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Discussions

Interactive comment on "Ocean acidification: setting the record straight" *by* A. J. Andersson and F. T. Mackenzie

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We appreciate the thoughtful comments made by Hauck et al., which we feel will assist in moving the discussion regarding the importance of the buffer effect forward. First of all we might ask ourselves what does a significant buffer effect mean? In the original proposals of the Magnesian Salvation Theory, a significant buffer effect referred to a restoration in seawater pH that would prevent or alleviate any negative effects on marine organism calcification rates arising from anthropogenic acidification. In our discussion of the topic, this is the definition that we adopted. In contrast, the workshop led by Garrels and Mackenzie (1981) considered how much anthropogenic CO2 could be taken up by the ocean considering whether a homogenous or heterogenous equilibrium existed between the atmosphere, seawater and the solid CaCO3 phase. In this latter definition, any dissolution and production of total alkalinity could be considered a buffer effect. In this context, whether this process is significant or not requires evaluation relative to the capacity of seawater to take up CO2 and provide the observed changes in dissolved inorganic carbon chemistry under a homogenous scenario (i.e., no reaction with CaCO3). Clearly, the thermodynamic definition of a significant buffer effect can be substantially different from a definition that considers the buffer effect in the context of the effects on calcification rates and the amount of the buffer effect required to prevent negative effects on the calcification rates of marine organisms. Hence, our differences with Hauck et al. may simply be related to how we define a significant buffer effect.

Certainly, the proposed step zero of Hauck et al. is included in our assessment, and in our example there appears to be more than sufficient CaCO3 that hypothetically could produce a significant buffer effect. However, as demonstrated in Andersson et al. (2005), the size of the reservoir of CaCO3 on timescales of decades to centuries is of minor importance, because the kinetics of the dissolution reaction is too slow relative to the increase in DIC owing to uptake of anthropogenic CO2 and physical mixing in coastal environments to produce sufficient alkalinity. In the best case scenario, a metastable equilibrium could be established between the seawater and the most soluble carbonate phase present in the sediments (see Morse et al., 2006), but despite this metastable equilibrium the buffer effect (defined as preventing any negative effects on marine calcifiers) is insignificant. However, relative to the homogenous reaction this mechanism obviously produces a buffer effect, which may be significant. The Devil's Hole example clearly illustrated this scenario, where substantial dissolution and restrictive circulation allow for accumulation of total alkalinity. Nevertheless, in terms of a significant buffer effect that would alleviate negative effects on organisms, it is absent. We agree that the Andersson et al. (2003) paper only focuses on a global mean condition that may be different in other locations, but subsequent publications (Andersson et al., 2005, 2006, 2007; Morse et al., 2006; and the present one) highlight that these conclusions apply to most shallow environments. The bottom line is a relatively simple mass balance problem where the rate of dissolution has to be sufficiently fast relative to the increase in DIC from anthropogenic CO2 penetration of the water column and physical mixing to produce a significant accumulation of total alkalinity. This approach can be applied to any given environment regardless of size and properties.

The conclusions of Loaiciga (2006) are indeed based on erroneous thermodynamic calculations as pointed out by Caldeira et al (2007). We did not mean to imply that the work by Hauck et al. is erroneous in any way despite their being mentioned in the same citation as Loaiciga. We will make this clear in our revision of the paper.

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