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Discussions

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Interactive comment on "What drives the latitudinal gradient in open ocean surface dissolved inorganic carbon concentration?" by Yingxu Wu et al.

Anonymous Referee #1

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General comments

Wu and colleagues present a detailed analysis based on the GLODAPv2 data-base of DIC distribution in oceanic surface waters, and unravel the processes responsible for latitudinal gradients.

Major comment

A major short-coming of the paper is the use of Equation 5 (Page 5) to normalize DIC. This procedure has been criticised in the past and shown to create artificial variance in DIC and TA distributions (Friis et al. 2003).

Specific comments

Page 1 L 28 : The formulation of the sentence gives the impression that the concept of DIC was defined by Zeebe and Wolf-Gladrow (2001) which is not the case.

Page 1 L 28 : You might want to define the * of CO_2*

P 3 L5-13: I suggest to add calcification as a process controlling DIC, as global $CaCO_3$ production from calcification is estimated at about 1.6 PgC/yr (Balch et al. 2007), nearly equivalent to the net oceanic sink of atmospheric CO_2.

P 4 L 17: The formulation of the sentence gives the impression that the Mediterranean is "heavily influenced by river inputs". The Mediterranean Sea itself is an evaporative basin, with a salinity around 38 higher than the one of the North Atlantic. Only the Black Sea is "heavily influenced by river inputs".

P7L16: I think "organic matter burial" is more adequate than "organic matter sinking to the seafloor"

Page 8 Equation 10: Shouldn't you normalize to a constant salinity Alk_surf and Alk_supply prior to the calculation ? Rain or evaporation will change Alk_surf this will lead to a deviation from Alk_supply generating a value for Alk_CaCO_3 even in absence of actual calcification.

Page 8 Equation 10: similarly Alk_surf should be corrected for the assimilation of NO_3 and PO_4 by primary production prior to the computation of Alk_CaCO_3. Assimilation of inorganic nutrients by primary production leads to an increase of Alk_surf compared to Alk_supply (Brewer & Goldman 1976) that might obscure the actual signal from calcification.

Page 9: I do not understand how you accommodate equations 12 and 16 in your model. Equation 12 also accounts for NCP effect on DIC, so that if PO_4 is exhausted this equation allows to compute for "unused DIC". If Fe is the limiting (micro)-nutrient for primary production then this should lead to "unused PO_4" and allow to compute

"unused DIC" also from equation 12.

In addition, the authors preferred instead to use "unused nitrate" because "phosphorus has greater plasticity than nitrogen in plankton stoichiometry". Then this same reason should apply to compute the effect of NCP on DIC and prohibit the use of phosphate in equation 12, and nitrate should be preferred in equation 12.

For the sake of conceptual consistency in the approach it would be advisable that the effect of DIC assimilation by primary producers and NCP is computed in a consistent way independently of macro/micro nutrient limiting primary production.

References

Balch et al. (2007) Prediction of pelagic calcification rates using satellite measurements, Deep Sea Research Part II 54:478-495.

Brewer, P. G., and J. C. Goldman (1976), Alkalinity changes generated by phytoplankton growth, Limnol. Oceanogr., 21, 108-117.

Friis, K., A. KoÂĺrtzinger, and D. W. R. Wallace, The salinity normalization of marine inorganic carbon chemistry data, Geophys. Res. Lett., 30(2), 1085, doi:10.1029/2002GL015898, 2003.