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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.

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In their manuscript, Dr. García Bernal and co-workers present an assessment of the interesting hypothesis that variation in the Si:P ratio of weathering fluxes to the ocean (together with changes in upper-ocean turbulence) might explain changes in the relative importance of marine coccolithophorids and diatoms observed in the sedimentary record over the last ∼40 million years. To do this, they reconstruct Si and P weathering rates from observations as well using an Earth system model, and compare the changes in the reconstructed Si:P ratio to changes in a metric for plankton functional group dominance. The authors state that this analysis suggests that the Si:P ratio (together with putative changes in upper-ocean turbulence) can explain the ecological

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Discussion paper

success of diatoms relative to coccolithophorids over the timeframe analysed. However, I cannot find any convincing evidence in the manuscript to back up this claim. This is mainly because the main results of the authors' work (i.e. a reconstruction of the Si:P of weathering flux and a reconstruction of phytoplankton dominance) are compared in a weak and non-quantitative fashion, simply by asserting similarity between the timeseries presented in Figure 4. To my eye, these timeseries do not show a strong similarity, and it would take a much more careful and rigorous analysis to convince me. In light of this, I cannot recommend this manuscript for publication in Biogeosciences in its current form. I provide some detailed comments below.

1. Model description:

I find the description of the model in Section 2.2. lacking in detail. On the one hand, the authors state that they apply a published model (COPSE; Bergman et al., 2006). On the other hand, they entirely alter the weathering flux-uplift relationship of that model, for both Si and P. Such an alteration may of course be justifiable, but requires much better documentation than the three lines devoted to the authors' approach at the beginning of page 6. Also, with respect to the oceanic P cycle in COPSE, it would be good if the authors could provide justification for the high C:P ratio of burial used by the model, rather than stating that an alternative parameterization (which is not used in this study, as far as I can tell) is available (P4, L30-32).

On a slightly different point, can the authors comment on what changes in the model lead to the massive decrease in P weathering flux and increase in Si flux at around 18Ma? Is this the effect of some external forcing to the model, and is it entirely independent of the P burial rate record from Föllmi (1995)?

2. Discussion of main results:

I think it is telling that the manuscript's Discussion section does not refer to the results of this study, but rather to general ideas about changes that might have occurred to weathering fluxes in the last 40 Ma. What this manuscript is missing is a convincing, detailed analysis of its own results, beyond the assertion that the timeseries in Fig. 4 are consistent with each other. On the face of it I do not see any evidence for a close linkage between the records presented. Given this rather tenuous similarity, I would need some careful analysis before I could be convinced that they are at all related, but the authors only provide a qualitative descriptive comparison.

In my opinion, the authors need to address a few questions: - Are there likely threshold values in the Si:P of the ocean inventories (or the weathering flux) that might lead to non-linear coupling between this variable and relative phytoplankton dominance? - Could a simple analytical framework (e.g. a box model) be used to represent such thresholds/non-linearities and actually tie the records together and make them comparable in a slightly more quantitative way?

Additionally, the authors should spend some more time making sure that the records they present are understandable to the reader. Currently, the text does not clearly state what the important plankton metric is: is it the SCOR ratio or the normalised SCOR value? What different information can we get from these two? Currently, the two would seem to contradict each other in some cases (such as the relative dominance of diatoms during the putative "Oligocene diatom crash").

Minor comments:

P2, L13: The effect of inorganic:organic C rain ratio on atmospheric pCO2 goes back to Archer and Maier-Reimer (1994), and this work should be cited here rather than Cermeño et al. (2008).

P3, L3: The forces driving upper-ocean turbulence are not explained clearly. I assume that the authors mean the atmospheric temperature gradient between the equator and the poles and its effect on wind-driven mixing, but this should be clearly laid out for the reader.

P6, L20-23: I see neither the peak in Si:P of weathering flux at the E/O transition, nor a contemporaneous peak in diatom SCOR values, in contrast with the authors' description.

Fig. 1: I have a problem with panel b. Within the context of ocean-internal nutrient cycling, I would argue that an increase in upper-ocean turbulence results in an increase in nutrient supply (through increased vertical mixing), and thus the two axes of this panel fall together. Regardless of this, to be entirely conceptually correct, the diagram should show the cartoons for coccolithophores and diatoms on the line, rather than on either side of it.

Fig. 4: Are the error bars in panel a associated with the range of 0.1-0.3 Tmol P/yr mentioned in the main text? If so, it would be good to mention this explicitly in the caption.

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