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Discussions

Interactive comment on "Changes in the Si / P weathering ratio and their effect on the selection of coccolithophores and diatoms" *by* Virginia García-Bernal et al.

Anonymous Referee #1

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Review summary: This study by Garcia-Bernal and colleagues presents some model- and proxy-based arguments for past changes in Si and P nutrient supply into the ocean, followed by a comparison to metrics aimed at measuring the dominance of coccolithophores and diatom phytoplankton. From that the authors conclude that Si:P nutrient supply ratio to the ocean was an important factor contributing to phytoplankton ecosystem evolution, but the alternative hypothesis of increasing turbulence was somehow at play too. The study concludes with a long discussion of tectonic, climatic and biological processes that may have caused changes in Si and P supply to the ocean, their knock-on effects on biological carbon sequestration, and the efficacy of geo-engineered weathering and ocean fertilization for drawdown of anthropogenic carbon. Overall, I find that the evidence base is not well enough explained/documented, the comparison between nutrient forcing and ecological response not convincing, and the discussion ignores some critical weaknesses in data and theory. Therefore, I am pessimistic about the prospect of this discussion paper.

Specific comments:

Hypotheses: The study offers two initial hypotheses for processes that may explain the relative rise of diatoms over coccolithophores (see Fig.1): increase in weathering Si:P supply to the ocean favoring silicifying organisms, or alternatively global deepening of the mixed layer favoring organisms with high specific and/or population growth rates. After presenting evidence that may or may not support increases in Si:P of nutrient supply the authors conclude (p.8, lines 32-33): "the distinct fortunes of coccolithophores and diatoms during the Cenozoic era cannot be attributed exclusively to changing weathering fluxes and nutrient ratios." Why not? So, what should I be taking away from this study?

Observational evidence for environmental forcing: Four pieces of observational evidence are used without appropriate discussion: (1) the Follmi (1995) dataset for P burial is used without discussion of the mechanisms of P diagenesis and burial and without mention that shelf/slope sediments account for ∼75% of P burial (see e.g. Baturin 2007) without being well represented in the Follmi dataset; (2) the lithium isotope record is used as a direct (and linear) proxy of silicate weathering although most colleagues would probably argue that it records secondary mineral formation and the congruency of weathering instead of the primary weathering reaction progress (see e.g. the original paper by Misra and Frolich, 2012); (3) SCOR is (p.4, lines 4-5) "a proxy of plankton functional group dominance" and no discussion is offered why this concept of "dominance" can be equated to relative biogeochemical importance (what is the SCOR for the most productive phytoplankton group, unicellular cyanobacteria?); (4) evidence for long-term increase in the pole-to-equator temperature gradient have important implications for deep water formation, the oceans overall density structure and meridional overturning but it cannot be simply related to seasonal mixed layer depth and mixed layer light conditions as is done here.

Modeling: The model setup is insufficiently described. In particular the parameterization of the "weathering flux-uplift relationship (p5 line 19 to p6 line 3) is not transparent. What uplift record is used to force silica weathering? Likewise, it would be important to see what forcing gives rise to the simulated P weathering flux. I suspect simulated CO2 could be a good criterion to discard unrealistic model scenarios, why is it not shown? How does the model Si weathering vary when using the default CO2 dependent weathering scaling? As it is currently the model is a black box, the forcing is unknown, the output is incomplete and discussion is lacking.

Model-data and data-data comparison: Based on visual comparison the agreement between simulated and proxy-derived P and Si weathering fluxes is judged "remarkably coincident" and "satisfactory" (p 6 line 11,13). For P flux I find that the remarkable agreement cannot be coincidence, and for Si flux the poor match is unsatisfactory (model looks to follow strontium isotopes rather than lithium isotopes, the latter being used as the observational proxy). No objective analysis or relevant discussion is offered. Similarly, the authors find patterns in diatom and coccolithophores SCOR to be "consistent with changes in the Si/P weathering ratio (Fig. 4)" (p6 line 28-29). How so? I would have thought the various records are uncorrelated by any significance standard.

Discussion: The authors should seek to clarify the motivation for their discussion so as to avoid the sense that it aims to make ad hoc attribution of proposed changes in nutrient weathering fluxes to various tectonic, climate and environmental changes over the course of the Cenozoic. As one example, using denudation related to Himalayan orogeny as the core explanation for increases in silicate weathering after 20 Myrs even though that timing lags 15 Myrs behind seawater strontium isotope changes related to the same tectonic event is not helpful without detailed discussion of the discussion. Other aspects of the final section — such as the effect of the rise of diatoms

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on the biological pump and atmospheric CO_2 or the suggestion of geoengineering silica fertilization of the ocean to sequester anthropogenic carbon — are not sufficiently developed.

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