**Biogeosciences
Discussions**

Interactive comment on "Contrasting effects of temperature and winter mixing on the seasonal and inter-annual variability of the carbonate system in the Northeast Atlantic Ocean" *by* C. Dumousseaud et al.

Anonymous Referee #1

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General comments:

The manuscript by Dumousseaud et al. endeavors to test the hypothesis that warmer temperatures associated with anthropogenic climate change will result in shallower winter mixing depths, in turn leading to reduced primary production and carbon draw-down. The manuscript contributes a nice case study on the relative contributions of water temperature and winter mixing depth on the uptake of CO2 by ocean waters, but cannot represent a comprehensive test of this hypothesis on the basis of two years data. During the two years of their study, temperatures were not the only key forcing

parameter that varied – winds also differed significantly between the two summers, and the authors showed that this was an important consideration. Despite this issue, the manuscript represents an important contribution of new data and warrants publication. However, I would recommend revising the manuscript (and perhaps the title) to reflect several considerations that I detail in the following section as well as to reduce the emphasis on this hypothesis test somewhat, given the short time-series available and confounding factors.

Specific comments:

-One major question I had in reading the manuscript was the extent to which the study area reflected coastal vs. open ocean processes. The answer may be that both are reflected, in the English Channel and the Bay of Biscay, respectively, but this could be better delineated in the text. The authors seem to focus their introduction and discussion on the linkages to the North Atlantic, where deepwater formation is critical to the global carbon cycle. It would be nice to see more discussion on what the relevant coastal processes are in this region and to what extent, if any, the study area plays a role in the net North Atlantic uptake of CO2 and deepwater formation.

-On p. 9706, lines 6-9, the authors say that they used fCO2 data from the Santa Maria along with TA data from the Pride of Bilbao to calculate DIC values. I'm not at all clear on why this was done when the authors measured DIC also. Furthermore, given the very large variability of CO2 in coastal oceans that has been observed on short spatial and temporal scales, I question the validity of using fCO2 and TA from different ships that were collected at different times of places. At a minimum, it needs to be clarified what the benefit/goal of this approach was and how close in space and time the measurements on the two ships were, but I would suggest deleting this approach - based on what I understand from the text it does not add anything new.

-For future studies, I would recommend that the authors consider measuring atmospheric CO2 directly rather than relying on distant atmospheric sampling stations. On

a recent cruise in North American coastal oceans, sufficient variability in atmospheric CO2 was observed on the time scale of the cruise to affect the calculated air-sea CO2 fluxes. I believe that this work is not yet published, but I saw it in a poster presentation in the last two years. For coastal work, it can be important.

-Section 2.4 – The authors do not make it clear why coccolithophore abundance is being tallied in this section. Also, what exactly is the "image analysis" that is being done? It is not described clearly.

-The labels on figures 2, 3, 4, and 6 are much too small to read clearly, even at considerable magnification on my screen. Also x-axis labels on panel D should not overlap the data. Finally, it would be nice to see consistent units (i.e. umol/kg and mmol/kg for nitrate and oxygen, respectively, rather than umol/kg for one and mmol/m3 for the other). Also the caption and graph labels do not match for Fig. 2c.

-The authors don't clearly explain how the dissolved O2 anomaly of Bargeron et al. 2006 is calculated. In order for the reader to follow the subsequent discussion, I would suggest putting the equation in the text. Based on how it's currently written, I couldn't see where the supersaturated vs. undersaturated values were supposed to be on their figure (given that almost nothing had negative values in fig. 2d, which I assumed would indicate undersaturation).

-Same thing for the TA and DIC normalization techniques – show the equations so that the reader can follow the discussion better. However, it is not very clear why the authors are normalizing TA and DIC anyway. They seem to end up concluding that this isn't very informative in coastal waters, as I would have expected, so maybe this can just be deleted in the text and figures?

-Are the fluorescence units "arbitrary" because the waters are "optically complex case II" coastal waters or is this typical of how fluorescence is usually reported with this type of system? I am more accustomed to seeing chl values reported in mg/m3 or similar, but admittedly, this is not my expertise. A few words on how these measurements differ

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may be helpful.

-I gather that coccolithophores were just sampled from the underway system. If this is the case, how do they know they got representative sampling? I know for many ecosystems, there is a deep chlorophyll maximum. I have no idea what the depth distribution profile would look like for coccolithophores, but do have questions about whether the sampling from the underway system is representative. If it is not, the authors calculations on how much carbonate precipitation or dissolution could contribute to the observed changes in TA are not very meaningful. In general, I did not find the discussion on this topic to be sufficient (e.g. no effort was made to determine relative contributions of freshwater inputs and nitrate uptake to observed TA changes).

-With respect to the lines for different regions on all data graphs, it would be nice to make the symbols big enough that one could differentiate the shapes and put a visual key in, instead of text indicating the lines are red, green, etc., for people who are color-blind (10% of males are red-green color-blind).

-I'm not clear on why the authors are calculating TA from S data following the Lee et al 2006 algorithms to "validate" their TA data. Validation doesn't seem necessary per se, given that their data were measured using CRMs, replicates, etc., with excellent precision. It is nice to see that Lee et al algorithms seem to hold up reasonably well in these more complex coastal waters – it is certainly not the case everywhere, nor was it the intended use of the algorithms.

-I find the wording of the table 2 caption to be confusing. The paragraph discussing these results (starting on line 27 of p. 9713) also seems kind of out of place in the middle of the MLD discussion. This section could benefit from a little reorganization and clarification.

-Could you overlay the monthly NAO index values that you don't show in section 3.6 on the fig 5 mixing depth (lower) panel? It would be nice to see these values.

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-Fig 6 – The authors should revise this figure so that the labels are not superimposed on the data.

Technical corrections:

-Re: wording on p. 9712 line 13, instead of "the DIC distribution showed an increase" it would be better to say "DIC values increased"

-On p. 9713, lines 8-10 – What is meant by "the increase in atmospheric forcing over the winter of 2004/2005…"? Make the wording more specific so that the reader knows whether you mean air temp, wind, etc.

- On p. 9713, line 13 – is this a typo? There haven't been records kept for 500 years… (50?)

-Throughout the text, there are some minor grammatical mistakes that I don't have time to list individually. It does not interfere with the overall readability, but a copy editor should be able to clean them up quite easily.