comments

1274, 25 c.f 1275, 9: the change in Atlantic DIC sections is, if anything, larger for sl than in (8a&e vs. 8b&f) so the change for sl shouldn't be described as "slightly different" and that for in as a "large difference".

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1275, 12: the phrase "exports more carbon" is, again, not clear to me. I think you mean that DIC in NADW is reduced, AND as NADW is more dominant, whole ocean DIC is therefore reduced (and pCO_2 is higher).

1275, 18: are these effects really totally equivalent? How is this assessed?

1275, 24: "In contrast to" rather than "Compared with"

1276, 16: as mentioned previously, when the effect of whole ocean salinity change is accounted for, the circulation effects shown in Fig 7 go 10 ppm pCO_2 lower. The pCO_2 change due to LGb circ becomes -14 for LG conditions, and -3 for PI conditions.

1276, 24: see major comments

5.

5.1: see major comments

1279, 2: simultaneous DIC and ALK changes are to be expected. Also surface pCO_2 seems to increase, not decrease.

1279, 13: deep water is never stagnant - it just switches to AABW which has lower ALK/DIC resulting in lower $CaCO_3$ saturation state. Bottom water overlying deep water does not make sense.

1279, 16: this paragraph could be shortened. It would be better to briefly mention these factors and focus on things that the model results presented here can address. If carbonate compensation is to be discussed, then an estimate of the magnitude of this effect should be included.

1280, 6: Toggweiler 2006 would be a better reference for "proposing" that the Westerlies may cause changes in atmospheric CO_2.

1280, 17: quantify "does not regulate atm pCO_2 much". This is a topical and interesting discussion and could be expanded.

6.

1281, 5: again, what vertical DIC gradient?

1281, 8-11: does not seem to be a major conclusion of this study.

1281, 22: simultaneous changes in ALk and DIC are not unusual; unknown changes in rain ratio and particle settling rates do not have to be invoked.

Figures and Tables

Table 1: this table is very helpful. It would benefit from a caption briefly describing the experiments, in particular the parameters change in "solubility" and "circulation"

Figure 6: state if plots are for annual averages

Figure 7: ref to table 2.

Interactive comment on Clim. Past Discuss., 7, 1261, 2011.