Dear Reviewer,

Thank you for your thoughtful and helpful comments!
Please find our answers and changes below.

Comments on manuscript cp-2016-31 "Disentangling the effect of ocean temperatures and isotopic content on the oxygen - isotope signals in the North Atlantic Ocean during Heinrich Event 1 using a global climate model" by M. Bügelmayer-Blaschek, D. M. Roche, H. Renssen, and C. Waelbroeck

General comments

With an Earth system model of intermediate complexity including an iceberg module the authors investigate the distributions of $\delta^{18}O$ of water and of calcite in the North Atlantic ocean during Heinrich event 1. They analyze the temporal evolution of $\delta^{18}O_{calcite}$ and put forward two different geographical patterns: areas where the $\delta^{18}O_{calcite}$ hardly changes (or with large delay) during H1 in contrast to other areas where the $\delta^{18}O_{calcite}$ closely mimics the evolution of that of $\delta^{18}O_{seawater}$.

This is a very interesting research subject which is helpful in the context of improving our understanding of past climates. The method and tools are pertinent. However the analysis of the results is somewhat too qualitative. The draft seems to have been hastily written with several repetitions and inconsistencies. If re-worked thoroughly this could become a very pertinent paper.

Subject to the revisions of the specific comments below I would recommend publication in CP.

Specific comments

1. One important aspect which is not addressed in this study is whether the inclusion of an iceberg module in the model does help improving the modeling of $\delta^{18}O$ during Heinrich events or not? In short: is it worth including icebergs in climate models? Does it bring significant improvement of modeling studies? A comparison with the results of already available water hosing experiments performed with the same model would be welcome and significantly add to the value of the present work.

Please see general comments.

2. Some of the conclusions are clearly overstated: "The comparison of the model experiments with four marine sediment cores indicates that the experiment with an iceberg forcing of 0.2 Sv for 300 years yields the most reasonable results." (lines 20-21), "we find that the set-up of an iceberg forcing of 0.2 Sv over 300 years yields the most reasonable results" (line 403), and "From the comparison of simulated sea surface temperatures and $\delta^{18}O_{calcite}$ with proxy data, we find the best agreement between model output and data is reached when the iceberg discharge is stopped after 300 years." (lines 475-477). The data-model comparison as performed so far does not provide enough evidence for such conclusions.

3. The (short, lines 381-401) data-model comparison carried on page 12 suffers from several shortcomings.

    1. Cores 2 & 3 (NA87-22 & CH69-K09) are compared to the model results in the Baffin Bay (lines 381-386). Yet, if the map in Fig. 8 is correct, cores 2 & 3 are not expected to be representative of the Baffin Bay, but rather of the North-East Atlantic and the central North Atlantic.

2. ICE-600 et ICE-900 display similar changes in the Nordic Seas as observed in core 3 (ENAM93-21) but this agreement is dismissed on the basis of a lack of agreement for SST which is not represented for that area. Further, this decision somewhat contradicts the (sound) remark that reconstructed SST for high latitudes have high uncertainties (lines 448-451).

Thank you for those valid points, please see general comments.

3. We miss the information in panel a) and d) of Fig. 9 to appreciate how the $\delta^{18}O_{calcite}$ from core 4 (KNR316-GPC5) might fit the different experiments.

We unfortunately truncated the figures, we have changed this.

4. The duration of the perturbation in cores 1, 2, and 3 appear to be much longer than in the model experiments. How do you explain it?

It is important to notice that we didn't perform transient experiments, instead we applied a 300 / 600 / 900 year freshwater flux under constant LGM conditions. We first chose to apply a 0.2 Sv forcing over 300 years because Roche et al. (2014) found the best agreement with paleoclimatic data in this set-up, when comparing the maximum change in d18O_calcite during a before a Heinrich event. Yet, the estimated duration of the Heinrich events varies from 250 to 1500 years, therefore we performed three experiments of different time lengths, but we never intended to simulate the transient pattern recorded in data. We repeated that information of fixed boundary conditions in the discussion (390-392).

The data-model comparison is minimalist and too qualitative. I would recommend the use of additional cores – many are available – to improve that aspect. Wouldn't it also possible to perform cross-correlation between time series (model versus data; between variables)?

Please see comment above about the chosen cores.

4. "the timing of the first response to the iceberg forcing coincides between all the experiments" (abstract, lines 10-12;); "Yet, our results show that the timing of the first response to the iceberg forcing in SST, $\delta^{18}O_{seawater}$ and $\delta^{18}O_{calcite}$ coincides between all the experiments in the various regions within 300 years." (conclusion, lines 472-474): this should not be presented as an outcome of the study; given the experimental setup this behavior is expected.

Thank you for pointing this out, it is badly formulated. We re-wrote lines (550-553) to:

Our results show that the timing of the first response to the iceberg forcing in SST, $\delta^{18}O_{seawater}$ and $\delta^{18}O_{calcite}$ coincides between all the experiments in the various regions and is within 300 years. Applying the iceberg forcing for additional 300 (ICE-600) and 600 (ICE-900) years, respectively, causes a shutdown of the AMOC and more negative values in the North Atlantic.

5. The discussion on MOC (lines 421-434) should be dropped; it is neither relevant nor needed for the present work.

We do not agree that it should be completely dropped, but we have added more information concerning other studies (lines 490-500).

6. Question "(1) what is the impact of the duration of the iceberg discharge on the climate's response?" (line 109) should be reformulated. The consequences of the icebergs discharge on North Atlantic Ocean properties are examined but their climate impact is nowhere discussed.

Thank you for pointing this out. We have reformulated question 1) to: What is the impact of the duration of the iceberg discharge on the AMOC and the North Atlantic Ocean?

7. It is nowhere mentioned how $\delta^{18}O_{calcite}$ is computed.

We have added this information in the methods section.

8. "In these two regions ICE-600 and ICE-900 show much stronger reductions in SSS than ICE-300 at the end of the iceberg discharge because there the decrease is mainly caused by the advection of the fresh surface waters rather than by the amount of icebergs reaching these areas, which is comparable in all three experiments." (lines 223-226). I do not get the argument. Wouldn't it be simply due to the fact that the cumulative freshwater flux is much larger in ICE-600 and in ICE-900? May be reformulate?

We have rewritten lines (238-242) to clarify this statement:

In these two regions ICE-600 and ICE-900 show much stronger reductions in SSS than ICE-300 at the end of the iceberg discharge. This reduction is mainly caused by the advection of the fresh surface waters, rather than by the amount of icebergs reaching these areas, which is comparable in all three experiments.

9. ..."we find an immediate response in $\delta^{18}O_{calcite}$ to the iceberg release at the calving sites and in the North Atlantic, but it takes more than 100 years to cause a significant change in regions further away from the calving sites" (lines 240-243). This statement appears to be in contradiction with the foregoing discussion and Fig. 3.

Thank you for pointing this out, we have re-written the sentence so that it now states (254-257):
Moreover, we find an immediate response in $\delta^{18}O_{calcite}$ to the iceberg release at the calving sites and in the North Atlantic. In regions further away from the calving sites it takes up to 100 years for the iceberg discharge to cause a significant change.

Other comments

1. line 73: remove "yearly" (brings confusion with Sv units used for fluxes)

   We changed line 73 as suggested by the reviewer.

2. line 81: "simulated" is missing an "l"

   Thank you, we added the missing letter.

3. lines 87-90: "but the authors notice that the total volume released is similar to the one obtained by Roche et al. (2004)." Based on the numbers given here, the Roche et al. (2004) freshwater volume (0.29 Sv during 250 yr) is nearly 4 times that of Roberts et al. (2014) (0.04 Sv during 500yr). Can you develop the similarity?

Roberts et al. (2014) state in their table 1 a total ice volume flux of $60x10^4$ km^3, Roche et al. (2004) released 85 $x10^4$ km^3. We changed line 95-96 so that it now states

Their set-up indicates a much weaker freshwater flux of 0.04 Sv over 500 years than expressed by previous studies, but the authors notice that the total ice volume released is similar to the one

obtained by Roche et al. (2004).

4. line 95: replace “the take up” by “the uptake”
   We replaced it.

5. lines 104-105: the last part of the sentence “who showed that the freshwater flux that yields model results in best agreement with available proxy data evidence is 0.2 Sv” is part of the preceding discussion (lines 70-90) and should be moved there (line 79?)

   We deleted this part of the sentence.

6. lines 120-121: suggestion “The ~~included~~ atmospheric model ECBilt (Opsteegh et al., 1998) is a quasi-geostrophic, spectral model ~~calculated~~ running with a time step of 4 hours on a horizontal T21 truncation...”

7. lines 123-124: “...precipitation is only computed in the ~~lower-most~~ lowest (tropospheric) layer...”

8. line 130: suggested changes “~~The discretization is done on an approximately~~ CLIO has a resolution of 3°x3° ~~in longitude and latitude and presents~~ with 20 unevenly spaced vertical levels in the ocean”

9. lines 135-135: “The vegetation model ~~used is~~ VECODE (Brovkin et al., 1997) ~~that~~ accounts for...”

   We took the kind advise of the reviewer (points 6 – 9) into account and changed the manuscript accordingly.

10. lines 137-138: suggested changes “~~It depends on the~~ VECODE is forced by temperature ~~and~~, precipitation and CO_2 provided by ECBilt and accounts for long-term...”

    In the used set-up CO_2 was not provided by ECBilt.

11. lines 152-153: suggestion “and their meltwater fluxes are put into the ocean surface layer of the current ~~grid cell~~ water column.”

    We changed it as nicely suggested.

12. lines 158 & 399: “‰ ”, missing space after the per-mil symbol

    We added the missing space, thank you.

13. line 164: what does “This value” refer to? Here there should be reference to the work of Kageyama et al. (2010).

    This value corresponds to 0.2 Sv mentioned in the sentence before. Unfortunately, we don’t understand the comment of the reviewer why there should be a reference to Kageyama et al. (2010).

14. line 187: the MOC recovery for exp ICE-600 is not seen in Fig. 2

    We added “(please see supplement material)”.

15. lines 188-190: Previous works addressed the issue of MOC recovery in LOVECLIM; e.g., Rahmstrof et al. (2005), Menviel et al. (2008), Kageyama et al. (2010).

Thank you for pointing this out, we added the work of Menviel et al. (2008) in the discussion.

16. line 280: suggest "~~take up~~ uptake"
17. line 282 suggested "freezing point at ~~of~~ about -2°C"

    We changed the lines as suggested.

18. lines 350-352: suggested changes: "Before looking in detail at the four cores to investigate whether or not the simulated patterns can be confirmed by the data, several important remarks need to be made. ~~we have to point out that first,~~ First, while the sea level rise due to the released icebergs during HS1 is accounted for in our experiments~~. Yet~~, we do not simulate the background sea level rise starting at 19 ka after the onset of the LGM (Lambeck..."

    We re-wrote it as commented by the reviewer.

19. lines 395, 399 & 436: ($\sim$ 1Sv)
20. Figures 4 to 7: should specify that $\delta^{18}O_{calcite}$ and $\delta^{18}O_{seawater}$ represent surface values
21. Figures 4 to 7: scales are not legible
22. Figure 9: the left panels are truncated.
23. Figure 9, legend, 3d line: $\delta^{18}O_{sw}$ and $\delta^{18}O_c$ ("c" an "sw" should appear as subscripts).
24. Figure 10: left panels are truncated.
25. Figure 10: unit of IRD in panel a) number of grains/g sediment?

Thank you for your comments 19 to 25, we changed the manuscript accordingly.

References

- Kageyama, M., A. Paul, D. M. Roche, and C. J. Van Meerbeeck (2010), Modelling glacial climatic millennial-scale variability related to changes in the Atlantic meridional overturning circulation: a review. Quaternary Science Reviews, 29, 2931-2956, doi:10.1016/j.quascirev.2010.05.029.
- Menviel, L., A. Timmermann, A. Mouchet, and O. Timm (2008), Meridional reorganizations of marine and terrestrial productivity during Heinrich events, Paleoceanography, 23, PA1203, doi:10.1029/2007PA001445.
- Rahmstorf, S., M. Crucifix, A. Ganopolski, H. Goosse, I. Kamenkovich, R. Knutti, G. Lohmann, B. Marsh, L. Mysak, and Z. Wang, A. Weaver (2005), Thermohaline circulation hysteresis: a model intercomparison. Geophysical Research Letters, 32, L23605, doi:10.1029/2005GL23655.
- Roche, D., D. Paillard, and E. Cortijo (2004), Constraints on the duration and freshwater release of Heinrich event 4 through isotope modelling, Nature, 432, 379–382, doi:10.1038/nature03059.
- Roche, D. M., D. Paillard, T. Caley, and C. Waelbroeck (2014), LGM hosing approach to Heinrich Event 1: results and perspectives from data–model integration using water isotopes, Quaternary Science Reviews, 106, 247-261, doi:10.1016/j.quascirev.2014.07.020.