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***Interactive comment on “Linking the lithogenic, atmospheric, and biogenic cycles of silicate, carbonate, and organic carbon in the ocean” by S. V. Smith and J.-P. Gattuso***

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We thank Christoph Heinze for his very helpful review of our paper. We will, of course, deal with his specific comments in our final revision; the following response is directed at his “major concern” and his observation that he has potentially misunderstood us. We believe that there is, indeed, a partial misunderstanding.

C. Heinze considers it misleading that we pose the question in terms of insufficient CO<sub>2</sub> release to satisfy the constraints of eqs. 1 and 2 in our paper and that we point out that the atmosphere would be stripped of CO<sub>2</sub> on a timescale of 10,000 years if all cycling were met by these two equations. We note that Berner et al. (1983) came up with an

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estimate within the same order of magnitude. C. Heinze points out:

*... the concentrations in the ocean (and linked to this CO<sub>2</sub> in the atmosphere) as well as output of matter through sedimentation are kinetically controlled. Carbon can cycle many times in the ocean between surface and deep waters before leaving the ocean either through the atmosphere or the lithosphere at the ocean floor.*

We take no issue with this contention. In fact, this is precisely why we build our argument (p. 6585, lines 9-18) on the CaCO<sub>3</sub> burial rates (*not* the water column recycling rates) and flux of alkalinity to the ocean as estimated by Milliman and Droxler (1996).

We also take no issue with C. Heinze's argument that if alkalinity gets too low and pCO<sub>2</sub> gets too high in the ocean, weathering (or water column dissolution of particles) increases and brings these variables back into line. The one point we would make here for clarification, however, is that high-pCO<sub>2</sub> water below the mixed layer does not exchange CO<sub>2</sub> with the atmosphere until this water advects upward through upwelling and, by definition, is no longer below the mixed layer.

C. Heinze closes by stating:

*Perhaps the authors could rather use their consideration plus a more thorough study of input/burial estimates for CaCO<sub>3</sub> for a deconvolution of oceanic CaCO<sub>3</sub> export (assuming a steady state ocean and using their  $\Psi$  values from the sea surface.*

This leads us to three responses.

First, we claim no ownership of the  $\Psi$  values we derive; they are directly derivative from any program that allows various alternative variable pairs to partition the aqueous CO<sub>2</sub>

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system according to various scenarios. We have, over the years, used other equations for these derivations.

Second, we have performed the suggested deconvolution for the Hawaii time series station because there is little sediment burial of  $\text{CaCO}_3$  or organic matter over much of the North Pacific Ocean, because it is below the lysocline. The isolation of this station allows us to ignore alkalinity inputs from land and consider a 1-dimensional recycling model. We presented two versions of the deconvolution, as summarized in Table 1 and Figures 2 and 3.

Third, we agree with C. Heinze that a more global approach to the proper use of  $\Psi$  in such a deconvolution should include both inputs of alkalinity from land and three dimensional analysis of  $\text{pCO}_2$  distribution. We acknowledge that our calculation for one central ocean gyre station, with surface water in near  $\text{pCO}_2$  equilibrium with the atmosphere, and with no nearby burial of significant amount of either organic or inorganic carbon is a simplification. We would be delighted to see a more exhaustive analysis as suggested by C. Heinze, but it was beyond the resources we had available for this analysis.

Finally in closing, we observe the following. There is nothing novel about including organic cycling along with carbonate and silicate reactions in the consideration of solid-phase precipitation and weathering reactions. In the case of organic matter, the terms production and respiration. As we point out in the paper, this has been recognized since Ebelmen (1845). What is novel is including organic reactions with the Urey (1952) equations as inextricable equations in balancing the carbon cycle. Inasmuch as organic carbon production and consumption in the ocean are quantitatively far larger than  $\text{CaCO}_3$  precipitation and dissolution, and that a relatively small fraction of both organic matter and  $\text{CaCO}_3$  precipitated in the ocean ultimately become buried there, we do not find this linkage to be surprising.

## References

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