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Interactive comment on "Drivers of 21st Century carbon cycle variability in the North Atlantic Ocean" *by* Matthew P. Couldrey et al.

Anonymous Referee #1

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A. Summary and recommendations

In this manuscript, the authors assess the timescales of carbon cycle variability in the North Atlantic region of a numerical ocean circulation model under global warming conditions. By partitioning the carbon inventory into constituents associated with different physical and biogeochemical processes, the authors attempt to discern the drivers of variability on timescales from inter-annual to multi-decadal. The main result is that inter-annual variability in the carbon inventory is driven by changes in the saturation state of carbon, which itself arises from changes in temperature and preformed alkalinity. Other processes, including the biological carbon pump, play a more significant role on decadal to multi-decadal timescales, and the long-term trend is established by the uptake of anthropogenic carbon.

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The manuscript is well-written, and the figures well-presented. Overall, the analysis is interesting, but in my opinion incomplete. In particular, the authors have not taken sufficient advantage of the benefits conferred in using a numerical simulation, whereby the full carbon budget can be determined. Instead (unless I am mistaken) they evaluate variability only in the storage terms of total inorganic carbon and its constituents, leaving the author to wonder whether this reflects drivers of air-sea exchange or fluxes across the open domain boundaries – which appears to me to be of critical importance in interpreting the results. I elaborate on this issue below, and invite the authors to correct me if my understanding is mistaken. This concern, in combination with other issues outlined below, means that although the work inspires some intrigue, it has too many ‘open doors’ to provide a robust increase in our understanding of NA carbon cycle variability and thus to be considered ready for publication at this stage. I would invite the authors to consider resubmission after a more comprehensive budget analysis has been carried out.

B. Major issues and considerations

B.1. Linking inventory changes with variability in air-sea exchange.

As noted in the summary, I have a substantial concern with regards to the absence of a closed budget analysis. If the goal is to assess the drivers of ocean carbon uptake variability (which I perceive as the main thrust of the study), variations in the basin inventory of total carbon and its constituents confers relevant information only when the variability in lateral fluxes of carbon across the domain boundaries are either evaluated or considered negligible. As far as I can tell from the manuscript, neither of these has been done or shown. The authors comment on page 2, lines 9 to 11 (p2:9-11) that “variability in oceanic carbon concentration is a leading-order control on the amount of CO2 that the oceans absorb”, and offer references in support of this. However, this offers little information about the timescales or regions over which this is case.

It is plausible that changes in the carbon transport across lateral boundaries plays

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a non-negligible role in the variability of the basin-wide carbon inventory. A back-of-the-envelope calculation suggests a magnitude for the net carbon transport at either the northern or southern boundary on the order of 0.01 PgCyr-1 (volume transport of 10 Sv, mean seawater density of 1000 kgm-3, mean total inorganic carbon difference between inflowing and outflowing waters of 20 μmolkg-1). I suspect that this is a rather conservative estimate, and I would invite the authors to calculate the actual mean flux in the model. It is further feasible that, through volume transport changes alone, this could vary by as much as 100% on inter-annual timescales and longer (e.g. inter-annual variability at 26N as observed by the RAPID array; I am unsure if the same extent of variability also exists in the model, or at the equator where is the southern boundary of the analysis domain). Figures 2a and 3 indicate that changes in the carbon inventory are in the order 0.1 PgCyr-1 and less (except for the long-term trend). In my interpretation, these numbers indicate that convergence/divergence of carbon in the basin by transport across the lateral boundaries at least cannot be excluded a priori as a source of basin-wide carbon inventory change.

This convergence/divergence can of course be readily calculated in the model, and thereby easily ruled out as a source of variability. Or, if it is not negligible, it can be subtracted from the inventory to leave air-sea carbon exchange as an inferred residual (indeed the air-sea exchange could also be evaluated in the model, to ensure that the carbon budget, and that of the constituents, are in balance). It strikes me that this is an appropriate and necessary step to take before valuable information can be derived from the analysis. I am confident that the authors are aware of this, and am thus left unsure as to why they appear to have abstained from incorporating a full carbon budget closure as a key part of their analysis. This leaves me to wonder whether there is something that I have missed, or misunderstood. For example, perhaps a closed-budget analysis of the carbon constituents is not feasible because of using differences between simulations to get some of the terms and/or what is done to remove the model drift (though I cannot see clearly why this would be the case). If I am mistaken in my assessment, I would invite the authors to clarify their reasoning and their approach, pointing towards the analysis or literature that justifies neglecting variability in lateral transports. Without such a justification, the results are left unsatisfactorily open and challenging to interpret. For example, it may well make sense that inter-annual changes in surface temperature drive changes in carbon saturation resulting in carbon uptake variability, but the absence of a closed budget prevents one from directly making this assertion.

B.2. Redundant analyses.

Section 4.4, which explores the correlation between saturated and total carbon appears to me to be somewhat of a repetition of parts of Section 4.2, in which total carbon is shown to be strongly correlated to saturated carbon on inter-annual timescales. The only difference that I can perceive is that, in the latter case, preformed alkalinity is inferred from its relationship to salinity. Thus, under the guise of showing the explanatory power of saturated carbon variability (Figure 7b), you are simply repeating the assertion that these variables are correlated (Figure 5b) and additionally that an approximated preformed alkalinity has variability consistent with the 'actual' preformed alkalinity as determined in the model. As noted in the text (p15:30-31), this latter point is of some value with regards to the observation of carbon inventory variability in the ocean, but in my opinion (unless I am missing something critical about the information conferred in Section?) does not merit the protracted analysis of Section 4.4 – that is to say, there would be easier and more robust ways to show this. Further showing the modest change in explanatory power afforded by the inclusion of soft tissue carbon and dissolved carbonates is again a repetition of the information conferred in Figure 5b.

I wonder if a more valuable analysis would be to fold aspects of Section 4.4 into the previous section, by considering how variations in temperature and preformed alkalinity change the explanatory power of saturated carbon? As explained further below, I am still struggling to fully understand how the two components interact.

B.3. Spatial distribution of variability.

I found Figure 4, showing the spatial distribution of carbon inventory variability, to be rather insightful. Although caution in its interpretation is advised by the authors (p11:7-9), it still strikes me that a lot can be inferred about the potential driving processes of carbon uptake from these maps. [As an aside, I could not find anywhere an explanation for how these maps were constructed in terms of separating out the timescales of variability. I presume that the column inventories were band-pass filtered.] While the authors list hypotheses (p11, paragraph beginning line 11), they do not describe what aspects of Figure 4 lend themselves to these interpretations. Indeed, this list of hypotheses could have been drawn up even before the simulations were run. It would be valuable for the authors to elaborate more fully on what the spatial distributions tell us about the most likely sources of variability on different timescales. In line with this, it seems like an obvious further step to reproduce Figure 4 for the different constituents of the carbon partitioning. Even though one cannot definitively attribute spatial changes to basin-wide variability, there is much more to be learned from these distributions about the processes establishing carbon uptake variability.

Have the authors considered producing maps such as this in density-latitude space (that is, taking the zonal integral of carbon and constituents in density bands)? I suspect that this would confer substantial further information about the driving processes of variability on different timescales. For example, the density (or depth) range in which variability in soft tissue carbon (combined with carbon disequilibria) emerges, may reveal whether these changes reflect changes in production or export (p12:1-2), or indeed subsurface remineralization.

B.4. Robustness and clarity of statistical analysis.

The statistical analysis in the paper is straightforward, and thus commendably easy to follow. However, it hard to countenance in the absence of any tests of robustness, or reference to literature on time-series analysis. This is not a subject area that I know well, but I presume that there is a rich literature on the attribution of variability to co-varying components, including separation in time bands. Is the assessment of linear correlations and root-mean-square deviations an appropriate and robust method for doing this? If so, are there caveats or intricacies in the interpretation? Note that I am not criticizing the authors' approach off-hand, but simply highlighting that in my opinion the reader is given insufficient information or detail to convince themselves of the scope and implications of the analysis. An example of where uncertainty arose for me, is in the attribution of multi-decadal variability (Figure 5g, h, i). The authors note that saturated, soft tissue and anthropogenic carbon all have an RMS comparable to total carbon, high correlation (with saturated carbon negatively correlated), and strong linear gradients. The authors conclude that "large variability in Csat and Csoft components is approximately compensatory, and variability in the accumulation of anthropogenic carbon dominates the variability of the North Atlantic carbon sink" (p14:6-8). Is there an aspect of the statistical analysis that precludes an alternative interpretation, that variability in the saturation state compensates anthropogenic carbon uptake, and that biological drawdown establishes multidecadal variability in the carbon sink. I have the impression that either hypothesis is difficult to assert with the present analysis, but invite the authors to provide more detail on why that might not be the case.

B.5. Model validation.

I'm afraid that the authors' model evaluation did not convince me that "the model can be judged to be reasonable and our setup is fit for this study". I should state up-front that I am not a pedant for perfect model-observation matching. Simulations can of course look distinct from observations but still provide valuable insight. However, if the authors wish to infer (as it appears from the writing of the manuscript) that their results inform our understanding of ocean processes (rather than simply those operating in the model [understanding of which can valuable on its own]), specific components of the model-observation comparison should be robust. The authors have made some attempt to explore this but, in my opinion, have so far failed to sufficiently show that the simulations are fit for purpose. Most crucially, the thrust of the authors' approach is to assess the temporal variability of carbon inventories. It does not make sense to me therefore, why in their statistical comparison with GLODAP data (Figure 1), they include both spatial and temporal variations in the frequency distribution (panels a and d). Perhaps there is something that I have missed, but I cannot see that this provides any relevant information on how reliably the model reproduces ocean carbon variability. Indeed, I suspect that most of the distribution in Figures 1a and d reflects spatial variability, with some component associated with seasonality in the upper ocean. [As an aside, the x-range of the axis in panels a and d should be narrowed to show clearly the appropriate range.] This leaves me unsure what such a distribution actual informs us about the utility of the simulation. Indeed, it could be the case that the mismatch between models and observation in this frame is almost entirely due to spatial bias, and that the models captures the temporal variability well, but I cannot infer this from the present analysis. Of course, the GLODAP data is limited in what information it can provide about temporal variability (although decadal trends, and repeat sections, may be of some value). Comparison with data such as the HOT or BATS time-series, although not necessarily straightforward as the authors note (p6:28-30), would provide more robust assessment of how the timescales of carbon cycle variability in the model match those of the real world.

B.6. Inter-annual variability of preformed alkalinity.

In Section 4.3, the authors show that inter-annual variability in saturated carbon arises from changes in both temperature and preformed alkalinity. It is clear to see what processes might drive temperate variations. What I am left wondering is what process establishes inter-annual variability in preformed alkalinity? Given the apparently small variability in biology on these timescales, I can't believe that this arises from changes in the formation/dissolution of carbonates or the formation/respiration of organic matter. Is this then a signature of variations in dilution/concentration, presumably by inter-annual precipitation/evaporation? Or is this the result of convergence/divergence of preformed alkalinity at the lateral boundaries? On the other hand, is it simply a quasi-spurious result of the statistical analysis that requires more careful interpretation? These questions are left inadequately explored in the study, and I was left unsure how to interpret the analysis.

C. Conclusions

In conclusion, while this study provides some potentially interesting results, the analysis remains incomplete. In particular, the approach leaves too many open-ended questions for reliable conclusions to be drawn about the operation of the North Atlantic carbon sink, either in the model or in the ocean. Some substantial but relatively straightforward steps (such as the closure of the basin-wide carbon budget) could be taken to improve the reliability and ease of interpretation of results. I would advise the authors to explore a more robust, rounded approach, and then consider resubmission.

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