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Interactive Comment

Interactive comment on "Climate impacts on multidecadal $p\mathrm{CO}_2$ variability in the North Atlantic: 1948–2009" *by* M. L. Breeden and G. A. McKinley

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Q1: I find the writing of the manuscript misses much of the necessary details to assure reproducibility. I suggest the authors to consider significantly expanding the section 2.1 and/or provide technical details in the appendix in order to adequately document what went into this calculation, and if any, please discuss significant changes in model parameters and/or bcs from the earlier work.

R1: Thank you for pointing this out. As requested, we have expanded section 2.1 with additional details on the model, and also emphasize the reference to Ullman et al. 2009 and Bennington et al. 2009 that describe this model. We will proceed to include the following details, as requested:

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Q2: What is the timescale used for the SST and SSS relaxation?

R2:SST with a timescale of 2 weeks; SSS with a timescale of 4 weeks.

Q3: Are you using the glacier melt and/or river discharge to force the model salinity? R3: Glacier melt and/or river discharge are not included in the model forcing, instead the SSS relaxation approximates these impacts.

Q4: Are the freshwater forcing consistent between salinity and tracers? R4: Yes, the E-P forcing and SSS relaxation impacts both salinity and tracer concentrations.

Q5: How is the sea-ice dynamics treated? R5: Fractional ice from NCEP Reanalysis 1 is applied, with interpolation to daily.

Q6: How are the open boundary conditions set? R6: A sponge layer exists at 20S, and over the first 5 degrees of latitude to the North, there is restoration to climatological T, S, DIC and phosphate fields. For T and S, there is also a sponge layer at Gibraltar. More discussion of the sponge layer can be found in Ullman et al. 2009.

Q7: I think this is an important problem for calculating pCO2, but maybe the authors can explain what's important for CO2 and how well the model captures it in the N Atlantic.

R7: This simulation is essentially identical to that of Ullman et al. 2009 and Bennington et al. 2009, except for the longer simulation period and the pre-industrial pCO2 that is applied. Given that the surface ocean data to which we can compare has the influence of anthropogenic pCO2, we rely on the extensive biogeochemical comparisons to BATS and subpolar gyre DIC and ALK and pCO2 and SST variability and trends. Here, we compare to the natural carbon uptake estimate from Mikaloff-Fletcher et al. 2007, and include the subpolar gyre DIC profile compared to GLODAP (see Figure at bottom) in the supplementary.

Q8: There are many zonal and meridional WOCE/Clivar transects in the time period following 1990s. As the analysis of DIC variability (3.3) emphasizes the importance of

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the vertical mixing, it would be good to show how well this model reproduce the vertical gradients of DIC and alkalinity in the subpolar regions.

R8: As this is a pre-industrial model run, it is not possible to directly compare the model to data collected in the modern era, as of course the ocean DIC concentration has significantly increased over time. Thus, we compare here a 0-2000m 1948-2009 model DIC profile averaged over the subpolar gyre (35-55N, 5-60E) to the GLODAP (Key et al. 2004) estimate of pre-industrial DIC (see Figure at bottom). The model is largely within the uncertainty of the observed estimate from 0-2000m, and is a few percent below the observed estimate from 2000-4000m . As the average annual maximum MLD of this region is 284m, and the annual maximum MLD for all points in the region is, on average, 3215m, this comparison indicates that the model does capture the vertical distribution of pre-industrial DIC in a manner that is consistent with the available observations. This figure will be included in the Supplementary Material.

Q9: Line 23 in page 15226. Is the 100+60 year spin up enough? Is there a residual drift in the model at the end of the biogeochemical spin up? If any, please quantify the drift with respect to the variability/trend from the simulation period. Fundamental issue here is that the timescale of AMO is comparable to the simulation lengths itself and also the spin-up length. This would raise reasonable doubt unless clearly justified.

R9: Thank you, this point was also raised by Reviewer 1. Our response to their point 4, quoted here is "4. Thank you for this concern, as it has caused us to return to our notes to confirm the length of the biogeochemical spinup. The physical model is spun up for 100 years before the biogeochemical parameters are introduced and spun up with biogeochemistry for, in fact, 100 years. The percent change over the last five years of spinup in the basin-averaged surface DIC field is 0.00046% per year. For comparison, the percent change in DIC from a high AMO (1955) to low AMO (1975) is .012% per year, two orders of magnitude greater than drift at the end of the spin up. Therefore we do believe that a 100-year biogeochemical spinup is sufficient to eliminate model drift in the upper ocean, which is the region of focus for this study. We will include this

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updated information and change comparison in the revised text."

Q10: Definition of "intense"-ness is vague in the 1st sentence of the abstract. Were you considering per unit area uptake rates? In terms of the integrated carbon uptake, it might be smaller than the SH extra-tropics whose carbon uptake is close to 1PgC/yr.

R10:Thank you, we will specify here that "intense" is in terms of the per-unit-area rate of uptake.

Q11: Line 3 in page 15228. Again, please specify whether the freshwater forcing include the E-P from NCEP reanalysis + SSS restoring term.

R11: Thank you, we will include this. E-P from NCEP was included along with the SSS restoring. Line 25 page 15232, it reads as if the logic is inverted where chemistry controls physics. "The AMO is strongly associated with chemical change" should read like "The AMO strongly influences the chemical change". We will modify this text.

References in this response:

Key, R. M. et al. A global ocean carbon climatology: Results from Global Data Analysis Project (GLODAP). Global Biogeochem Cy 18, GB4031 (2004).

Interactive comment on Biogeosciences Discuss., 12, 15223, 2015.

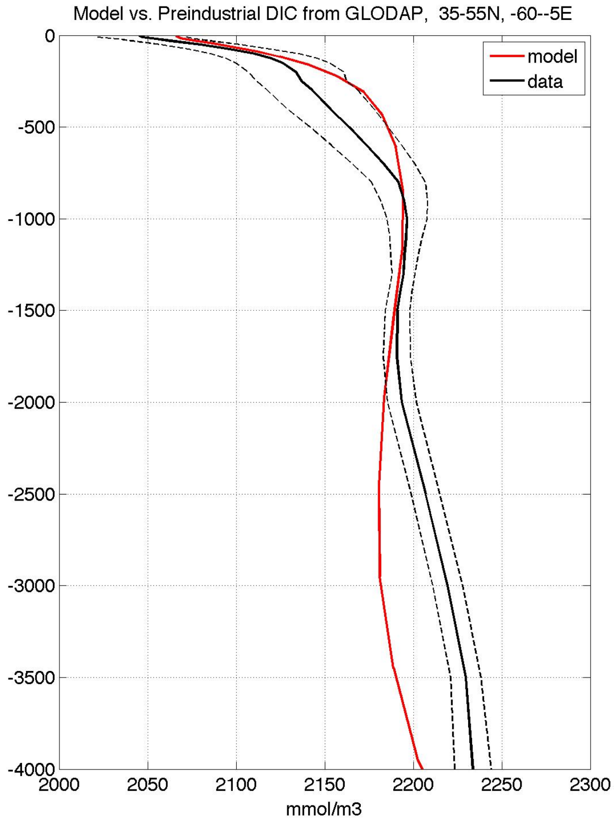

Fig. 1. 35-55N, 5-60E average profile of 1948-2009 Model (red) and GLODAP preindustrial DIC (black) in mmol/m3

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