

Interactive comment on “Sensitivity of pelagic CaCO₃ dissolution to ocean acidification in an ocean biogeochemical model” by A. Regenberg et al.

A. Regenberg et al.

ar@gpi.uni-kiel.de

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We thank David Archer for his comments and suggestions on our manuscript, which we highly appreciate. Please find our responses following the individual points, which are repeated below.

1. This paper addresses the range in predicted water column dissolution of CaCO₃ in the mineral form of calcite, an interesting and useful calculation. In particular the dependence of the dissolution rate law formulation is explored on the depth dependence of the dissolution response to ocean acidification. The analysis does not deal with sedimentary CaCO₃ dissolution, however, or water column dissolution of arago-

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nite, which can have a significant impact on ocean alkalinity / total CO₂ distributions to which the model results are compared. Presumably in areas where there is no CaCO₃ in the sediments, the ocean biogeochemical model will get all of that CaCO₃ dissolution flux, but in areas above the saturation horizon, organic carbon respiration can drive a significant fraction of the CaCO₃ rain to the sea floor to dissolve. The systematics of sedimentary dissolution differ from those in the water column because of regulation of the dissolution rate by the diffusive pore water regime. I worry about tuning the ocean models to fit the observations while leaving out these pathways.

Reply: We fully agree that many more factors than pelagic calcite dissolution are affecting the distribution of alkalinity in the ocean. Some factors are already listed on p. 11354, lines 9-11 in our manuscript. However, since mentioning the effect of sediment redissolution, grazing and organic carbon respiration are still missing, these will be added in a revised version. Please note that the effect of grazing (dissolution in the guts of the grazers) is already included in the model (see also below), even though it was not mentioned before. Maybe here is a little misunderstanding: in our study we do NOT tune calcite fluxes to fit alkalinity observations. We simply compare the resulting alkalinity fields to observations, assuming that a more realistic representation of calcite fluxes would reduce the overall model-data misfit. Of course, this is based on the assumption that errors add up rather than compensate, which is debatable. Anyways, in the absence of any clear indication which of the parametrizations would be the best one, we extended our study to test whether the different formulations would yield different results in an ocean acidification scenario. As shown in the manuscript, this is clearly the case for the depth attenuation of calcite fluxes in the mid-depth ocean, with potential impact on particle ballasting.

2. The derivation of the rate constants at different values of n, based on sediment trap data, also could be affected by the dissolution of aragonite. Short-deployment sediment trap fluxes from Acker seem to show attenuation with depth even where calcite is supersaturated. This has been modeled as resulting from dissolution in animal guts,

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as well as due to aragonite, but my recollection is that it's still hard to explain these results. Feely's water column alkalinity source flux calculations, based on water mass ages, also show a shallow-water source, and Milliman's water column alkalinity budgets. Given that the primary mechanism for explaining these observations is still unclear, it seems dodgy to simply fit the trap data to a calcite dissolution rate. At any rate the data and analysis should be presented. There's been a lot of literature on deriving the appropriate rate constant and reaction order for CaCO_3 dissolution in sinking particles and in sediments. The analysis here seems like a reinvention of the wheel.

Reply: We are still convinced that it is a useful approach to fit the attenuation with depth of modeled calcite fluxes to sediment trap data. Since the model calculates calcite fluxes prognostically, both directly correspond to each other. We agree that it cannot be excluded that the observed CaCO_3 from sediment traps may consist to some extent of aragonite, which would bias the derived rate constants (k) towards higher values. However, since the traps used were deployed in depths well below 1000m (and the aragonite saturation horizon) we expect that the ratio of aragonite versus calcite is rather low and negligible. As mentioned above, we do not fit calcite dissolution to bulk dissolved properties. Therefore, we also do not focus on the problem of shallow CaCO_3 dissolution. However, this point was investigated in more detail in a preceding study by Gangstø et al., Biogeosciences 2008. In the present study we did not include aragonite since no appropriate data exists to fit aragonite dissolution kinetics in a similar way as it was done for calcite. However, shallow calcite dissolution is included in the model by the calculation of net calcification. This is realized by the assumption that only 85% of the calcite passes the guts of grazers, while the remaining 15% are treated as directly routed back to TALK. Practically, this leaves a certain amount of TALK unused, i.e. virtually remineralized. We agree (and state in our text on page 11346, lines 4-10) that many studies about CaCO_3 dissolution kinetics exist. However, no clear picture of the pelagic calcite dissolution kinetics exists, which is already discussed in detail in a preceding study (Gehlen et al., Biogeosciences 2007). In fact, we make use of these many studies by exploring the full range of rate orders, which have been detected to

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range between 1 and 4.5. To our knowledge such a systematic and impartial study has not been carried out before. The procedure of the optimization of the dissolution rate constants was already explained in detail by Gehlen et al. 2007. Our approach mostly differs in the point, that instead of the (under-)saturation value of the lower traps we now calculate an average value of undersaturation that considers the vertical gradient of Omega or Delta between the two deployment depths and which is weighted by the time the particle remains at a certain degree of undersaturation, based on sinking speeds as used by the model. Consequently, the derivation of the rate constants is highly model dependent. We'll be happy to provide the sediment trap data and an explanation of the step-by-step details about the optimization procedure in a revised manuscript, either as supplementary or in the text.

3. The fact that the reaction rate order is not easy to constrain in the ocean between values of 1 and 4.5 (with adjustment of the rate constant k) means that the data are noisy relative to this range. I would take this to mean that the results presented here, showing the sinking flux sensitivity to the reaction order, would be similarly sensitive to uncertainty in the rate constant. But this uncertainty wasn't really considered.

Reply: It is true that the sensitivity to uncertainties in k has not been assessed. However, it is known that the reaction rate order n and dissolution rate constant k are dependent on each other, therefore, the optimization of k as a function of the rate order in our model framework is a straightforward approach. Assessing the uncertainty in k would include a number of other uncertain assumptions, for example particle sinking speeds. Since the focus of our study was to test the sensitivity of the different formulations of undersaturation Omega and Delta to ocean acidification, we feel that the investigation of uncertainties in k are an interesting point for a follow-up study but beyond the scope of this study. Furthermore, as the optimization of k is highly model dependent it was not our intention to define one specific equation which would be generally applicable.

4. The statement "Ocean acidification proceeds from below" seems a bit off to me. The dissolution response may eventually be more intense in the high-pressure deep,

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but the pH shift is strongest in surface waters. Dissolution of CaCO₃ also happens in the shallow water column, maybe due to aragonite, and decreased production also counts as a neutralization process, both of which may have already started. So stating in that same paragraph that carbonate compensation may start soon also seems off to me.

Reply: We agree that the formulation is not applicable as a general description of ocean acidification. It was referring to the study of Gehlen et al. 2008, who found that in an area of deep water formation bottom water undersaturation and thus CaCO₃ sediment dissolution may be reached rather quickly. We suggest to remove the sentence, as the reviewer is correct in saying that the same mechanism will also apply to shallower depths.

Yours sincerely, Anke Regenberg

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