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Interactive comment on "Climate impacts on multidecadal pCO_2 variability in the North Atlantic: 1948–2009" *by* M. L. Breeden and G. A. McKinley

Anonymous Referee #2

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Comments

This manuscript attempts to establish a statistical and conceptual link between AMO, SST, ocean pCO2 and air-sea CO2 fluxes. The primary effects come from the SST and the vertical transport of DIC as explained by the previous publications from the same group. What is significant in this new paper would be further linking this to the AMO on multi-decadal timescales. The manuscript is written in a reasonably coherent and clear way. While I generally support the qualitative conclusions, I have several suggestions as listed below.

There are many scientifically important figures in supplementary material. I hope that these are placed in the main text. Instead, model description misses many details. These technical details should fill up the supplementary material, not the scientific

content.

Basic statistics and model fields can be compared to observations before presenting higher order statistics such as EOFs. Since the hypothesis involves vertical exchange of DIC, I argue that model skills should be disclosed especially in relation to the vertical tracer distribution in the subpolar latitudes.

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.

What is the timescale used for the SST and SSS relaxation? Are you using the glacier melt and/or river discharge to force the model salinity? Are the freshwater forcing consistent between salinity and tracers? How is the seaice dynamics treated? How are the open boundary conditions set? 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.

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 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.

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.

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.

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

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”.

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