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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.

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We thank Dr. de Souza for his constructive criticisms and helpful comments.

R.C.: Reviewer comment

A.R.: Authors response

R.C: The authors state that this analysis suggests that the Si:P ratio (together with putative changes in upper-ocean turbulence) can explain the ecological 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

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Discussion paper

weathering flux and a reconstruction of phytoplankton dominance) are compared in a weak and non-quantitative fashion, simply by asserting similarity between the time series presented in Figure 4. To my eye, these time series do not show a strong similarity, and it would take a much more careful and rigorous analysis to convince me.

A.R: This issue has been expressed by both reviewers. Our analysis was not intended to demonstrate a strong coupling between nutrient weathering ratios and phytoplankton ecology. As stated in our response to Reviewer 1, the Earth is a complex system with many independent forcing mechanisms acting simultaneously, and often in a non-linear fashion. The geological record provides an averaged signature of a myriad of factors, which preclude us from identifying satisfactorily specific cause-and-effect relationships. Thus, though our analysis is purely descriptive (i.e., we were motivated by the realization that there was a conspicuous lack of knowledge concerning the linkage between weathering flux ratios and plankton ecology over geologic time), yet, some features emerge that may help to improve our understanding of the linkage between continental weathering ratios, ocean hydrodynamics and phytoplankton ecology. For instance, we find that throughout the Cenozoic the Si/P weathering flux ratio is commonly well above the classical Redfield. Additionally, some consistent patterns in the Si/P weathering ratio from both sediment data and model simulations such as the peaks observed in the mid- and late-Miocene deserve further scrutiny. We think that the analysis adds new and interesting information to compose a more detailed picture of the mechanisms that rose diatoms to ecological prominence. Our analysis may serve as a starting point to undertake some of the challenging questions raised by the reviewer below (i.e. 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?)

R.C: 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.

A.R: We provide a thorough description of the changes included and the extent to which these changes altered the outputs.

Si weathering from COPSE's default configuration decreases significantly towards the present primarily associated with a reduction in volcanic degassing. In the original model configuration, Si weathering is strongly dependent on volcanic degassing despite the latter being poorly constrained by data. Strontium isotopes, in contrast, suggest an increase in continental weathering and erosion coincident with the uplift of the Himalayas. This apparent controversy is further discussed in the new version of the manuscript as it seems to be a critical aspect of the model, at least for the last 40 million years. We elaborate on a more detailed justification of the new weathering-uplift parameterization.

R.C: 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).

A.R: We have used the parameterization set by default in the initial model configuration. We intended to alter external forcing mechanisms associated with uplift and weathering yet preserving other critical aspects of the original model.

R.C: 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)?

A.R: Yes, the model analysis is entirely independent of the Follmi record. Si flux is primarily driven by uplift and silicate rock weatherability. On the other hand, P weathering depends on weathering of silicates and carbonates, and the concurrent oxidation of sedimentary organic matter by assuming a relative proportion of P in each rock type.

R.C: 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 time series 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.

A.R: As stated before, we cannot establish a straight relationship between weathering flux ratios and the temporal distribution of plankton groups such as diatoms and coccolithophores. This limitation lies in the fact that other potential mechanisms are acting simultaneously and the analysis of the sedimentary/fossil record does not allow us to easily discern patterns from mechanisms.

R.C: 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?

A.R: These are very insightful ideas. Ecological studies suggest that diatoms dominate phytoplankton communities as long as Si exceeds 2 micromolar and other nutrients are not limiting. Further studies have shown that the Si/P nutrient supply ratio also exerts a strong control on diatom dominance relative to other phytoplankton(Egge and

Aksnes, 1992; Egge, 1998). Upper ocean turbulence and Si weathering fluxes have been put forward to explain the ecological success of diatoms in marine environments over geological time scales (Falkowski and Oliver, 2007; Cermeño et al., 2015). Here we include the Si/P weathering ratio as another potential control on diatom ecological success. We recognize that our analysis is insufficient to resolve the questions raised by the reviewer, but they help to expand the picture by showing that the Si/P weathering flux ratio is well above Redfield throughout most of the second half of the Cenozoic, when upper ocean turbulence and increased Si supplies contributed to the expansion of diatoms in marine environments.

R.C: 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").

A.R: The SCOR index has been used to quantify the dominance of plankton functional groups such as coccolithophores, diatoms, foraminifera and radiolarians (e.g. Liow et al. 2011, Hannisdal et al. 2011, Cermeño et al. 2015). Given that the microfossil record is largely limited to data of species presence/absence, estimates of dominance are commonly based on taxonomic richness (rather than abundance). However, taxonomic richness is not necessarily indicative of biogeochemical significance. The PaleoBiology database has global coverage, which allows us to compute the extent of geographic distribution (SCOR). The SCOR is thus a more realistic measure of biogeochemical significance than diversity. Normalised SCOR values were used to adjust both diatom and coccolithophore SCOR into the same axis. On the other hand, we used the SCOR ratio to illustrate changes in the relative importance of each phytoplankton group through time. These distinct metrics are now explained thoroughly.

R.C: 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).

A.R: Done

R.C: 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.

A.R: Exactly, the amplification of temperature gradient between the equator and the poles intensified atmospheric circulation and wind-driven mixing. This is now explained in detail.

R.C: 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.

A.R: The Si/P weathering ratio from the sedimentary record increases across the Eocene/Oligocene. This is not observed however from the analysis of model outputs. This is commented in the new version of the manuscript. The diatom Oligocene crash is particularly visible when we look at the diatom SCOR in Fig 2b. Diatoms exhibit a conspicuous decrease during the Oligocene that is also observable for diversity estimates (Rabosky and Sorhannus, 2009). This is surprising because this diatom crash is preceded by a rapid expansion across the E/O transition. We suggest that this increase in diatom diversity and SCOR could be associated in part with the high Si/P nutrient ratios observed in the sedimentary record. This observation and the discrepancy in the E/O weathering ratio between the sedimentary record and model outputs are now clarified in the new version of the manuscript.

R.C: 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.

A.R: We have repositioned the cartoons accordingly.

R.C: 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.

A.R: Yes, the meaning of the error bars is now mentioned in the text.

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