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

Anonymous Referee #1

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The manuscript attempts to shed further light on the important but murky question of what has driven recent observed variability in North Atlantic CO₂ uptake. This is done by exploring the behaviour of a regional ocean model simulation forced with reanalysis atmospheric data (and relaxed to obs.), and is therefore in some senses a simple reanalysis of the North Atlantic carbon cycle from 1948–2009 - but, with atm. CO₂ being held constant, so the authors can specifically examine the climatic drivers of variability (rather than the response to rising atmospheric CO₂). Whilst this work is not directly compared to observations, because it is forced with observationally-based atmospheric forcing, we should be able to learn something useful from this about the real world. The authors identify the major components of multidecadal variability, and attempt to isolate the drivers behind this. Separating out the drivers of variability from a single simulation is extremely difficult, because all of the components of the carbon cycle are linked, but

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the authors take a qualitative approach to this, and present a nice narrative of what could be going on. In my opinion, there are a few further possibilities/explanations that need to be tested, but if after these further tests the story presented here still holds up, I see this as a valuable contribution to the field, and one that would merit publication in Biogeosciences.

Issues that I feel should be addressed:

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 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.

2) On page 15229 the authors discuss the AMO as being internally-driven North Atlantic SST variability. Since the internal variability AMO hypothesis is based on AMOC variability, the authors should present the AMOC time series from the model run. This is important because the internal ocean circulation variability is likely to have an impact on the surface CO₂, both through the meridional transport of DIC and alkalinity, but also because the surface circulation responds to the AMOC change with (for example) a substantial subpolar gyre circulation and temperature response. 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.

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3) If we assume that after addressing my point 2 above, DIC is still the front-runner for explaining the non-SST driven variability, figures 4 and 5 become key in explaining the mechanism behind the DIC change. Whilst the evidence presented here fits nicely with the paper's narrative, 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.

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.

More minor points:

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?

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

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with the observed changes (e.g. Zhang, 2008, GRL).

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?

5) NASST not defined as far as I can see

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. . .

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