Response to the editor's comments

Your manuscript has now been seen by the two initial reviewers again, which diverge in their recommendation how to proceed. While referee #2 is essentially satisfied with your changes, referee #1 still asks for substantial and important additions to the manuscript.

Although an extended modeling framework with LGM boundary conditions and an extended timeframe as requested by referee #1 in his point 2) would clearly strengthen the paper, I can understand that this may be beyond the scope of this paper at this point, however, it should be clearly tackled in future research and this could be mentioned in the conclusions.

Such statements were already included in the previous mauscript version. The discussion and conclusion section stated "Resolving the likelihood of these different possibilities will be an important task for future research." and "Such an assessment requires simulations with more realistic initial conditions." We have now modified the latter statement to "Such an assessment requires simulations with more realistic initial conditions, which will be an important task for future studies."
We have separated the discussion and conclusion sections. In the new brief conclusion section we have added the following sentence with regard to our biological pump effect on CO2 and d13C_CO2 results. "However, this hypothesis needs further testing with more realistic deglacial simulations in the future."

Moreover, as a truely glacial run was out of reach at this point, I think the comparison with observations can only be qualitative, i.e., the amplitudes in observed d13C changes cannot be compared quantitatively. That is to say that common trends (d13C goes up/down both in the model and data) can be made, but comparisons of the amplitude of these changes in tenth of a permille are not really quantitatively meaningful due to the starting boundary conditions as outlined by referee #1 and Eric Galbraith in his previous comment. A good quantitative correspondence may be fortuitous. This is not to say that the results in your paper are wrong but the wording should be carefully chosen and the statements better qualified with respect to this in the entire text. I give many examples below, where such a qualification or change in wording is advisable.

We agree that this is a possibility and have added statements qualifying our results and clearly emphazasing the caveats as outlined in more detail below.

Having said that, I strongly agree with referee #1 that your revisions did not sufficiently address many of the referee's other points, such as for example an in-depth discussion of previous work and their results on CO2 and d13C changes in response to freshwater hosing experiments. Referee #1 provides a detailed list in the re-review with necessary changes (points 1, 3, 4, 5, 6, 7, 8) that I would ask you to address point by point. Final acceptance of the paper is dependent on adequately addressing these points and I would strongly encourage you to do this extra work as

I think your paper is an important contribution to this research question and should be published in CP in its final form.

We have responded to all points from referee #2.

Specific comments

Based on the fact that you start with HOL boundary conditions, your experiment is closest to the 8.2 kyr event. Can you make a statement based on your model, how large a potential freshwater forcing was at the 8.2 k event given that the CO2 drop was small (Indermühle et al., Nature 1999; Elsig et al., Nature 2009)?

The 8.2 kyr event was much shorter than our AMOC reduction experiments and thus an appreciable response of the deep ocean carbon reservoir as simulated here cannot be expected.

Line 16: is this overestimation in d13C in the North Atlantic due to the starting boundary conditions?

Three possible reasons for this overestimaton are discussed in the first paragraph of the discussion section. The mismatch in initial conditions is indeed one of them.

Line 35: As stressed by referee #1 the citation of Tschumi here is misleading

We argree with referee #1 that Tschumi do not claim wind stress changes have led to the initial deglacial CO2 rise and d13C_CO2 drop. However, they force their model with wind stress changes in the Southern Ocean and they conclude that changes in stratification in the Southern Ocean may have caused the observed CO2 and d13C changes. We now clarify this point now in the introduction (page 5, lines 25-28) writing "Tschumi et al. (2011), who's simulations include δ13C and are forced with wind stress changes over the Southern Ocean, conclude that stratification changes there can explain the observed rise of atmospheric CO2 and the decrease in δ13CCO2 during HS1." We hope that this formulation is acceptable.

Line 43: cite the basic papers on the bipolar seesaw by Stocker&Johnsen, Paleocean 2003, EPICA community members, Nature 2006, Blunier et al., Nature 1998) here.

These papers are now cited.

Add a thorough discussion on previous work in the introduction

We added two paragraphs to the introduction. One describing previous hosing experiments and one describing previous SH wind experiments.

Line 99-101: Not only the mean level may be different but also the amplitudes of changes. This should be clearly stressed here and discussed throughout the manuscript

We have added the following sentence: "Possible sensitivity of the results on initial conditions is further discussed in the discussion section below."

Line 119. Here and throughout the manuscript. The paper by Parrenin is only a minor update of previous CO2 measurements. Please cite also Monnin et al., Science 2011 and Marcott et al., Nature 2014

We added those references.

Line 120-121. Can you specify where this decrease in efficiency occurs?

We have removed the reference to the efficiency of the biological pump here since it is referred to a little further down more specifically. This paragraph only discusses the global mean and and vertical distribution. The figures of changes in remineralized d13C_DIC (bottom panels in Fig. 4) give a good indication of the spatial redistribution of respired carbon.

Line 120-131: state that these numbers are dependent on the boundary conditions

We note that Schmittner and Galbraith (2008) used glacial boundary conditions and came to very similar results. Thus it is not clear that the results depend strongly on boundary conditions. We also feel that the caveats are discussed in depth in the discussion section. In the model description section we specifically note that these are idealized simulations. However, we add a qualifier at the end of the paragraph as explained in the response to the following comment.

Line 133-139: state that these numbers are dependent on the boundary conditions and that the quantitative agreement with ice core measurements may be fortuitous

See previous response. Nevertheless, we added here the following qualifier: "but the model response may depend on boundary and/or initial conditions and this agreement may be fortuitous"

Line 160-167: dependent on the boundary conditions

Line 181-186: using other boundary conditions the agreement would be most likely different for the same freshwater forcing, please qualify your statements

Line 190-195: here you argue that the difference in model and observations may be due to the model boundary conditions, at other instances, where they agree, you do not.

Line 206-208: agreement dependent on the boundary conditions

Line 236-237: I wonder in how far this is dependent on the boundary conditions

Instead of adding qualifiers to each statement in the results section we have added the following qualifier in the discussion section (page 9, line 7): "and the agreement with the ice core observations could be fortuitous"

Line 243-246: That's not what Tschumi et al. want to say

This paragraph has been removed.

Line 268-269: See and discuss references given by referee #1

This reference is included in the introduction.

Line 275-278: This statement confused me as your AMOC goes down to 2 Sv, please clarify

We have added the following statement "Due to these issues our results can only be regarded as semi-quantitative." in order to make clear that at this point we cannot provide a truly quantitative AMOC reconstruction during HS1.

Line 282-283. See comment above on Tschumi et al.

We think the phrasing here is appropriate as Tschumi clearly focus on Southern Ocean processes and state in 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 CO2 rise of 35 ppm between the Last Glacial Maximum and 14.6 ka BP."

Line 314: see comment on Parrenin paper above

We've added the references.

Line 390: please explain FeL

Explanation was added

Figure caption 1: see comment on Parrenin paper above

We now use the Marcott et al. (2014) data.

Response to reviewer 2

I am disappointed by the response of the authors and it reflects a missed opportunity to improve the manuscript based on input provided by the reviewers and E. Galbraith.

A marked-up version with the changes identified would have been helpful for the reviewer(s).
The authors should provide the simulations requested by E. Galbraith and reviewer 2. The authors should summarize the finding of earlier freshwater model experiments in a paragraph in the introduction. The authors should summarize the literature for changes in SH westerlies in another paragraph in the introduction. The authors should remove the misinterpretation of Tschumi et al, 2011 in the next manuscript.
1) Figure 10 reveals that the simulated d13C anomalies are in the range from 0.5 to – 1.6 permil whereas the observation based range is about a factor of two smaller. This is an important finding and should be noted in the result section. Moreover, the authors should try to find a model solution where they can reduce this large mismatch. The author should clearly spell out in the abstract and the discussion that this most likely leads (or at least "may lead") to an overestimation of the response in CO2 to an AMOC shut-down for the LGM.

Such a discussion was already included in the first revision. In the abstract we state "… but the model overestimates d13C reductions in the North Atlantic." and in the discussion section "However, as discussed above, one possible explanation for the overestimated North Atlantic d13C_DIC changes in the model is that it overestimates the early deglacial AMOC changes. If this was the case the model could possibly also overestimate the effect of the AMOC changes on atmospheric CO2 and d13C_CO2. This would imply that other processes have contributed to the early deglacial CO2 and d13C_CO2 changes."

2) The authors apply a cost-efficient Earth System Model of Intermediate Complexity with an energy balance atmosphere. The advantage of such an approach, e.g. in contrast to a comprehensive ESM, is that many simulations can be carried out and the sensitivity of simulated responses to initial states and parameters can be explored.

The authors should extend the simulation FW015 (or preferentially a simulation that starts from LGM conditions) to a state where the AMOC is turned on again. They may restart their simulation and add a negative freshwater anomaly at 14.7 ka to mimic the onset of NADW at the beginning of the B/A.

I appreciate that it might be more difficult to get an LGM state, but this should also not be out of reach, given that the model has been initialized for the LGM before. Further, the authors state: "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." and "Whereas simulations with glacial boundary conditions (lower GHG, orbital parameters, ice sheets) are easy and have been done before (e.g. in PMIP)

It is possible (or even "easy") and the authors should run the model for an initial state with a shallower than modern AMOC to see whether they can reduce the d13C data-model mismatch and what the implications are for atm. CO2 and d13C

It is a weak argument that the LGM ocean is poorly constraint and that their model might be unrealistic for the LGM. This argument applies equally for the simulations now described. We know that the modern ocean is different from the LGM state and the early termination, e.g. from d13C and 14C data etc. Still the authors use the modern condition as a starting point for the simulation. They do not hesitate to use the results for the modern conditions to come up with their strong and, as pointed out by E. Galbraith, likely unrealistic conclusion on the early CO2 rise.

We object to the reviewer insisting to demand the presentation of new evidence (conduct new simulations), which, as we have already responded to in our initial response, is not possible currently. We think we have appropriately discussed the caveats of our study in particular the issue of initial conditions.

3) A paragraph is needed in the introduction that summarizes the results from earlier freshwater hosing experiments. This would allow the reader to put this work in the context of the literature. It would provide a starting point for the work done here. In particular the findings that the CO2 responses, and related carbon stock changes on land and in the ocean, are found to be very sensitive to the initial state must in my opinion be discussed upfront. Similarly, responses were found to be smaller for LGM boundary conditions than for preindustrial conditions. A point that also need to be mentioned upfront. I have already provided various references in my initial review. Another study that is of interest in this context is the work by Menviel et al., QSR, 2012 where the results of transient glacial-interglacial simulations is discussed and model outcomes are compared with proxy data. This reveals that changes in AMOC for a reasonably realistic LGM state have a much smaller impact on atm. CO2 than postulated by Schmittner and Lund.

We have moved the paragraph discussing hosing experiments from the discussion section to the introduction and we have also expanded it including citation of Menviel et al. (2012).

4) A paragraph is needed in the introduction that summarizes the results from earlier wind stress experiments on deglacial CO2. The available results suggest a weak influence on changes in SH westerlies on atmospheric CO2 for the deglacation. Thus, the results presented by the authors fall well within the range of earlier quantitative studies and do not provide much new insight. There is even one study available with the UVIC model (d'Orgeville et al., GRL, 2010) which is not mentioned either.
See for example Menviel et al., 2008, Tschumi et al, PO, 2008, d'Orgeville etal, 2010, Lee et al., PO, 2011, Voelker and Köhler, PO, 2014) for wind and CO2, or also work by England and Sijp and by Böning et al., NatGeo, 2008 etc. considering physics only.

The authors should condense the text and figures on SH westerlies as it is a confirmation of earlier work.

We have now included a separate paragraph in the introduction discussing previous modeling of SH westerly wind changes including citation of most of the suggested papers.

5) Abstract, line 3, and line 22-25: The authors focus here on SO wind changes as the only mechanisms for SO circulation changes. The text gives a wrong impression. While it is reasonably clear from the literature that the sensitivity of atm. to realistic wind stress and wind position changes is fairly weak, there are other mechanisms that may have been responsible for deep ocean circulation changes in relation to the Southern Ocean atmospheric window. It is clear from the literature that SO changes did take place, e.g. as indicated by opal records, the d13C and D14C records. The sentence "We propose that the observed early deglacial rise in atmospheric CO2 and the decrease in _13CCO2 may have been caused by an AMOC induced decline of the ocean's biologically sequestered carbon storage without the need to invoke changes in Southern Hemisphere winds." gives the impression that SO changes played no role at all. As mentioned above, it is not a new finding that SO wind changes played a small role. The more general statement, that there is no need to invoke SO changes to explain atm. CO2 is can hardly be reconciled with the available proxy evidence. In particular, as this MS makes no statement on the role of SO D14C and on SO opal fluxes. If the authors wish to talk about SO wind in the abstract, they should do this in a clean statement, e.g ." Our results confirm the findings of earlier model studies that changes in SO winds likely played a small role for the deglacial CO2 rise."

We discuss now the differences between model simulations in detail in the introduction. As explained there our study specifically examines the hypothesis of Anderson et al. (2009) that AMOC changes trigger changes in atmospheric circulation and southern hemisphere westerly winds, which than would lead to outgassing of carbon. We do this by using wind anomalies from a consistent coupled ocean-atmosphere model simulation of an AMOC shutdown, which has not been done before. This is in contrast to the more idealized forcings used in previous studies. Therefore our SO wind results are not merely a confirmation of earlier work.

We have rephrased the abstract accordingly.

6) Line 243 ff and line 283 The misinterpretation of the work by Tschumi et al., 2011 is to be removed. Tschumi et al., 2011 quantify the influence of SO ventilation changes on CO2 and d13C, but they do not

postulate that SO wind changes are responsible for ventilation changes. They clearly state this in their paper at several places. For example, at the beginning of the discussion:
"Second, changes in deep ocean ventilation are generated by varying the magnitude of the wind stress in the Southern Ocean by more than a factor of two. This is a convenient technical tuning mechanism to adjust the deep ocean ventilation age in the model. However, we stress the fact that these variations in SO wind strength exceed the range of realistic changes. The atmospheric CO2 response to realistic variations has been found to be rather modest (Tschumi, 2008; Menviel et al., 2008).

We have removed this paragraph.

7) Line 267: The authors state that a deep ocean state estimate of the global LGM circulation and carbon cycle that is consistent with sedimentary _13CDIC reconstructions does not exist. While of course no model is perfect, there exist simulations of LGM conditions including isotopes. For example, Menviel et al., QSR, 2012 provide a set of simulations with different LGM states and their simulated d13C anomalies for the LGM as shown in their Figure 9 compare reasonably well with reconstructions. As mentioned above, simulated changes in atm. CO2 in response to changes in AMOC are relatively small and do not explain the initial deglacial CO2 rise.

We have included a discussion of the different model CO2 responses to AMOC in the introduction.

8) The authors state in the discussion section when dealing with the d13C model-data mismatch: "
„the model would overestimate
changes in volume fluxes and perhaps carbon isotopes even if a complete AMOC collapse
did occur during HS1." They should spell out that this applies not only to the volume flux and carbon isotopes but also to atmospheric CO2 as indicated by previous studies.

This was already explicitly discussed in the previous manuscript version "one possible explanation for the overestimated North Atlantic d13C_DIC changes in the model is that it overestimates early deglacial AMOC changes. If this was the case the model could possibly also overestimate the effect of the AMOC changes on atmospheric CO2 and d13C_CO2."

Further comments:
line 133: Sentence is confusing. "Because biologically sequestered, organic carbon is isotopically light (_13Corg = -20‰) its loss increases deep ocean _13CDIC by 0:06 permil (Fig. 3f) and its gain decreases _13CDIC (by _ 0:3 permil) in the surface ocean and _13 135 CCO2 by _ □0:25‰in the atmosphere (Fig. 1d)."

We have rephrazed this sentence.

My understanding is:
- The decrease in global DOC inventory as shown in Figure 3a tends to decrease d13C of DIC, atm. CO2 and land C as isotopically-light carbon is moved from DOC to the other carbon reservoir.
-The increase in land carbon storage tends to increase d13C in the atmosphere and ocean
-The decrease in the efficiency of the biological pump tends to decrease d13C in the atm. and the surface ocean and in DOC and POC and to increase d13C in the deep ocean.
The result of these three processes is a slight increase in the mean ocean signature of DIC.

Figure 1: Reference to Schmitt et al., 2012 seems missing here.

Thanks. We've included it.