

Interactive comment on “CO₂ air-sea exchange due to calcium carbonate and organic matter storage: pre-industrial and Last Glacial Maximum estimates” by A. Lerman and F. T. Mackenzie

Anonymous Referee #4

Received and published: 1 October 2004

The first part of this paper is an extensive discussion of the effect of CaCO₃ precipitation on CO₂ equilibrium. Carbonate system calculations such as this are done routinely by lots of people, and I could imagine assigning this problem as a class project for students. The comparison on page 16 between present-day (model predicted) riverine organic carbon fluxes to the ocean and the CO₂ rise in the atmosphere associated with deglaciation is oversimplified, in that those fluxes continue to this day and are obviously balanced by carbon sinks (photosynthesis, for example), and do not represent a one-way flux of C to the atmosphere. The next major section is a discussion of various terms in the global carbon balance across the deglaciation, with sea level rise, changes in weathering, and so on. The tabulated fluxes are interesting, but the overall amount of pCO₂ change you can get from the CaCO₃ cycle in the ocean is constrained by the change in the depth of the lysocline, which is known, and we therefore know that changes in the pH of the ocean were probably small. It could be that the boron iso-

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topes are correct and the pH of the deep ocean was considerably higher than today. But that's the question, not, as is answered here, whether it is plausible that changes in weathering could generate the observed pCO₂ changes, lysocline constraints aside. In short, I think the point(s) of the paper should be distilled down into a more concise presentation.

Interactive comment on Biogeosciences Discussions, 1, 429, 2004.

BGD

1, S242–S243, 2004

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