Biogeosciences Discuss., 8, C2362–C2365, 2011
www.biogeosciences-discuss.net/8/C2362/2011/

Biogeosciences Discussions

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

Anonymous Referee #2

Received and published: 6 August 2011

General Comments

In this review paper the authors provide an overview of 4 controversial issues involving the science of ocean acidification that require some thoughtful re-evaluation and further synthesis including: (1) surface seawater CO2 chemistry in shallow water coastal areas, (2) experimental manipulation of marine systems using CO2 gas or by acid addition, (3) net versus gross calcification and dissolution, and (4) CaCO3 mineral dissolution and seawater buffering. They describe the controversies associated with each of the issues and give their perspectives on ways to move the science forward. While this paper has the potential of becoming a useful summary of the scientific problems of concern and possible solutions, I believe the paper could be greatly strengthened with careful revisions of the introduction, abstract and discussion of the history and

background relating to each of the issues they discuss, and by providing greater clarity about the details of each issue as recommended in the specific comments below.
Specific Comments

Page 6163, line 1. This is a very incomplete representation of the history of the pioneering work on ocean acidification. The first discussion of ocean acidification and its biological impacts actually goes back to the early 1950s, and many significant papers of the 1970s, 1980s, and 1990s were not cited. The authors should review the summary of Doney et al (2009) and perhaps seek more information from Dr. J.-P. Gattuso or Dr. Joan Kleypas for the details of the history of ocean acidification. For example, some of the biological issues were discussed as early as the early 1980s in several noteworthy papers by Byrne et al (1984) and Betzer et al (1984) and their students. The basic chemistry issues were described in the 1960s and 1970s. Consequently, a more thorough review of the literature is clearly warranted here.

Page 6263, line 12 and parts of the abstract. The authors state. . ."With the increasing number of investigations and interest concerning this environmental problem, the number of opinions, often emotional, and misinterpretations of this issue have also increased by both scientists and the public." . . . .It is not clear whether or not the authors are referring to the published scientific literature or the published grey literature and/or media publications. If they are referring to the scientific literature then they should give specific references and clear examples of the misinterpretations. If they are referring to the grey literature or media publications, then, in my opinion, this paper is not suitable for publication in Biogeosciences and should be published in some other journal. In either case, the authors must clearly cite what articles they referring to.

Page 6265, line 1. In addition to the biological feedbacks mentioned here, shouldn't the authors also consider the chemical feedback of reduced air-sea CO2 influx due to the higher seawater pCO2 values from the upwelling and biological impacts? Wouldn't this supersaturation potentially produce a lower acidification rate in coastal waters? The authors should provide more details about this possibility and how these processes

might affect acidification rates in coastal regions.

Page 6172, line 15. The authors state that “Later, Andersson and colleagues (Andersson et al., 2003, 2005, 2006, 2007; Morse et al., 2006) showed in a number of papers based on a numerical modeling approach that the global coastal ocean, as well as most shallow water environments in general, will not be significantly buffered by dissolution of carbonate minerals on time-scales of decades to hundreds of years.” This is an important statement for the paper, but perhaps it does not apply everywhere. It would be much better if they immediately followed this statement with a brief explanation of why this is true and where exceptions might be found.

Page 6172, line 22. I agree with the comments in the discussions of this paper by Hauck et al (2011) discussion and suggest that the authors revise their text accordingly. In particular, Hauck et al state that. . ..”We agree with the authors in their recommendations that the points "evidence of a buffer effect“ (when the seawater is already undersaturated with respect to CaCO3), "kinetics“, and "physical mixing“ should be considered in studies addressing a possible buffer effect. We would like to add another step (Step Zero), which has to happen even before these considerations: proper quantification of how much CaCO3 is available in the region considered and identification of the mineral composition (aragonite, calcite, magnesium rich calcites). The authors of this paper need to address this point explicitly.

Page 6172, line 23. The reference to Lo′aiciga, 2006 should be removed. It is based on inappropriate assumptions and erroneous thermodynamic calculations (see comment by Caldeira et al., 2007). It is better not to cite the Lo′aiciga, 2006 paper at all.

Pages 6173-74. This section is very hard to follow because of all the assumptions being made that are not well described by the authors. The authors need to give some examples of the rates of dissolution of the Mg-calcite reservoir and how that might, in turn, impact the CO2 concentrations in coastal waters. Perhaps in addition to Figure 3 the authors should provide another figure or table showing some model results of the

time scales required for neutralization of anthropogenic CO2 to occur in coastal waters.

Figure 1. Figure 1 provides very different units for seawater pCO2 as compared with Figures 2 and 4, with no explanations. This may be very confusing to many readers. I suggest that the authors be consist with their units throughout this paper.

Interactive comment on Biogeosciences Discuss., 8, 6161, 2011.