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

K. Friis et al.

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We thank Dr. Olson for his thorough review that has made us look again and in more detail at the former literature that formed the basis of this paper. Ultimately this has strengthened our confidence in our study. We hope that we have properly addressed Dr. Olsons' comments in the following:

In the first section of his review, which is providing an overall assessment of our paper, Dr. Olson mentions that our study is dealing with two closely related, but different tracer approaches to investigate the calcium carbonate cycle. Dr. Olson argues that this comparison of approaches results in a paper that seems a bit confused. Whilst we agree that it would be advantageous to use just one approach, discussion and contrasting the two is a crucial issue of the paper. The reason being that this allows us to compare our results with those from (e.g.) Milliman et al. (1999) and GA2002 at the

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same time. We believe that for the subpolar North Atlantic the ATeS approach is able to address differences between our study and studies of the aforementioned investigators well. For investigations of the whole North Atlantic, i.e. our model study, we need to come back to the TA* approach [TA* is staying consistent with our manuscript, apart from that TA* is referring to exactly the same as TA*, the abbreviation used by Dr. Olson in his review.], since the regional AT-S approach is not easily capable of dealing with such a large region. For the sake of comparing model results with results derived from observations, we think the TA* approach is probably the better approach in general, see Friis et al. (2006).

Our reply to the primary comment: The TA* formulas/approach used by GA2002 do not directly imply significant SDCCD. It is much more the interpretation of the TA* along: (I) a circulation model that does not allow for neither dia- and isopycnal mixing, and (II) the mingling of almost independent distribution patterns of tracers age and TA* that lead to the conclusions of SDCCD, see also, page 1718 line 13-15. With regard to Fig. 4 in Feely et al (2004) we know from personal communication with Dr. Richard A. Feely that the shown dissolution rates are erroneous. This, in fact, also becomes clear when looking at Fig. 7 in Chung et al. (2003), where no significant amount of TA* is found in the upper ˜2000 m of the water column in the NA. The opposite indeed needed to be the case if Fig. 4 in Feely et al. (2004) was correct: dissolution rates are a direct derivative from TA*.

Our reply to the secondary comment: Ultimately the ATeS approach is not meant to be superior to the TA* approach with respect to the investigations of CaCO3 dissolution, following the lines of argumentation proposed by GA2002. ATeS is independent of the TA* tracer and, we feel, it is a more lean and direct approach for such an investigation. It only needs one independent parameter (salinity) to derive an empirical surface relationship of alkalinity. At the same time the empirical ATeS relationship can be interpreted to some extent. For example it allows us to interpret the relative importance of E-P effects on surface alkalinity in a particular region. A clear advantage of our ATeS

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approach is that it is able to tackle former flaws in the interpretation of 'traditional salinity normalized alkalinity' profiles and the excess alkalinity patterns at intermediate and deep ocean depth at the same time, as shown in Fig. 1 and Fig. 6. In contrast, the TA* approach seems incapable of explaining the differences in the alkalinity increase patterns from observations, Fig. 4 in Milliman et al. (1999) and Fig. 7 in Chung et al. (2003). These figures are directly comparable with our Figures 1 and 7, respectively. Indeed, GA2002 are largely in agreement with the in situ dissolution findings of Milliman et al. (1999), since excess alkalinity based and salinity normalized alkalinity based data are not clearly related to each other.

Our reply to specific comments of Dr. Olson (following the order of the review): Addressing concern that the model study does not broadly support the observational results: We have now better specified the relative importance of our model study compared to the overall investigation in our revised paper, especially in the abstract.

Our reply to Dr. Olsons' comment that alkalinity, and especially the salinity normalization of alkalinity, is not an issue in the Milliman and Droxler (1996) and Milliman et al. (1999). Dr. Olson also notes that Milliman et al. (1999) have not used salinity normalized alkalinity in their publication, but present a variety of circumstantial evidence of chemical dissolution of calcium carbonate above the chemical lysocline. In a way this point is correct, so we have changed the wording from "the most importantĚ" to "a very importantĚ". The latter is since, a very important point presented by Milliman et al. (1999) is that their findings would not be valid without a reconfirmation of these circumstantial findings by alkalinity, (see herefore also our points on page 1717 line 8-14). This is in fact what Milliman et al. (1999) try to address when showing their Fig.1 that is a "model of the CaCO3 budget for the global open ocean". Fig. 1 is based on salinity normalized alkalinity, adapted from Milliman and Droxler (1996) and is rather important for the "Implications" section. Also the excess calcium profiles shown in Fig. 4 of Milliman et al. (1999) are based on salinity normalization of alkalinity measurements, see the legend for Fig. 4 that reads "Excess calcium (defined as dissolved

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calcium measured by potentiometric titration)Ě". Dr. Olson points out that the alkalinity referred to in the publications of Milliman and Droxler (1996) and Milliman et al (1999) is not salinity normalized. This being the case then our critical review of these publication findings, which is along our figure 1, would be incorrect. Dr. Olson has again made a very valid point here and in both publications it would appear that the alkalinity referred to is not 'traditionally salinity normalized'. However, from our knowledge of global alkalinity distributions and the logic behind these authors shown alkalinity model it appears obvious that the authors simply omitted mention of their use of the normalized alkalinity data. In order to clarify this issue we have communicated with both Dr. John D. Milliman (at VIMS) and Dr. André Droxler (at Rice University). Unfortunately, neither were able to offer definitive statements on the alkalinity used/the issue in question. However, after further investigation into Wollast (1994) - the publication these authors are ultimately referring to - it has become clear to us that the alkalinity data used must have been traditionally normalized total alkalinity: Wollast (1994) is using mean alkalinity profiles of the North Atlantic, North Pacific and Southern Indian Ocean as provided by Takahashi et al. (1980). The Takahashi et al. (1980) profiles are ultimately based on the GEOSECS expeditions in the 1970's and are now available from Schlitzer (2000). Looking at the average profiles in Wollast (1994) and the ones available from Schlitzer (2000) doesn't leave any other option than the conclusion that the alkalinities in Wollast (1994), Milliman and Droxler (1996), and Milliman et al (1999) must have been traditionally normalized alkalinities, though it is not implicitly stated in these publications. The latter is most likely a very common case in the literature, since the traditional normalization has been a standard transformation for total alkalinity for almost 140 years. Only recently this transformation has been recognized for being flawed (Friis et al., 2003).

Our reply to Dr. Olsons' comment that the results from the model study that are shown along the meridional A16 section need to be shown with the direct counter part from observations for comparison: The TA* section is shown in Chung et al. (2003). We now explicitly refer to this plot in Chung et al. (2003). We believe the differences between

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observed and model patterns were properly discussed from page 1725 line 26 to page 1726 line 4. Indeed, the problem here is also that published equations for the preformed alkalinity by Chung et al. (2003) do seem to incorporate an unknown typo. By personal communication we have requested corrected equations, but these were not offered to us so far. Finally, we do not think that the main emphasis of our argument lies with the direct comparison of modeled and observed data. The latter is the topic of another publication (Friis et al., 2006). The main model results we are presenting are addressing length scale patterns of excess alkalinity. The support function of the model rather than the stand alone argument has now better pointed out in the manuscript.

Explanation to Dr. Olson as to why the source of alkalinity in the surface ocean that compensates for loss of alkalinity due to sedimentation does not contribute to a TA* signal in the upper ocean: By definition TA* is indirectly set to zero at the sea surface, i.e. preformed alkalinity equals actual alkalinity, see also Fig. 7. Our model routine is a simple way to account for riverine input of alkalinity, which is needed to maintain steady state. As stated in the beginning of this section all further details can be found in Friis et al. (2006), which has now been published and will make it easier for the reader to follow up on.

Reply to the suggested changes in Fig. 5 and Fig. 6: Fig. 5: It would be nice to add depth as a third dimension to this figure on the one hand, but on the other hand it is already shown in Fig. 4,making this redundant. Additionally, it would add unnecessary complexity to the figure. Fig. 6: Referring to our previous comment for Fig. 5 we think the figure is complex enough already. The saturation state is already shown in Figure 3. Therefore we believe addition of the saturation horizon to figure 6 to be unnecessary.

Reply to the suggested addition of a line in Fig. 6 that would show alkalinity changes by remineralization titration: Ultimately, we feel that we are making a stronger case not going for these options. This is because the figure is already very complex with three dimensions and a mixing line between endmembers. We also thought it is very interesting to see that regional normalized alkalinity actually decreases with depth to

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about 2000 m and that this titration effect is not masked by formerly suggested SDCCD.

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