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Climate of the Past Discussions
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Interactive comment on "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* Marianne Bügelmayer-Blaschek et al.

Anonymous Referee #1

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This study analyses three simulations of Heinrich Event 1 with an isotope-enabled model of intermediate complexity. Time series of simulated d18O seawater and calculated d18O calcite (based on simulated d18O seawater and simulated seawater temperatures) are then analysed and loosely compared to four sediment cores. The three different simulations differ in the length of the iceberg calving episode and have different impacts on the Atlantic Overturning Motion. The authors conclude that the duration of the simulated Heinrich event causes a strong and non-linear response in the AMOC.

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I have several major problems with this study.

First, a very similar study appeared last year (Bagniewski et al., 2015, “Quantification of factors impacting seawater and calcite d18O during Heinrich Stadials 1 and 4”, Paleoceanography, 30(7), 895-911). Bagniewski et al. conducted several simulations of Heinrich Events with an isotope-enabled model of intermediate complexity. They disentangled the effect of ocean temperature and isotopic content on d18O calcite in the North Atlantic and worldwide. They took this study one step further and also disentangled the pure d18O seawater meltwater signal and the changes in d18O seawater due to changes in circulation and climate. They then compared their simulated values with over 20 sediment records at the surface and at depth (including time series in their supplementary material). Bagniewski et al.’s research questions and conclusions are similar to what is presented here.

There is however one exciting new aspect in this study: the simulation of more realistic meltwater pattern and the fact that seawater temperature changes due to latent heat loss of melting icebergs are taken into account. To make this study more original, one solution might be to rewrite the paper and focus on the impact of a more realistic parameterization of iceberg drift and melting on d18O seawater and d18O calcite (i.e. compare pure hosing experiments with the same amount of total freshwater with the simulations presented here). Given that most modeling studies of Heinrich events are based on simplified meltwater hosing scenarios, such a study would be of great interest to the modeling community. An in-depth discussion on how well such a coarse resolution model can be expected to capture iceberg drift should also be included.

Second, the authors cannot conclude that it is the length of the Heinrich Event that causes the non-linear response of the AMOC. To make this conclusion, one additional simulation should be integrated which introduces the same overall volume of icebergs released in ICE-900 over only 300 years. I would not be surprised if the impact on the AMOC in such a simulation is similar to ICE-900, but I might be wrong. There are many publications discussing the hysteresis behavior of the AMOC for a whole zoo of models (including model intercomparison studies), and I would strongly encourage the authors to cite the original literature when discussing this aspect. Most of these publications base their analysis on the total volume of freshwater added. If LOVECLIM shows a significantly different behavior based on the duration only (and not on total volume) then this might be an interesting and publishable result. Otherwise, there is nothing new about this result.

Third, the comparison with only 4 sediment cores is deceptive. There are more cores in the North Atlantic that recorded d18O calcite changes over this period with high enough sedimentation rates. These cores should be taken into account in this analysis.

Finally, time series of the model results at the location of these cores should be shown with the sediment data in the same figure (for the better or worse) and discrepancies should be discussed. Nobody would expect a perfect fit, but this one-on-one comparison is still important to validate the model simulations.

Other comments:

Proxy data reconstructions and a few model simulations suggest that the main source of dense waters in the North Atlantic during the last glacial was the Nordic Seas. Labrador Sea Water seems to have played a small (or maybe even absent) role. LOVECLIM simulates an important convection site in the Labrador Sea for the control run. During the transient simulations, this convection site shuts off and influences d18O seawater, temperatures and, of course, the strength of the AMOC discussed in this paper. The fact that this convection site might or might not be realistic and how this impacts the results should be discussed in the paper.

Along the same lines, wouldn't one expect Baffin Bay to be completely ice covered and at freezing point just before and during Heinrich Event 1? How realistic are the simulated warm conditions in this region (probably related to the near-by convection site) at this point of time? See for example Gibb et al. 2015, "Diachronous evolution of sea surface conditions in the Labrador Sea and Baffin Bay since the last deglaciation",

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The Holocene, 25(12), 1882–1897.

What is the equivalent sea level rise for each of the 3 simulations and how does this compare to data/reconstructions?

Which equation is used to calculate d18O calcite in the model?

Line 36: “ocean cores” should probably read “sediment cores”

Line 55: IRD transported by sea ice?

Line 56: Should read “>63um”?

Line 158-159: How can the value of iceberg d18O not be important for a study that analyses changes in d18O during a Heinrich event?

Line 187: it is not obvious (Fig 2) that ICE-600 recovers 700 years later

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