REPLY

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We welcome the comments of Dr. Berner, and appreciate the opportunity to respond. As we find little ground for disagreement, this response will be brief. In his comments, Dr. Berner emphasizes the importance of the link between rates of clastic sedimentation and organic matter burial, and the effect on computed values for atmospheric oxygen. He suggests that our fluxes for organic carbon burial, even when corrected for the relatively low clastic deposition rate of the Mesozoic (Ronov, 1993), are too high, in turn giving rise to unrealistically elevated oxygen levels.

In response, we first note that our oxygen curve for the last 200 Ma is only marginally outside the upper bound given in Berner and Canfield (1989), as described in our figure 14 (Arvidson and others, 2006, p. 169). Second, we note that both the state of organic matter cycling as well as the coupling between land and ocean with respect to organic matter were incompletely represented. In Arvidson and others (2006, p. 170), we stated the following:

> "However, in the current state of the model development, the description of organic matter cycling on land is oversimplified and thus incomplete. For example, the weathering of organic matter on land produces only an exchange of CO_2 for O_2, but yields no flux of organic matter from land to ocean, nor (as mentioned earlier) is the sedimentation of organic matter tied in any quantitative way to clastic sedimentation rate."

This last statement was intended as a caveat: an explicit reminder that the dependence of organic matter fluxes on sedimentation rate was neglected in this version of the model. As Berner points out, the burial of organic carbon in the published version of MAGic is a (nonlinear) function of the fixation (production) flux, which is in turn proportional to the availability of reactive phosphorous. The feedback to oxygen is indirect, and occurs through mediation of the C/P ratio of the depositing sediment via a parameter (DOA; Van Cappellen and Ingall, 1996) containing dependencies on both oxygen concentration and productivity fluxes. Incorporation of variations in clastic sedimentation and its control on organic matter deposition would likely have depressed oxygen levels below our "standard run" result. Unfortunately, as explained in the original text, our attempts to provide "full" coupling of organic carbon between terrestrial and marine reservoirs were not immediately successful, in that they also generated large instabilities in the seawater carbonic acid system. Wishing an opportunity to explore these sensitivities further, but not wishing this issue to delay presentation of the larger results of this complex model as they stood, we decided to publish the results using an abbreviated and incomplete representation of the interaction between these systems.

Last, we note that our major aim in constructing this model was *primarily* to ask the question: is our current understanding of long term, global biogeochemical cycling congruent with the sum of accumulating proxies for ocean and atmospheric composition and the mass-age data for the rock record itself? The answer is a qualified "yes", although certainly areas for improvement exist, such as those Dr. Berner has identified here, in terms of oxygen cycling and coupling between terrestrial and marine reservoirs, and links to atmospheric and seawater CO_2. These issues will be addressed in the next revision of the MAGic model.

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