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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.

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Response to Reviewer 1:

A. Summary We are grateful for the thorough and engaged review that Reviewer 1 has provided. We agree that the manuscript would benefit from some further analyses, especially regarding a closed budget approach. In addition, we aim to explore our results in more detail, as suggested.

B1 We agree that the exchange of DIC and its components across the lateral boundaries of the North Atlantic is worth exploring quantitatively. We have begun this analysis, and agree this it should be incorporated into the work.

B2 While we found it interesting to explore the extent to which it was possible to derive

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interannual DIC variability using a very limited set of predictors, Reviewer one is correct that the underlying message of Section 4.4 is the same as Section 4.2. Reviewer 2 also felt that this analysis was repetitive of the point that saturation effects dominate interannual variability. As a result, the section will be removed and relevant parts will be included elsewhere.

B3 We agree that Figure 4 and the text describing the list of hypotheses should be drawn together more closely. The aim of the section was to get an overview of which processes might drive variability at different timescales, using inferences drawn from the hotspots highlighted by the figure. This section will therefore be clarified to more clearly illustrate how particular features of Figure 4 implicate particular hypotheses about the basin as a whole.

B4 We thank the reviewer for asking for this clarification about the inferences drawn from our statistical analysis. Reviewer 1 makes the point that this statistical approach alone does not make it clear how any components of DIC compensate each other when more than two components dominate variability over a particular timescale. The reviewer is correct, and that more information is required to determine, for example, on multidecadal timescales whether there is compensation between just Csat and Csoft or between Csat and Canth (or some mixture of the two). Our approach provides a robust method of attribution of total DIC variability to individual components (since the total variability is exactly equal to the sum of the parts). As a result, most of the assertions of the manuscript are supported by this statistical analysis (for example when one or two components dominate variability as is the case on interannual and decadal timescales, and for the long term trend). As the reviewer correctly points out, it is not possible (with the present analysis alone) to determine which components compensate other components when 3 or more vary substantially. Instead, such statements will be revised to communicate that multidecadal Csoft and Canth variability act in opposition to that of Csat, and that all components are important in setting the total variability.

B5 Comparing observations and model output at the basin scale is challenging because there is no consensus on how best to make like-for-like comparisons when scaling up point observations to the model gridpoint scale, or the other way around. The analysis shown in Figure 1 is useful because it allows for a relatively 'raw' comparison regardless of the spatiotemporal distribution of observations. Quantifying carbon cycle variability at the basin scale from observations like GLODAP is a serious undertaking in its own right, even before any comparison with model output is attempted. Nevertheless, we will include some new validation work that aims to better leverage the temporal information in the GLODAP dataset. In particular, this will focus on the largest spatial scales for which repeat hydrography is available in the North Atlantic.

B6 Reviewer 1 raises interesting questions about the role of preformed alkalinity in interannual variability. This manuscript certainly highlights that a detailed investigation of alkalinity variability in the North Atlantic is warranted. The inclusions of a closed budget analysis will shed some light on this point, but a thorough study is beyond the scope of this work and is left to future research. To address this point, however, more interpretation of our results will be included. We agree with the reviewer that biological variability is unlikely to dominate on these timescales (since soft tissue carbon variability is small). Concentration/dilution effects are likely to be important or leading order (this is why, for example, it is possible to estimate much of the interannual DIC variability using salinity as a predictor of preformed alkalinity). The effects of transport across the lateral boundary will be addressed with the inclusion of the closed budget analysis. However, other effects are also important. In this study, interannual variability in preformed alkalinity depends on 1) surface alkalinity (concentration-dilution effects due to the hydrological cycle primarily) and 2) the location and intensity of water subduction. It is not trivial to distinguish these processes, and would make a good focus for subsequent work.

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