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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.

Anonymous Referee #2

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The data presented in the manuscript by Dumousseaud et al suggest that the deeper MLD during the cold 2005/2006 winter, associated to a negative phase of the NAO index, has caused higher surface concentrations of DIC and nutrients in surface waters, compared to the warmer 2006/2007 winter during which the NAO index showed a positive phase. The intensity of the following spring blooms was accordingly higher in 2006 than in 2007, based on fluorescence and dissolved oxygen anomalies. These data were also supported by SeaWIFS time series of Chl-a. The effect described above was important in the Bay of Biscay but did not appear in the central or western English

Channel.

The authors make the conclusion that the cold winters lead to higher productivity during the following growing season, in agreement with several recent publications. By comparing two situations (cold winter/higher spring productivity and warm winter/lower spring productivity) they further argue for an expected lower uptake of CO2 by the ocean, due to a stronger warming from winter to summer and decreased solubility of CO2 in seawater despite a stronger intensity of the phytoplankton bloom. This is in contrast with previous findings and it highlights the importance of local forcings like wind-speed for the computation of Air-Sea CO2 fluxes. I'm not confident whether this case of study could be generalized as a general trend for the future since the presented dataset of 24 months cannot also compare the effects of strong wind intensity following a cold winter or the effect of lower wind intensity following a warm winter. Hence, I suggest the last two sentences of the conclusion are rephrased in this sens.

Beside this, the dataset provided by Dumousseaud is presented as unique for carbonate chemistry and coccolithophore abundances. Nevertheless, for the later, only 3 values are presented: 0.9×106 cells L-1 in May 2006, 0.4×106 cells L-1 in July 2006 and a base line lower than 0.1×106 L-1 for the remainder of the cruises. This does not constitute, in my opinion, a "unique" dataset for coccolithphore abundance in the English Channel and the Bay of Biscay, and the introduction should be adapted accordingly.

With regard to the second dataset presented by the authors, carbonate chemistry, many points remained unclear to me when reading the Material and Methods and the Results sections. Therefore, I would recommend revising the manuscript for the following reasons: 1. A very detailed protocol is provided by the authors for the sampling and the determination of the carbonate parameters (sections 2.2 and 2.3), pointing out the accuracy and the precision of the measurements of TA, for example. However, it is not clear to me whether TA and DIC samples were filtered or not, since the presence of particulate calcium carbonate phases (in the form of coccolithophore remains, coccol-

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iths or forams) in the water column is a major interference for the titration of TA and the determination of DIC by coulometry. 2. The authors use their TA values from the Pride of Bilabo transect with external values of fCO2 (calculated from xCO2 obtained using an equilibrator-based system and infra-red absorption during the Santa Maria cruises) in order to correct DIC by a factor 2 % in 2007. They applied this correction "despite no evident problem with the analysis" (L. 149) was stated except the agreement was wrong for the first 6 months of 2007 (from February to July 2007). Such a correction of DIC, in my opinion, is fuzzy and the text poorly justifies its application to the 2007 dataset. This part needs to be clarified. The speciation of dissolved carbonates in seawater and the computation of seawater fCO2 (or pCO2) can be deduced with accuracy from the couple (TA, DIC), along with nutrients, T and S measurements with the CO2SYS program. Therefore, I suggest to the authors to compare, on the figure 7, the pCO2 retrieved from the Santa Maria transects to those retrieved from the Pride of Bilbao transects (without the 2 % correction on DIC) in order to validate the dataset presented here. 3. "The calculated DIC values were compared against the measured values and showed good agreement ($\pm$ 4-6 μmol kg-1), with the exception of the February, April, May and June 2007 data" (L. 146-148). This period corresponds to the occurrence of cocolithophore blooms in the Western English Channel and the Bay of Biscay (e.g. Garcia-Soto et al., Evolution and structure of a shelf coccolithphore bloom in the Western English Channel, Journal of Plankton Research, Vol.17 iss. 11 pp.2011-2036, 1995) during which large amounts of particulate CaCO3 can be released into the water column. In the case where DIC and TA were not filtered to remove CaCO3, this would help explaining that aberrant values were found at this period. 4. "Seawater samples were pre-filtered through a 200 μm mesh to prevent zooplankton grazing during the filtration" (L. 158-159). Which was the volume filtered for coccolithophore abundance determination? Could you show the time series of coccolithophore abundances? It would be helpful to compare also time series of coccolithophore abundances with remote sensing water-leaving radiance or remote sensing derived CaCO3 concentration. 5. It is stated by the authors that "the wind-speed data used for the calculation of the gas transfer velocity was (were?) obtained from the MET Office Gascogne Buoy hourly data and averaged for each crossing" (L. 191-192). This buoy is located 45.201 N 5.000 W, off Bordeaux. As I understand the text, a single α is derived from this location and applied to the entire transect "crossing eight regions of different oceanographic characteristics" (L. 90). Wouldn't it be more appropriate to use synoptic wind-speed data, for example using wind-speed fields along the transects to derive α rather than a single value? 6. The equation (L. 300) proposed by the authors "in order to estimate the maximum TA drawdown (ΔTA) expected from the coccolithophore abundances (C)" (L. 294-300) is an estimate of the particulate inorganic carbon (PIC) associated to the abundance of coccospheres in the surface waters and not an estimate of the impact of biogenic calcification on seawater TA, as stated by the authors. To be exact in the computation of PIC, the abundance of suspended coccoliths released continuously by E. huxleyi should be added to this value since the cocolith:coccosphere ratio can reach up to 175 in the Western English Channel (Garcia-Soto et al., 1995). Calcification is a continuous process (with a specific rate) that occurs along the life cycle of the coccolithophore blooms (3-4 weeks in Garcia-Soto et al., 1995), during which HCO3- is converted into calcite, which lowers the TA by 2 μmol as 1 μmol of CaCO3 is produced. The drawdown of TA (ΔTA) is the integration over the watermass and over the time of several processes such as calcification and dissolution of CaCO3 and is related to the recent history of the watermass, and not on the standing stock of PIC. The authors, here, make the confusion between rates and standing stocks and suggest a weak influence of coccolithophore calcification on the carbonate chemistry of the Bay of Biscay. The mixing of cold and deep nutrient-rich waters in surface due to the occurrence of tidal waves in the Northern Bay of Biscay (see Fig. 3 in Wollast and Chou, 2001) brings alkalinity to the surface and lateral advection of waters with a different history with respect to coccolithophores are other cases where the ΔTA cannot be stated as a robust descriptor of the influence of cocolithophore calcification in surface waters. 7. It is stated "that the seasonal distribution of TA was not primarily controlled by production and dissolution of calcium carbonate (confirmed by the low coccolithophore

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abundances observed), but by uptake and supply of nitrate, (. . .) and freshwater inputs or removal such as mixing, precipitation, evaporation or river inputs " (L. 304-307). The uptake of nutrients by phytoplankton, in agreement with the nutrient-H+-compensation principle (Wolf-Gladrow et al., 2007), increases the TA and, therefore, cannot explain the drawdown of TA observed by the authors in April 2006 and 2007 in the Bay of Biscay, except if calcification concomitantly occurs with primary production, the general feature of coccolithophore blooms. 8. A C:N ratio of 8.4 is used through the text (from L. 380 on), for which no reference is given. The computation of the ΔDIC:ΔNitrate ratio in Table 3 roughly gives values ranging from 6.4 (close to the Redfield C:N ratio) to 9.4 (provided that the ΔNitrate is given in μmol kg-1 instead of μmol L-1, as referred in Table 3, and in the sake of homogeneity with the scale of figure 2c in μmol kg-1) with an average of 8.0 for the entire zone of study. Such a value above the Redfield stoichiometry is a strong indication for C-overconsumption in this area. This is an important conclusion that should be pointed out later in the conclusion. 9. "The historical time-series of the monthly NAO index showed a dominant negative phase for the period between March 2005 and November 2006 (. . .), corresponding with the enhanced winter MLD observed in the Bay of Biscay during this period, and a positive phase during the winter of 2006/2007, corresponding with the shallower winter MLD during this period". I suggest the NAO index should also be presented on the figure 5. For a better reading of the correspondence between MLD and T, the width of the figure 5b should be the same as fig 5a.

In conclusion, I do not recommend this manuscript for publication in Biogeosciences in this state of development. I'm surprised that this manuscript minimises the contribution of coccolithophores in the carbonate system of the Western English Channel and the Bay of Biscay, as the Acknowledgements point it out as a contribution to the CARBOOCEAN and EPOCA European projects.

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