Throughout this document the original referees comments are shown in regular font, while our response is in bold.

<u>Response to Editor</u>

Thank you for the prompt handling of the manuscript and the thoughtful comments, the paper has improved considerably as a result of the input.

The most important point here is that referee #2 is criticising the fact that your model runs are based on interglacial boundary conditions. This is also seconded by the SC and has been mentioned in my initial editorial response after your submission. Referee #2 and the SC make a convincing point that due to this, the effects of a shut down of the AMOC in your model are most likely overestimated.

In our revised manuscript we provide arguments that the effects MAY be overestimated but we don't think that they are MOST LIKELY overestimated. Such an assessment (most likely overestimate) would require knowledge of the state (both physics and carbon cycle) of the LGM ocean, which unfortunately remains elusive.

In view of this criticism, there are two ways forward: The first (and clearly from an editorial point of view preferred) possibility is that you redo your model runs based on glacial boundary conditions. If that is not at all possible, the minimum requirement to go forward is that you extensively discuss this limitation and the potential effects this limitation has on your results and to compare your work with other modeling studies using interglacial and especially glacial boundary conditions.

As we mentioned in our initial response the state of the circulation and carbon cycle of the LGM ocean is poorly constrained; no 3-D model simulation exists to our knowledge, that is consistent with the ocean interior observations/reconstructions including carbon isotopes. Until this situation changes we cannot perform simulations from realistic initial conditions. Whereas simulations with glacial boundary conditions (lower GHG, orbital parameters, ice sheets) are easy and have been done before (e.g. in PMIP) their value is low as long as they are not assessed by observations.

In short, realistic initial conditions are not available at this time. Hence, we have to resort to your option 2, which is an in depth discussion of this important point. This is now provided in the revised manuscript's discussion and conclusion section. We have also re-written the abstract and title in order to put more emphasis on the robust conclusion that the AMOC was reduced and to more clearly outline the model-data differences and uncertainties with respect to the CO2 response.

The latter is also required, as referee #2 criticises the lack of an in-depth discussion of previous work or in one case its misinterpretation (work by Tschumi).

We don't agree that we have misinterpreted the work by Tschumi. Please refer to our response to the referee, who cites a different paper (Tschumi et al. 2008) than the one we were referring to (Tschumi et al. 2011).

However, although the scope of our manuscript is not an extensive review of modeling of AMOC or Southern Hemisphere wind changes on CO2, in our revised manuscript we now provide almost all the additional references suggested by the referees.

I assume that most of the other points raised by the 2 referees can be accomodated relatively easily.

From an editor's perspective I have another request, that can be easily accommodated. To give the paper the full credit and to help other scientists in this field, you should provide your d13C data compilation in a supplement to the paper together with the used age scales (as also requested by referee #1).

We have produced a supplement including all the sediment data plus some selected model output. The file is 15 MB and available here: http://people.oregonstate.edu/~schmita2/data/schmittner14cp/schmittner14cp_sup.tar.gz

Response to Anonymous Referee #1

In this paper, the authors compare atmospheric CO2 and d13C, and foraminifera-based ocean d13C trends, from the very latest last glacial maximum (18.5-19.5kyr BP is used) into the last Heinrich event (16.5-15.5kyrBP is used) to the d13C trends predicted from hosing experiments using the pre-industrial control of the intermediate-complexity UVic model with MOBI imbedded. For the two weaker hosing experiments, the AMOC recovers immediately after the fresh water forcing is over. For the two strong experiments, the AMOC essentially collapses and remains in the collapsed state even after freshwater forcing is removed. Thus, only the early phase of the "deglacial minimum" is evaluated, not the d13C recovery.

I am favorably impressed with the manuscript although I do have several minor comments that can be easily addressed by the authors. Importantly, while matching the data reasonably well, it achieves the initial deglacial CO2 decrease by a change biological cycling in response to a large AMOC decrease, without changing southern ocean winds. I think this paper is also an important step towards understanding the evolution of deglacial d13C signals. While not perfect, if shows reasonable matches to carbon isotope signals. It also shows that a source of isotopically depleted carbon is not needed to explain low d13C values in the mid-depth South Atlantic, as suggested might be the case by Tessin & Lund, 2013.

In Intro, there is heavy reliance on Pa/Th records for setting the stage (p2 lines 24-35). Also, page 9: the C13 data are matched best by complete cessation for many millennia, providing support for the McManus record. On the one hand McManus et al data suggest complete cessation, but on the other, Gherardi et al data suggest unlikely. Several studies show that Pa/Th strongly affected by particle flux and composition. The observation of highest production ratios during HS1 may be indirectly caused by a large reduction in the AMOC, through its affect on other parameters.

Methods: While I agree that a 1ka uncertainty is not a problem for this sensitivity study, I do think that age models of records not previously published should be provided. Age control points can be added on Figure 6. If radiocarbon reversals occur over the study interval, that can be noted on the core location table.

All age models are from published data except one core from the Brazil Margin (90GGC), which is part of a manuscript that is currently in review (Lund et al., Paleoceanography). This manuscript had positive reviews and we expect it to be accepted for publication soon. For this reason we do not include a detailed description of the data here. However, if the reviewer or the editor feel we should do this please let us know and we will reconsider. Also, we chose not to include age markers in Figure 6 as it is already quite busy and additional symbols would make the figure cluttered with different symbols.

P5 line8. Here, high C13 in NADW is attributed to "well-equilibrated surface waters". Although it is somewhat implicit in the next sentence I think it should be clarified that

NADW source waters are relatively depleted in nutrients - AAIW, with higher d13as is a better example of a water mass with well-equilibrated source water.

Thanks. We've modified the sentence accordingly.

Line 13: I'd add: (Fig4D-4) "as suggested previously". Boyle and Keigwin, 1982 might have been the first to suggest that the North Atlantic- Pacific gradient reflect the AMOC. Most recentlysuggested by Yu et al., 2014 EPSL.

We cite Boyle and Keigwin but could not find the Yu reference.

Line22: I think best to be cautious about saying this might explain the d13C minimum. The focus of this paper is the early deglacial d13C decrease. It does not deal with the whole planktonic d13C minimum, which generally well outlasts the interval of dramatically reduced AMOC, and whose end may have other causes.

Good point. We have rephrased the sentence to refer only to the *onset* of the planktonic d13C minimum.

Lnes25-28: I would be interested in more details on the mechanisms of these changes.

We reference now Saenko et al. (2004), who examine the Atlantic-Pacific seesaw of ventilation in this model in detail.

P6 line11 Not clear to me what is meant by "illustrating the local effect of freshwater forcing".

We have rephrased this sentence to make it clearer.

The metric used to compare data and model is the correlation between the difference at model CTL-year 2500 and the difference between HS1 and latest LGM. Because the evolution of the signal is in some cases quite different in the data and models, it may also be valuable to use a metric that takes evolution into account.

We now include other metrics (rms error, bias, and ratio of standard deviations) none of which, however, considers evolution.

Is the ability to do this compromised by poor age control?

Yes, in order to do this right error estimates for the age models would be needed, which are not available currently. Such estimates require substantially more time and effort than we have for the revision of this manuscript.

Is this a likely reason for the poor temporal match in MD97-2120, NIOP905, 17JPC? Or does the model simply not simulate the evolution well?

This is difficult to say. My guess would be both. But, as mentioned above, a proper analysis of the temporal evolution would require error estimates on the age models.

We have included a note on the temporal mismatch with MD97-2120 and NIOP905.

I understand that in some cases the poor temporal correlation is related to the modern control. Might this be often the case, and if so, why does the value after 2500 years show a correlation?

Again, it is difficult to say in which cases the modern control causes biases. The choice of 2500 years is subjective but we don't think the results depend strongly on this choice. Given the approximate errors in the age models of ~1,000 years we want to avoid a time period close to the transitions. In some of the records (e.g. panels A, B, E, P of Fig. 7) the changes seem to be later than in the model, but most of those differences are probably within the error of the age models. A more systematic analysis would require age model error estimates.

P7, top. more detail on these processes might be helpful.

I'm not sure which processes the reviewer refers to.

Page 8, line 1, typo "largest" effect

Thanks. Corrected.

Page 8 line 16. Since the time evolution of the signal is not well simulated, perhaps be more specific. The d13C difference after 2500 years is similar to the late LGM-late HS1 difference?

We have rephrased this sentence accordingly.

Line 23 – not clear to me that there is robust evidence for a weaker LGM circulation although many believe that to be the case.

We agree. Our hypothetical phrasing of the sentence is inteded to highlight the uncertainty in the LGM circulation.

Page 9 – would be useful to highlight where (in space) the differences in biological pumping are most pronounced.

Figures 3 (top right Delta_DIC_org) and 5 (bottom Delta_d13C_rem) show where respired carbon (d13C) changes. Figure S2 in Schmittner and Galbraith shows the changes in the distribution of preformed nutrients.

Lines 8-10. Unless this other mechanism is discussed here, I might change the ending.

We have re-written this paragraph.

Figures – In addition to adding age markers to Figure 6, this figure could be improved - perhaps when there are two records on the same panel they can be shown in different colors with matching label?

Good idea. Done. Thank you :-)

I think streamfunction figures, comparable to Fig 4, would be a useful addition and would clarify the connection of isotope changes to circulation changes, discussed in some instances (e.g. upper South Atlantic).

We've added a streamfunction figure (new Fig. 2).

Response to Anonymous Referee #2

This is an interesting study compiling d13C data and model results. A series of fresh-water hosing experiments for preindustrial climate conditions are carried out with the UVIC climate-carbon cycle model. Model results in terms of changes in d13C and in atmospheric CO2 are compared to the ice core d13CO2 and CO2 records and to marine records of d13C as compiled from the literature for the period of the HS1 event (19 to 15 ka BP). The authors postulate that the early deglacial rise in CO2 was caused by a collapse of the Atlantic Meridional Overturnig Circulation that triggered a decline in ocean carbon storage.

This study presents interesting results and is a valuable contribution to the field com-plementing earlier studies investigating the response of CO2 and d13C to freshwater hosing experiments. I like the figures, the model analysis and the compilation of the d13C data. The study, however, does not consider the sequence of AMOC collapse and recovery nor takes into account the LGM initial state as done by earlier studies (e.g. (Menviel et al., 2008a;Marchal et al., 1999).

The manuscript requires substantial revisions regarding the interpretation of the results and of the main conclusion. This will require a revision of the abstract, the introduction and the discussion section and I also suggest that additional simulations are performed.

There is a broad literature on freshwater hosing experiments and their impacts on CO2 and d13C in the land, ocean, and atmosphere system. This body of literature is simply disregarded by the authors. While I appreciate that it is getting increasingly difficult to follow the literature, the authors apparently did not pay any attention to refer to earlier work.

It is not true that we did not pay any attention to or simply discarded earlier modeling work addressing CO2/d13C effects of hosing. In fact in our original manuscript we did cite five of those studies: Menviel et al. (2008, 2014), Schmittner and Galbraith (2008), Schmittner et al. (2007a) and Tschumi et al. (2011).

This is very disturbing in particular as both researchers have a long-standing track record in the field. Clearly, the authors should do a literature survey and point the reader to earlier work to place this work in the appropriate context and to discuss their findings in comparison with earlier results.

The scope of our manuscript is not an extensive review of the literature on modeling AMOC effects on CO2. However, as outlined in detail below we now do cite more of those studies as suggested by the reviewer.

In addition, the range of studies on the potential influence of SH wind changes on ocean circulation and the carbon cycle is not very well reflected.

1. Main conclusions are to be refined:

1.1) There are issues which need attention regarding the simulated rise in atmospheric CO2. First, the simulations are started from a preindustrial steady state. This caveat and its implication must be fleshed out more clearly and already be stated in the abstract. While discussed in the conclusion, the obvious implication, an overestimation of the CO2 change by the model, is not mentioned.

We have stated now in the abstract that the model starts from modern conditions. We also state clearly now in the abstract that the model overestimates the d13C amplitude in the North Atlantic. Acknowledging the uncertainties in the CO2 response, the title and abstract were modified to put more emphasis on the more robust conclusion that the AMOC was reduced and to more clearly highlight the uncertainties in the CO2 response. However, as discussed in the revised conclusions/discussion section and below we do not think that overestimation of the CO2 changes by the model is an obvious implication.

The AMOC was shallower in the NA during the LGM than at preindustrial. Consequently a smaller body of water was affected by a slow-down of the AMOC in the real ocean as compared to the model run.

It is not clear that a smaller body of water was affected. E.g. Kwon et al. (2012, Paleoceanography 27, PA2208) suggest that that NADW took over *more* of the global ocean interior during the LGM.

This mismatch is reflected in the too large changes in d13C in the North Atlantic as evident in Figure 6A-C and even more clearly in Figure 7, where all model results show substantially larger d13C changes than reconstructed. (The high correlation given in Fig 9 and the text is a bit misleading as the slope is quite different from the 1:1 line)

To address the reviewers concerns, we have expanded the discussion of the model data differences. We note that in addition to the correlation coefficient the RMS error provides another metric supporting the conclusion that model FW0.15 fits the data best.

Similarly, the Brazilian margin data show a d13C change between 1.6 and 2.1 km (line22 p2863) whereas the model shows a substantial d13C change in the entire water column below 1500 m.

These features indicate that the overall change in d13C in the North Atlantic is overestimated by the model and thus also the change in DIC and in turn the change in atm CO2 during H1.

Thank you for bringing up this point, which was echoed by Galbraith's short comment and the editor. We agree that it is possible that the model overestimates early deglacial AMOC effects on DIC and CO2 and discuss this now in the revised manuscript's discussion section.

Part of the data-model mismatch at the Brazil Margin may also be related to the depth of NADW; during the modern, it appears to be centered between 2-2.5 km whereas at the LGM it was centered between 1.5-2.0 km (Curry and Oppo, 2005).

I suggest that the author perform a spin-up under LGM condition and repeat their freshwater hosing experiment that show a collapse of the AMOC.

We agree that repeating the simulations with more realistic LGM initial conditions would be highly desirable. However, realistic deep ocean LGM conditions are currently not available. Even though it would be possible to use a simulation with LGM boundary conditions its value would be low if the deep ocean circulation and carbon cycle simulation is inconsistent with observations. Furthermore, it is important to note that our results serve as a sensitivity test and provide an initial estimate of the AMOC's impact on deep ocean d13C and the subsequent effects on the biological carbon pump.

1.2) Various proxy data and studies indicate a distinct evolution of the North Atlantic ventilation and the AMOC. For example, (Robinson et al., 2005) document the radiocarbon history in the western North Atlantic and show that ventilation strength varied from LGM, H1, Bolling/Allerod, Younger Drias and the Holocene. The AMOC recovered at the onset of the Bolling/Allerod (McManus et al., 2004). This may have reversed earlier changes in CO2 associated with an AMOC slow-down. However, this sequence is not reflected by the model and not discussed by the authors.

I suggest that the authors continue their simulations and force their model (e.g. by freshwater removal) to trigger an onset of the AMOC, ideally for an LGM spin up. This would allow the authors to compare the simulated d13C changes not only during H1 but over the period from H1 to the end of the B/A.

We agree that it would be interesting to simulate the full deglacial but extending the simulations is beyond the scope of our study. We emphasize this point now at the end of the introduction and include a brief discussion of the later deglacial at the end of the discussion/conclusion section.

2. Reflection of earlier work is missing

I find the introduction weak. In particular the literature on the subject is not reflected. Why? There is a range of (modelling) studies available that address the influence of AMOC changes on atmospheric CO2 and/or d13C and both from a terrestrial and oceanic perspective. Example that come immediately to my mind are (Menviel et al., 2008a;Marchal et al., 1998;Marchal et al., 1999;Menviel et al., 2012;Menviel et al., 2014;Köhler et al., 2005;Bozbiyik et al., 2011;Obata, 2007) and there is certainly much more in the literature.

I encourage the authors to do a thorough literature research, to discuss this earlier work and to compare their findings with earlier studies. The findings of earlier studies have also implications regarding the authors' main conclusion. Earlier studies find that the response in CO2 and the carbon cycle is sensitive to the initial state (e.g. (Menviel et al., 2008a;Köhler et al., 2005)

The purpose of our manuscript is not an extensive review of modeling studies regarding AMOC influence on CO2. We had already cited Menviel et al. (2008a) and Menviel et al. (2014) in our previous version. In the discussion section of the revised manuscript we now cite most of the additional references you mention. We also discuss the issue of the initial state.

3. Literature on SO wind changes is poorly represented

Again, the authors should search and carefully read and reflect studies addressing SO wind changes. The SO wind hypothesis has been challenged meanwhile by a broad range of studies. See (Tschumi et al., 2008) (Menviel et al., 2008b) and follow-up studies by others (e.g. (Lauderdale et al., 2013;d'Orgeville et al., 2010). Unlike stated in the introduction and reiterated in the conclusion, (Tschumi et al., 2011;Tschumi et al., 2008) do not support the suggestion that SO wind changes are responsible for the deglacial CO2 rise. To the contrary, Tschumi et al., 2008 state in their abstract: "Our results are in conflict with the hypothesis that Southern Hemisphere wind changes are responsible for the low atmospheric CO2 concentrations during glacial periods"

Contrary to the reviewers implication (as interpreted by the editor) we have not misrepresented the work by Tschumi. In our original manuscript we did not cite the *Tschumi et al. (2008)* paper, which discusses Southern Hemisphere winds as an explanation of the *full* glacial-interglacial CO2 change. We did cite a different paper *Tschumi et al. (2011)* because it focuses on the early deglacial CO2 rise, which is more relevant to our manuscript. In this paper, the authors perform model simulations driven by wind changes over the Southern Ocean. Here is a quote from their abstract: "Our results provide support for the hypothesis that a break up of Southern Ocean stratification and invigorated deep ocean ventilation were the dominant drivers for the early deglacial CO_2 rise of 35 ppm between the Last Glacial Maximum and 14.6 ka BP."
This is consistent with our original quotations in the introduction and in the conclusions. We have added "and/or stratification" to the sentence quoting Tschumi et al. (2011) in the introduction because in their above quote the word "wind" is missing and in the main text they note that they use wind changes only as a "knob" to change ventilation in the Southern Ocean.

P2860, line 18ff: please provide details how freshwater forcing is applied. Is there a compensation of salinity elsewhere? How are tracers affected by the freshwater input?

This info has been added.

Response to Eric Galbraith

The questions of what drove the CO2 rise during HS1, and what AMOC variability does to CO2 in general, are both really interesting, and really difficult. Published models disagree on the impact of an AMOC shutdown (as pointed out by Referee #2), which suggests a real need to bring in tighter observational constraints to test model predictions.

In this light, it is great to see the global array of foraminiferal carbon isotope measurements being brought to bear on these problems. This paper uses the carbon isotopes in the right way, in comparing to a good ocean model that accounts for both the physical processes (including air-sea exchange) and the biological respiration. As such, I think this is a very useful set of simulations and comparisons with data.

However, I'd like to raise two points with regard to the conclusions.
1. The model simulations start from pre-industrial boundary conditions (also pointed out by Referee #2). As shown by Schmittner et al. (2007), the effect of water-hosing on the UVic model in a pre-industrial state is much larger (27 ppmv) than on an LGM state (5 ppmv). Since the real deglaciation started from the LGM state, it seems the impact of the AMOC shutdown on CO2 should be overestimated by something like a factor of five in the model experiments shown here. This contrasts with marine isotope stage 3, when the intermediate ocean state would have presumably still left the AMOC with a larger amount of leverage on CO2. So if Schmittner et al. (2007) is still right, the actual direct effect of the AMOC shutdown during HS1 should have been much smaller, according to the UVic model (nevermind disagreement with other models, which show equivocal impacts of AMOC shutdown on CO2).

The LGM state of the global physical and biogeochemical ocean is currently unknown. The initial LGM state that Schmittner et al. (2007) used in their simulations was not tested against deep ocean reconstructions because at that time the model didn't include d13C. Thus we don't know if Schmittner et al.'s (2007) result is realistic.

Anyway, we now include an extensive discussion of this topic in the revised manuscript's discussion and conclusion section.

2. The comparison with data stops short just before the B-A. Prior experience with the UVic model shows clearly that the ocean will take up CO2 once again when the AMOC resumes (e.g. Schmittner and Galbraith, 2008). However, this did not happen during the deglaciation - instead, there was a permanent, net increase in CO2 between the LGM and the B-A. The mechanism behind the HS1 CO2 increase was therefore either a) did not include an input from the AMOC shutdown, or b) did include an input from the AMOC shutdown, which was followed immediately by a compensatory subsequent source of CO2 that masked the AMOC re-uptake of CO2 during the B-A.

You're right, an AMOC resumption would lead to a decrease of atmospheric CO2 in the model. We discuss this now (hopefully more clearly than before) at the end of the discussion and conclusion section. However, the HS1 – B-A transition is beyond the scope of our mansucript, which focusses only on the LGM – HS1 transition only. We prefer to keep speculations on the B-A to a minimum (see last paragraph of discussion and conclusions section).

Together, these two points suggest to me that the AMOC variability played only a minor role in the CO2 rise during HS1. That's not to say it didn't have any role, nor

We don't agree with that inferrence. Neither of those points implies a minor role of the AMOC on the HS1 CO2 rise.

that it didn't have a big impact on the redistribution of carbon isotopes within the ocean during HS1 - it probably did, and these results provide very useful support to the idea that NADW formation really did shut down at the time. But I think there's

We don't claim that and think that it cannot be inferred from our study. We only infer a substantial AMOC reduction for a multi-millennial time period during HS1. The fact that the model overestimates the d13C changes in the North Atlantic may indicate that the AMOC was not completely shut down during HS1.

enough wiggle room between the model d13C and the foraminiferal d13C observations to allow other processes, such as iron fertilization, changes in sea ice, changes in Southern Ocean convection, and changes in the marine ecosystem to have done the heavy lifting of atmospheric CO2 during HS1.

We cannot exclude causes other than the AMOC for the HS1 CO2 rise, but we think it is possible that it was entirely caused by the AMOC.

If the authors agree with me (at least partially) on these points, perhaps it would be helpful to somehow quantify the model disagreement in a way that would suggest the non-AMOC processes that contributed to the net CO2 rise during HS1? In other words, could the model-data mismatch help to diagnose any other processes that were behind the net CO2 rise?

Thank you for the suggestion but this is presently impossible. We require realistic initial conditions to improve these estimates in the future.