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Discussions

Interactive comment on “Dissolution of calcium carbonate: observations and model results in the North Atlantic” *by* K. Friis et al.

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Review of: Dissolution of calcium carbonate: observations and model results in the North Atlantic by K. Friis et al.

This paper addresses a topical issue in ocean biogeochemistry, whether significant dissolution of CaCO3 occurs above the lysocline as most prominently proposed by Milliman et al (1999).

The study focuses on the subpolar North Atlantic and is carried out mainly through evaluating the distribution of salinity normalised alkalinity. The authors find that mixing, in particular between North Atlantic Deep and Antarctic Bottom Water, can explain most of the observed variability of salinity normalised alkalinity and so shallow-depth calcium carbonate dissolution (SDCCD) does not seem to be an important process in the region. Additionally the authors present output from model simulations to support their findings. The findings are in opposition to the findings of in particular Feely et al. (2004) who found significant SDCCD in this region, and so are important to publish. However, there are however two major issues that need further attention. I guess that these issues has most to do with the fact that the major part of the paper deals with salinity normalised alkalinity, AT_S, when it is really the excess alkalinity (TA* or delta_CaCO3) that is the issue. This becomes a bit like comparing oranges and lemons (but not apples and oranges).

Primarily: Friis et al. concentrates on salinity normalised alkalinity, AT_S. Feely et al. (2004), on the other hand, evaluates the variable delta_CaCO3 = 0.5(Tameas-Ta0)+0.63(0.0941xAOU). Friis does not explicitly explain why delta_CaCO3 implies significant SDCCD when AT_S does not. In particular in the upper 1000 m or so. The model output that is presented concentrates solely on delta_CaCO3, and shows in effect that there is minimal transport of this parameter to the upper 1000 meters or so. Feely et al (2004) found significant delta_CaCO3 in this depth range (or at least significant delta_CaCO3/age, their figure 4, and there might be issues with age estimates being to low). Taken together, the model output and Feely et al. (2004) support CaCO3 dissolution above 1000 m.

A figure of observed delta_CaCO3 should be presented. If the distribution of this is similar to the model output this would lend strong support to the conclusions. If it is different, that needs to be explained. In either case, the reason for why Feely et al. (2004) estimated significant SDCCD whereas the AT_S data indicate none must be discussed more thoroughly.

Secondly: I do not completely understand why AT_S is superior to delta_CaCO3 for this purpose. delta_CaCO3 also takes into account non-zero intercept of alkalinity vs. salinity (from the preformed term). It also takes into account the effect of organic matter remineralisation, which causes some issues for AT_S (page 1724). Without CaCO3 dissolution it would be zero everywhere (provided preformed alkalinity was accurately determined). Won’t it be affected by mixing in the same way as AT_S? A more detailed motivation for the extended use of AT_S is appropriate.

Specific points: 1716 line 10. It says that the model supports the observation. It is hard to agree with this statement as long as the observed distribution of delta_CaCO3 is not shown. line 25. Please specify: Ě. into shells of CaCO3. line 25. Conventional wisdom of the upwelling term is that Ě.. What is meant by “the upwelling term”? delete.

page 1717 line 8. I really can’t see how the alkalinity profiles are the most important piece of evidence presented in Milliman et al. (1999). The paper concentrates to a very large extent on sediment fluxes and in fact, as far as I get it alkalinity profiles are not presented at all, the caption of figure 4 says excess Ca2+.

line 15. Again. Neither Millman et al (1999) nor Milliman and Droxler (1996) presents profiles of salinity normalised alkalinity. They do not estimate the extent of SDCCD from such profiles.

page 1718 line 21 -23 I can’t see how the model output supports the observational findings as long as estimates of delta_CaCO3 are not presented.

page 1720. line 5 Is there a missing parenthesis in this equation? Should go like AT_S=((ATmeas-ATS=0)/Smeas)Sref + ATS=0

Page 1721. line 15 It is stated that there is a source of alkalinity in the surface ocean to compensate for loss due to sedimentation. Please explain why this does not contribute to a delta_CaCO3 signal in the upper ocean.

Page 1723 line 26 Reads “the diapycnal mixing of waters from below the saturation horizon onto waters above the saturation horizon”. Can not isopycal transport bring waters from below, above the saturation horizon? Isn’t this an argument of Friis et al. (accepted for GBC, cited in the paper)?

Page 1724 lines 20-30 Can the alkalinity change inferred from nitrate change be drawn as a line in figure 6?

Also, figure 6 is discussed prior to figure 5, please change the order of these plots.

And, can the depth be indicated in Figure 5 as well.

Can the calcite saturation state be indicated in figure 6. Maybe insert an arrow to indicate which waters are undersaturated.

Page 1725. line 25. It is stated that figure 7 shows observed and modelled excess alkalinity. I can only see the modelled. This also relates to my general comment. The observed TA* must be shown in the paper. All the model results show is that mixing from below can not create significant TA* in the upper water column. Why then does Feely et al. (2004) estimate significant CaCO3 dissolution rates in the upper 1000 m. Because of age estimates?

Also, please specify that the figure shows conditions of the eastern basin.

References: Feely, R.A. et al., 2004. Impact of anthropogenic CO2 on the CaCO3 system in the oceans. Science, 305, 362-366. Milliman J.D. et al., 1999, Biologically mediated dissolution of calcium carbonate above the chemical lysocline. Deep-Sea Res. I, 46, 1653-1669 Milliman, J.D. and Droxler, 1996, Neritic and prelagic carbonate sedimentation in the marine environment: ignorance is not bliss. Geol. Rundsch., 85, 496-504

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