

## Author reply A. Schmittner (Referee) comment “Millennial changes in North Atlantic oxygen concentrations”

The manuscript presents new data of millennial variations of dissolved oxygen in the North Atlantic. I think the manuscript is well written and illustrated and that the main conclusions are supported by evidence. Quantitative reconstructions of dissolved oxygen are rare but, in my opinion, extremely valuable. I applaud the authors for a job well done. The comments listed below are rather minor, but the authors may find them useful in case they prepare a revision.

Title Page: typo in first affiliation (Univeristy)

This will be corrected.

Fig. 1: is it necessary to show the whole globe in the top map? It may be better to zoom into the North Atlantic to see better which sections are used.

We will revise our Figure 1 to focus in on the North Atlantic.

Page 12949, lines 16-17: I don't think that this statement is accurate. No references are provided that would support it. Contrary to what is claimed here Gregory et al. (2005, GRL 23, L12703) have argued that the AMOC reduction is mostly due to heat fluxes rather than freshwater fluxes.

Perhaps our wording is a too strong here. According to Gregory et al. (2005) 'the THC weakening is caused more by changes in surface heat flux than by changes in surface water flux.' We will revise this line to 'The future reduction in ocean overturn is mainly attributed to changes in surface heat flux and to a lesser extent to surface freshening (Gregory et al., 2005).'

Page 12950, line 2: You may also want to cite Schmittner & Lund (2014, Clim. Past 11, 135-152), who present the first evidence from distributed deep ocean d13C compared with model simulations.

We will add this reference to provide support for the interpretation of the nutrient proxy d13C.

Page 12950, lines 20-23: I think this is a very optimistic statement. I also don't see how those reconstructions would directly help constrain future projections. I'd suggest to remove the statement or to provide some arguments supporting it.

This section will be deleted.

Page 12952, line 16: Here and elsewhere I suggest to replace d13C with d13C\_DIC in order to differentiate between water column data from other sources such as carbonates.

This will be changed in the manuscript.

Page 12952, lines 22-24: d13C\_DIC distributions in the ocean are also affected by temperature dependent fractionation during air-sea gas exchange (e.g. Lynch-Stieglitz et al. BGC 9, 653-665) and the degree of equilibration of surface waters with the atmosphere (e.g. Schmittner et al. 2013, Biogeosciences 10, 5793-5816)

We can rephrase these lines to 'Furthermore, variations in  $\Delta\delta^{13}\text{C-DIC}$  distributions in the oceans are also affected by temperature dependent fractionation during air-sea gas exchange (Lynch-Stieglitz et al., 1995), degree of surface water equilibration with the atmosphere (Schmittner et al., 2013), exchange at source waters, biology and also mixing with other water masses (Gruber et al., 1999).'

Page 12952, line 6: Fig. 3 is discussed before Fig. 2.

We can change the order in the text and also swap the captions (+figures) around.

Page 12953, lines 13-14: The following part of the sentence is somewhat ambiguous: “inferred from bottom water and anoxic boundary dwelling foraminifera” because it is not clear if the forams are dwelling in bottom water and in the anoxic boundary, or if “inferred from bottom water” means inferred from insitu measurements of bottom water oxygen concentrations. Please clarify.

We can rephrase this to 'Hoogakker et al. (2015) furthermore show that additional observations of  $\Delta\delta^{13}\text{C}_{\text{bw-ab\_pw}}$ , inferred from the difference in  $\delta^{13}\text{C}$  between bottom water and foraminifera living at the anoxic boundary dwelling foraminifera (*Globobulimina* spp.) as well as between bottom water suspension feeding (*Cibicidoides wuellerstorfi*) and anoxic boundary dwelling foraminifera (*Globobulimina* spp.) all fit the original observations exceptionally well at [O<sub>2</sub>] between 55 and 235  $\mu\text{mol/kg}$ .'

Fig. 4: it may be interesting to plot the cibicides and *Globobulimina*  $\delta^{13}\text{C}$  separately to see which of those dominate the resulting variability of the gradient.

We can do this, and then should also change our figure caption to 'Figure 4. Benthic foraminifera  $\Delta\delta^{13}\text{C}$  at deep site MD95-2042 and intermediate ODP Site 1055 and their planktonic foraminifera oxygen isotopes. Original benthic foraminifera  $\delta^{13}\text{C}$  records (MD95-2042 from Shackleton et al., 2000) of epifaunal *C. wuellerstorfi* (red circles) and deep infaunal *G. affinis* (blue circles) are also shown intercalated between the  $\Delta\delta^{13}\text{C}$  records. Several Heinrich events and cold events are shown.'

Page 12956, lines 8-10: Please show the  $\delta^{13}\text{C}$  so that the reader can understand this note.

We can show this, also see above.

Page 12956, line 12: “event” should be “events”.

This will be corrected.

Also, please note C21 in the figure. We will add this note.

Fig. 4: what is the event (grey bar) between H6 and C19 in the lower panel?

It is what we refer to in the text as 'the cold period that follows' at page 12956, line 23.

Fig. 5 and discussion: it seems to me that the millennial events are barely resolved. H1, e.g., seems to be two data points. H4 also two or three (hard to see from the figure). I wonder if bioturbation could dampen the signal. I suggest to discuss this point, which may also be relevant for the model-data comparison on page 12960.

We do discuss the smoothing effect (page 12960, lines 21 to 23), but in view of the reviewers comment can amend this line to emphasize that this is through bioturbative mixing e.g. 'However, it is noted that the model outputs depict a particular (extreme) point in model time, whereas reconstructions from deep sea sediments represent an averaged view where extremes have been smoothed out by bioturbation.'

Page 12957, lines 1-2: “they are significantly reduced compared with warm interstadial intervals as well as the LGM.” They don’t seem to me to be significantly reduced compared with the LGM. Please provide statistical calculations such as means, errors for the means and significance levels for the means to be different.

We can change 'significantly reduced' to 'lower'.

Page 12959, lines 6-8: Note that the first modeling study to show this was in fact

Schmittner (2005, Nature). I suggest to cite the original paper.

We can change the sentence to 'Model simulations suggest that export production during Heinrich events was globally reduced, with a decrease in the North Atlantic (Schmittner et al., 2005; Mariotti et al., 2012).'

Page 12960, line 23: "smoothed out" by bioturbation? Another reason for the larger amplitude changes simulated by the Schmittner et al. (2007) model is that their simulation starts from a pre-industrial background state. If a glacial state with a weaker and shallower AMOC was used the amplitude of the oxygen changes at the deep site would have been presumably smaller. This may also explain the overestimated amplitude in benthic  $\delta^{13}C$  simulated in the North Atlantic by Schmittner and Lund (2014).

We can add the following sentence at line 21 ' The larger amplitude changes in seawater [O<sub>2</sub>] simulated by Schmittner et al. (2007) may be the result of the prescribed pre-industrial boundary conditions with strong AMOC; if they had used a glacial boundary conditions with weaker AMOC the oxygen changes at the deep site might have been smaller.'

Page 12960, lines 19-20: How were the ranges of 24-60  $\mu M$  (intermediate) and 15-101  $\mu M$  (deep) determined? Please explain in detail how those numbers were calculated. Perhaps a table with means and error estimates for each of the events may be useful. From Fig. 5 it seems to me that many interstadial events are above the 235 threshold where the method becomes non-quantitative. So, how exactly were the pre-Heinrich stadial reference values calculated? And how exactly were the Heinrich values calculated.

As the reviewer mentions, most of the interstadial events are above the 235  $\mu mol/kg$  threshold of the calibration equation. For this reason the Heinrich and cold stadial event reconstructions are compared with modern values (245  $\mu mol/kg$  MD95-2042 and 254  $\mu mol/kg$  at ODP 1055) and basically represent the range of [O<sub>2</sub>] observed; at MD95-2042 these are Heinrich and cool stadial values between 144  $\mu mol/kg$  (H4) and 230  $\mu mol/kg$  (C20), and at ODP 1055 they are 194  $\mu mol/kg$  (cool event after C19) and 230  $\mu mol/kg$  (C20). Thus it is also much more straightforward to compare with the range found by Schmittner et al. (2007); model simulated minus model pre-industrial. Our observational values represent the extremes found rather than any averages. We can add a table summarizing the lowest reconstructed [O<sub>2</sub>] at the two sites associated with Heinrich events and extreme cool events.

We can also clarify the main text and add 'compared with modern' to line 15 (Furthermore, while compared with modern the model simulations of Schmittner et al. (2007)...).

Page 12961, lines 11-13: "For North Atlantic Intermediate Water however there is now mounting evidence that this overturning cell was stronger during millennial cool events." I don't agree with this statement. I don't think the intermediate cell was stronger during stadials than during interstadials. I have looked at some of the references provided on page 12958 to support that notion, but I'm not convinced. I think we should be careful in interpreting the radiogenic isotopes. Some of the issues have recently been noted by Hayes et al. (2015, DSR II 116, 29-41).

We can rephrase this line to ' For North Atlantic Intermediate Water however there is now some evidence suggesting that this overturning cell was actually stronger during millennial cool events.'

Supplementary Information: Too little information is provided on how the modern water column data were obtained, processed, and analyzed. Latitude, longitude and cruise information is missing. From the general website provided in the Figure caption to Fig. 2 it is not possible to reproduce the dataset. Please provide detailed steps there were taken and analysis. Are the water column  $\delta^{13}C$  data quality controlled? An alternative global dataset with quality controlled data is available here:

<http://cdiac.ornl.gov/oceans/Schmittner13bg.html>

The data used for Figure 2 (now 3) were quality controlled. We clicked 'DATA EXCLUSION USING WOD QUALITY CONTROL FLAGS' and selected only data with the accepted value (e.g. flag 0). We can provide the original data files from noaa that we used if needed, but they are just as easily downloaded from their website? The data used by Schmittner et al. is the same (same relationship), but it would take quite a bit of effort to group the data for the different ocean basins as is done in our figure, which is very easy to do with nodc.noaa data select and search.