Biogeosciences Discuss., 12, C8392–C8398, 2015
www.biogeosciences-discuss.net/12/C8392/2015/

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

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Received and published: 9 December 2015

Reviewer #1 Major Comments

1) SST and DIC have been identified as the variables of interest. This seems sensible, but because much of the analysis is qualitative (in the sense that patterns are compared with each other, and unit-less time series are compared with each other), the fact that these patterns and timeseries typically match with the explanatory variables does not allow us to definitively accept DIC and SST as the drivers. The analysis (e.g. figure 1 and 2) should also be carried out with the other important candidate driver, alkalinity. Alkalinity may well be important, and for the narrative in the paper to hold up, this needs to be either ruled out, or brought into the story.

R1: Thank you for this helpful suggestion. We have included EOF analysis of the alkalinity contribution to pCO2 in Figure 2 (below). EOF1 explains 19% of the variance of the pCO2-ALK, and has a center of maximum change in the subpolar gyre. The principal component (PC1) of this pattern, however, does not correlate highly to PC1's of total pCO2, and pCO2-chem (r=-.25; r=.44, respectively), or to the AMO (correlations added to the supplementary table 1). This indicates that while alkalinity does have a contribution to the spatial pattern shown in the EOF1 of pCO2-chem, the temporal evolution differs substantially. Conversely, the primary mode of DIC variability, captured in the EOF1 of pCO2-DIC, does correlate strongly with AMO and with pCO2-chem itself. Discussion to this effect will be included in the revised manuscript.

Further, we hope to bring to your attention that the analysis here is not, as suggested, simply qualitative. The variability of pCO2 and its components is quantified in units of uatm (Fig 1-3) and for DIC units of mmol/m3 (Fig 4-5). Variability over time is quantified as the value of the map multiplied by the unitless timeseries.

Revision to Figure 2 is attached.

2. . . In this simulation, the authors use a realistic atmospheric forcing, therefore if they were to widen their definition of the AMO to include the idea (that is gathering weight) that a substantial component of the AMO variability over the interval of interest could be atmospherically forced (rather than resulting from internal ocean variability) - see Booth et al., Nature 2012 - it might be possible to justify the narrative presented here even if the AMOC changes don't fit with those many would suggest are intimately associated with the AMOC variability.

R2: Yes, we agree that consideration of the MOC in the model is important for this analysis. We show the MOC time series in the supplementary material (Figure S6) and find that it is not directly correlated to the AMO, but rather covaries with the NAO (supplementary Table 1,2). This is consistent with Booth et al. (2012) who suggest that the AMO may be driven more by atmospheric aerosol forcing than internal oceanic variability as represented by the MOC. We will include this discussion and citation in the revised text. Further, we will emphasize in the revised text that since this model is a regional model that is restored to climatological T and S at 20S (as clarified in the text in response to Reviewer 2 comments), it should be primarily atmospheric forcing that generates the model AMO. Mechanisms of internal ocean variability involving the Southern Ocean are not present here.

3. . . I would like to have confirmed that the regressions onto the AMO definitely relate to the AMO 'down-and-up', rather than (e.g.) the AMO's trend. Because the AMO is higher in 2009 than 1948, if any of the factors that are look at in figures 4 and 5 also have some trend, the regression could pick this up even if the multidecadal variability were not playing a role. A simpler to understand and more robust figure (in my opinion) would just present difference maps between the high- AMO periods and low-AMO period with everything first detrended. If the vertical mixing narrative presented in the paper definitely does explain the time series in figure 2, this should be very clear in these plots.

R3: What the reviewer describes as his/her desire is precisely what the regression plots in Figures 4 and 5 provide. These fields illustrate the change in each field at each grid point associated with an AMO index of one standard deviation. To consider the change in any field when the AMO has a value of 2 as observed at the end period, one should double the regression pattern, and so forth. This method does not allow the higher value of the AMO at the end of the period to dominate over other periods of time. Additional confirmation can be found with Supplementary Figure 2 in which typically low AMO period (1970-80s) are compared to the typically high AMO periods (1950-60s and 1990-2000s). These illustrate patterns that are quite comparable to the regressions.

4) Finally, it is pretty important to rule out any contributions here from model drift. The authors state that 'drift in the biogeochemical parameters is eliminated' after a 60 year spin-up. Perhaps I'm overly skeptical, but find this somewhat hard to believe - 60 years

is a very short spin-up. Can the authors present evidence for this, or present data from a parallel control run (if this exists)? As noted above, drift could really influence this analysis.

R4: 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 updated information and change comparison in the revised text.

Reviewer #1 Minor Comments

1) I'm not convinced that figure 3 is particularly useful. Is column 1 not essentially just column 3 minus column 2 (based on the definition of the AMO)? In which case, I found the explanation built around this figure overly complicated. I wonder if this could be removed, and the points made with reference to this figure be made instead by just contrasting figures 1 and 2?

Minor R1: We appreciate your concern about potential redundancy in the figures and do recognize that the resulting structures have many similarities. The important distinction here is how the patterns are derived. EOF analysis objectively identifies the spatial pattern explaining the greatest fraction of variability in a field, and also returns how this pattern varies in time with the principle component associated with each EOF spatial field. Though in several cases these principle components have strong correlations with the AMO or SST trend, they are not strictly identical to these indices. It is, however, of strong interest that the dominant mode of variation in pCO2-DIC, in particular, is the AMO without this index being given to the statistical analysis.

Regression analysis, in contrast to the EOF approach, prescribes an index of temporal variation and retrieves the associated response to this index in each pCO2 field. With this technique we are able to isolate the pCO2 and component responses to the AMO (Figure 3, column 1) as separate from the SST trend and to the total SST pattern (Column 2,3) . This distinction is not possible using EOF analysis alone. It is a point we hope to make that these patterns combine in space, and will clarify the text. But we do not feel that these points can be clearly made without Figure 3.

2) I like the use of the barotropic stream function and MLD changes to explain the vertical DIC changes. I wonder if it might be useful to move these into the main paper? I would also suggest it is worth pointing out that the changes are broadly in agreement with the observed changes (e.g. Zhang, 2008, GRL).

Minor R2: Thank you, we agree that these figures are important to the text and shall include them in the main text. We shall add the suggested reference as well.

3) It would be useful if the methodology section could include an explanation of why a regional model was used, and some basic model validation. Currently the only validation that I can see relates to the temporally and spatially averaged N. Atlantic CO2 uptake. Perhaps this is published elsewhere?

Minor R3: Thank you, we have added model validation in response to the comments from Reviewer 2 – please see below. We chose a regional model because we are focused on North Atlantic carbon variability specifically and to reduce the computational requirements for this goal the by limiting the model's geographical scope. Also, as referenced, previous work (Ullman et al., 2009, Bennington et al. 2009) has shown that this model performs well enough to interrogate processes related to North Atlantic carbon variability

5) NASST not defined as far as I can see Minor R5: NASST is defined in Section 2.2

as the total North Atlantic SST index.

6) There are a few minor wording issues that will hopefully be picked up in a revised manuscript - e.g. 'of' missing P15225 line 14-15, trend(s) P15224 line 9. . . Minor R6: Thank you for noting these typos, we will eliminate these errors in the revised manuscript.

Relevant Comments from Reviewer #2 Concerning Model Details

Q:What is the timescale used for the SST and SSS relaxation? A:SST with a timescale of 2 weeks; SSS with a timescale of 4 weeks.

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

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

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

Q: How are the open boundary conditions set? A: 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.

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

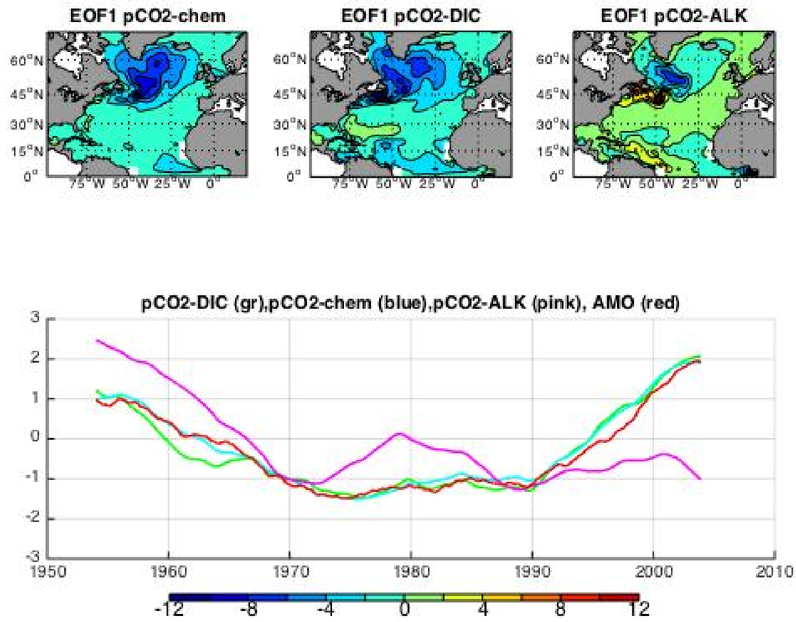

Fig. 1. Top: EOF1 patterns of pCO2-chem, pCO2-DIC, pCO2-ALK units microatmospheres; Bottom: Standardized PC1's of pCO2-chem (cyan), pCO2-DIC (green), pCO2-ALK (pink) and AMO Index (red).