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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.

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Reply to anonymous referee #1:

We would like to thank the reviewer for his/her valuable comments which helped us improve this manuscript. Our responses to the specific comments and technical corrections are listed below:

- 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. - We appreciate this comment and have further explained this in section 3.7 where the air-sea CO2 fluxes differences between the two regions are discussed.

-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. - The positions of the two ships were very close to each other (as seen on Figure 1) and the comparison was done to validate/corroborate the DIC data. The correction has been further detailed and clarified in the main text.

-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. - This is an important comment and this is something we will endeavor to do in the future. However, the time series from the sampling station is best suited to our purposes, given that we are estimating time-averaged rather than instantaneous fluxes.

-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 image analysis consisted in the counting of the coccospheres on each scanning electron micrograph (SEM) taken. The text has been modified for clarification.

-The labels on figures 2, 3, 4, and 6 are much too small to read clearly. 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 figures have been modified to make the observations clearer. The nitrate unit in the figure has been modified to match the text and the caption.

-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). - The equation has been added and the text clarified.

-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. - The equations have been added in the text.

-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 may be helpful. - Fluorescence measurements can be affected by different factors,



including light intensity, and can also differ between systems. Such measurements are therefore only used as an estimate of the phytoplankton biomass and are generally reported in arbitrary units.

-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). - The authors agree that TA changes can also be related to freshwater inputs (hence the normalization) and nitrate uptake. However, the authors consider that the TA distribution did not show large variability compared to other variables such as nitrate and DIC. For clarity reasons, the calculation of the TA variation estimated from the coccolithophore abundances has been removed as it does not represent a key point in the manuscript.

-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 colorblind (10% of males are red-green color-blind). - Ok, the figures have been modified

-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. - The Lee et al. algorithm was used more as a comparison and this sentence has been modified.

-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. - Thank you, this has been clarified and this paragraph has been moved to a different section.

-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. - The winter NAO index was used (December through March), and therefore adding these values would not benefit the figure. The values were added in the text in section 3.6 instead.

-Fig 6 – The authors should revise this figure so that the labels are not superimposed on the data. - Thank you, the figure has been changed.

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

-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. - This has been clarified and changed to "cold and dry".

- On p. 9713, line 13 – is this a typo? There haven't been records kept for 500 years (50?) - This is an estimate and it has now been clarified in the text.

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Interactive comment on Biogeosciences Discuss., 6, 9701, 2009.