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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 #2

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In this study the ocean carbon pool is partitioned into different contributions (anthropogenic, soft, carbonate, saturated and disequilibria) and the significance of these contributions on controlling variability in different timescales in the North Atlantic is investigated using a realistic numerical model. The highlighted conclusion is that on interannual timescales the variability is driven by the effect of the ocean temperature and preformed alkalinity changes to the ocean uptake of carbon, while on decadal-multidecadal timescales the changes in the soft tissue pump also become important. The anthropogenic carbon component is not driving the variability on interannual timescales but becomes increasingly important on following timescales and dominates on long timescales.

The study is interesting, worthy of publication and fits the scope of the journal. The

results are well supported by statistical analysis, strengthening the authors arguments. The statistical methods are described in detail and the overall manuscript reads well. However, to me, in places there is too much focus on the statistics and repetition, rather than a focus on the insight gained from the analysis. I also believe that the figures showing the correlations between different processes and the different carbon contributions should be presented in the main text rather than be buried in Appendix F. Further discussion on the different mechanisms that lead to these correlations is also necessary in my opinion, as I personally find this part of the study to be very interesting. Finally, although the manuscript reads well, I believe that a re-organisation of Section 4 would benefit the manuscript. Based on the above and some other issues discussed in my following comments, I recommend the following revisions/concerns be addressed before the manuscript be accepted for publication.

Major comments:

1. Section 4 is to me somewhat repetitive and in need of re-organisation. I suggest that the whole section is re-organised into discussion driven by the timescales such that there are 3 subsections (interannual variability, decadal-multidecadal variability, and long-term variability).

1.a. Section 4.1: I found some of the statistical analysis repetitive and unnecessary. In my opinion, the timescales decomposition using the FFT, and so Figure 2b, do not add any new insight that cannot be derived by the following analysis using equations 2-6. Therefore, I think this analysis is redundant and that the associated text, Appendix G and Figure 2b should be removed. Instead, I suggest that the authors expand somewhat on the discussion concerning the analysis using equations 2-6 so that all the carbon contributions are shown for the different timescales along with the total in each panel of Figure 3. Else if the authors do not want to crowd the figure, they may repeat this figure for the different carbon contributions. I believe this visual comparison is intuitive and will complement the results from statistics in Figure 5.

1.b. Likewise, in my opinion, Figure 4 should be repeated for all the constituents. As it is now, I gain no insight from the text on page 11 last paragraph continue to page 12. In this paragraph the authors simply suggest plausible processes/hypotheses. Instead, I believe that the authors should isolate which of these hypotheses may apply to their results based on the maps of Figure 4 repeated for all the carbon constituents. Then the statistical analysis will complement, refine and confirm the insights gained by these maps.

1.c. Section 4.4 “Role of Csat in interannual DIC variability”, to me this discussion is somewhat redundant. In my opinion the authors already explained that Csat, and so solubility and preformed alkalinity dominate the interannual variability. As I understand, the novelty of this section is the empirical estimates of preformed alkalinity from salinity, and Csoft from AOU to demonstrate that temperature and salinity observations can to first order be used to evaluate the DIC variability, with the AOU observations having the potential to further improve this evaluation. Hence, I suggest that the authors substantially shorten this discussion. If the authors follow my advice about reorganising section 4 into 3 subsections driven by timescales, this section will naturally merge with section 4.3 and the previous discussion about the interannual timescale into one section.

2. Section 5 and Appendix F. In my opinion, the figures in Appendix F are interesting and I think that they can be highlighted better if they are in the main text (merge F1-F3 as a single figure in section 5). Subsequently, I suggest that these correlations and the mechanisms/processes that drive them are discussed further in Section 5. Although some of the correlations are explicitly explained (AMOC-Csoft), others are only discussed implicitly or not at all. For example, why is the correlation between AMOC and Cdis positive only on decadal and longer timescales (Figure F1)? Why is the correlation between open boundary and Cdis positive on multidecadal timescales but negative on longer time scales (Figure F2)? I suggest that at least the sign of the significant correlations in Figures F1-F3 is discussed and explained.

Minor comments:

3. In the Abstract, line 10, “A mixture of saturation and anthropogenic drivers”: I suggest that the authors rephrase since the anthropogenic drivers may be misinterpreted as both due to changes in atmospheric CO2 and in climate (e.g., temperature); but the later is accounted in the saturated part in this study. Maybe instead use “. . . of saturation and carbon uptake driven by the anthropogenic increase in atmospheric CO2”, or something equivalent.

4. Page 4, starting paragraph in line 7: “Other work has investigated in detail . . .other work has focus on the spatially varying . . .”. Which other work? Does this refer to the studies referenced in the previous paragraph? Maybe the authors can clarify or reference here this other work.

5. Page 4, lines 17-19: “The main hypothesis is that oceanic uptake. . .”. I am a little confused by this statement. Is this hypothesis established by previous studies? Is it an a priori assertion of the authors? If the later, I find it a little confusing that this hypothesis does not match the study’s results. In my understanding, contrary to what is stated in this hypothesis, the study shows that interannual variability is driven mainly by the saturated component rather than the saturated component and soft tissue pump, and decadal variability is dominated by the saturated part rather than the anthropogenic part. Maybe the authors should rephrase or use a more generic hypothesis like: The main hypothesis is that different processes will drive variability on different timescales.

6. Page 5, line 28 “to distinguish physical from the biogeochemical ocean carbon cycle. . .” and subsequent use of “physically adjust” in line 31. In my opinion the use of physical here is not accurate. To me the physical part would be the part driven solely by the changes in physical processes due to warming e.g. circulation, stratification. The terms “physical” and “physically adjust” as they are used include the effect from the solubility changes due to changes in temperature and the effect of changes in alkalinity which I would consider as a chemical rather than a physical process. I would suggest to please rephrase.

7. Page 5, line 33: “non-steady state anthropogenic carbon ..”. I am a little confused here, I think that in your warming only run the carbon inventory changes would rather be associated with the non-steady state of both anthropogenic and natural part as described in McNeil and Matear (2013). I may be mistaken but please can you clarify.

8. Page 6, subsection 2.2. Are the Csoft and Ccarb the same in both the warming only and the anthropogenic simulations? If not, then to me it seems that some of the changes in biology would be accounted for in the Canth. Is this correct? Can you please clarify?

9. Page 7, Figure 1. Please can you add in the figure’s caption that the right panels are for the GLODAP climatology and an equivalent “climatology” from the model rather than the unmapped database.

10. Page 11, Figure 3. Is this figure showing the root-mean square of DIC anomalies of long-term mean as stated in the caption? I think this is a typo and this figure shows the filtered carbon inventory time series following equation 2-6. If not, then I am confused as to how this root mean square is estimated and what it represents? Please can you clarify?

11. Page 20, line 25 “. . . can be estimated to first order by quantifying saturation effect”. I understand that the authors mean their saturation component here but since this is the conclusion section, I think that they should be more explicit. Saturation effect in general in my opinion includes both changes due to atmospheric CO2, temperature, and alkalinity which may cause confusion. Maybe rewrite to something more like “ . . .to first order by quantifying the effect of solubility and alkalinity”

12 Typos: page 8, line 8: alkalinity is misspelled; page 21, line 23: an “is” is missing such that “ . . . interior, it is possible to estimate Apre. . .”

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