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Earth System Dynamics
Discussions

Interactive comment on "Ocean Phosphorus Inventory and Ocean Deoxygenation: Large Uncertainties in Future Projections on Millennial Timescales" *by* Tronje P. Kemena et al.

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First of all, we thank the reviewer very much for the constructive comments and advices, which helped to improve the manuscript a lot. Please find our reply below. Best regards,

Tronje Kemena and co-authors

Reviewer comments: This is an interesting paper describing potential future changes in marine phosphorus (P) cycling over the coming two millennia as obtained with an earth system model of intermediate complexity. The major conclusions are that there

are large uncertainties in these projections due to our lack of knowledge of the expected changes in P supply due to weathering and benthic release. An interesting observation is that, in this model, nitrogen fixation cannot keep up with P supply. The paper is generally well-written and deserves rapid publication. I do have a number of recommendations that I suggest the authors consider in a revision:

1. The presentation of the scenarios could be improved. In the model, 12 different runs were performed to explore how the marine P cycle responds to different model settings. The differences in the results of these scenarios play a crucial role but it takes the reader quite some time to figure out what is what. This could be improved if the sequence of the 12 scenarios in the text would be the same as in Table 1. A brief explanation of the various groups of scenarios could also be added in the caption of figure 2. We changed the sequence in Table 1 to fit the description in section 2 (with exception for the simulation Anthr). The descriptions and sequence of the burial experiments were also improved. We start in section 2.3 (burial experiments) with a general description how sinking organic matter interacts with the subgrid bathymetry and than explain how benthic fluxes of P are calculated: "The water column model is not coupled to a prognostic and vertically resolved sediment model. Instead, sinking organic matter interacts with the sediment via "transfer functions" (Wallmann, 2010) on a detailed subgrid bathymetry (Somes et al., 2013). Sinking organic matter is partially intercepted at the bottom of each grid box by a sediment layer and the intercepted amount depends linearly on the fractional coverage of the grid box by seafloor. The intercepted organic P is remineralized in accordance with Eq. (1) and Eq. (2), whereby organic C and N are completely remineralized under oxygen or nitrate utilization without any burial. Fractional coverage of every ocean grid box by seafloor was calculated on each model depth level according to the subgrid bathymetry (Somes et al., 2013). The subgrid bathymetry was inferred from ETOPO2v2 (National Geophysical Data Center, 2006). ETOPO2v2 has a horizontal resolution of 2-minutes fine enough to adequately represent continental shelves and slopes. The coarse standard model bathymetry in the UVic model has a horizontal resolution of 1.8° latitude x 3.6° longitude." (Line

126-137)

We added also a brief description of the different groups of scenarios to the caption of Table 1: “We divided all simulations in four groups indicated by different colors. These are: reference simulations (in black) with and without anthropogenic fluxes of P; simulations with different formulations for the burial (in red beginning with the acronym Bur); simulations with weathering fluxes of P for different climate sensitivities (in blue beginning with the acronym Weath); and simulations with different representations of the sediment (in purple).”

2. The reasons for the N limitation upon ocean deoxygenation could be mentioned explicitly in the abstract. We mention now the reasons for the N limitation already in the abstract: “In the model, nitrogen fixation was not able to adjust the oceanic nitrogen inventory to the increasing P levels or to compensate for the nitrogen loss due to increased denitrification. This is because low temperatures and iron limitation inhibited the uptake of the extra P and growth by nitrogen fixers in polar and lower latitude regions.”(Line 25)

3. It would be helpful to the reader to explicitly discuss the model assumptions leading to N limitation including uncertainties in changes in Fe cycling. I also would suggest to move Fig. S2 to the main paper. We mention and discuss in section 4.2 model assumptions for N uptake of diazotrophs and we mentioned how Fe limitation is simulated: “In our model, diazotrophs are limited by P and Fe and are not limited by N. Their growth rate, which depends on temperature being zero below 15°C, is slower relative to non-fixing phytoplankton. These characteristics allow them to succeed in warm, low-N and high–P environments that receive sufficient iron.”(Line 362) “We acknowledge that in the current study we did not account for potential future changes in iron concentrations (from atmospheric deposition, shelf inputs) and that the lack of a fully prognostic iron model may lead to a different sensitivity of the response of diazotrophs. Similarly we did not account for the ability of phytoplankton to adapt to changing N:P ratios, that may affect marine biological productivity and in turn deoxygenation. These would require

further studies.”(Line 374)

Further, we discussed how N-limitation and possible future changes in the Fe-cycle could affect the utilization of P by diazotrophs to section 5: “As a next step it would be reasonable to investigate how different parameterizations of the N cycle and a full dynamic iron cycle will affect the utilization of the added P. For example benthic denitrification is not simulated in the UVic model. Model simulations showed for this century, that the enhanced denitrification in the water column could be compensated by less benthic denitrification (Landolfi et al., 2017), which could reduce the N-limitation and therefore enhance the effect of P fluxes on the biological pump. Sources of bioavailable Fe are still not well quantified and how these sources change under climate change is under debate (Hutchins et al., 2016; Mahowald et al., 2005). A more realistic representation of a dynamic iron cycle in UVic would affect N2-fixation in many areas of the global ocean (Fig. 8).” (Line 490)

We moved Fig. S2 to the manuscript and adapted the numbering.

4. There is a lot of recent work on river fluxes of P to the ocean that would be appropriate to reference for context. We referenced to following publication in the text (Line 250, 234, 241): Harrison, J. A., Beusen, A. H., Fink, G., Tang, T., Strokal, M., Bouwman, A. F., Metson, G. S., and Vilmin, L.: Modeling phosphorus in rivers at the global scale: recent successes, remaining challenges, and near-term opportunities, Curr. Opin. Environ. Sustain., 36, 68–77, doi: 10.1016/j.cosust.2018.10.010, 2019. Beusen, A. H. W., Bouwman, A. F., Van Beek, L. P. H., Mogollón, J. M., and Middelburg, J. J.: Global riverine N and P transport to ocean increased during the 20th century despite increased retention along the aquatic continuum, Biogeosciences, 13, 2441–2451, doi: 10.5194/bg-13-2441-2016, 2016. Seitzinger, S. P., Harrison, J. A., Dumont, E., Beusen, A. H. W., and Bouwman, A. F.: Sources and delivery of carbon, nitrogen, and phosphorus to the coastal zone: An overview of Global Nutrient Export from Watersheds (NEWS) models and their application, Global Biogeochem. Cycles, 19, 1–11, doi: 10.1029/2005GB002606, 2005.

Detailed comments (partly overlapping): - Line 10: suggested change: ": : :.that enhanced weathering and increased benthic phosphorus (P) fluxes" Added "that".

- Line 14 and 15 and elsewhere: "until the year 2300" Corrected.

- Line 25: suggested change: "In the model, nitrogen fixation was not able to adjust: : :": Added.

- Line 25. Here, it would help if the authors clarify why nitrogen fixation does not adjust to the high P levels. Because these are model results, this can be specified. We added a sentence to clarify this: "This is because low temperatures and iron limitation inhibited the uptake of the extra P and growth by nitrogen fixers in polar and lower latitude regions."(Line 27)

- Line 27: "this contrasts with" Corrected.

- Line 27. Here, the authors could clarify whether the palaeo reconstructions refer to model studies or reconstructions based on data or both and exactly how those results are different. We made here a very general statement, so we decided to remove this sentence.

- Line 49. Suggested change "the Earth has experienced" or "the Earth experienced" Corrected.

- Line 49. It's not clear what is meant by "climate OAE-like states". I would suggest to rephrase. We rephrased this sentence to be more specific: "Evidence in the palaeo record indicates that the Earth has experienced several OAEs with large-scale anoxia, euxinia and mass extinctions (Kidder and Worsley, 2010)."(Line 51)

- Line 66. What about increased inputs of Fe from continental shelves upon ocean deoxygenation? Could they alleviate the Fe limitation in the model? What is the uncertainty in the dust inputs? We just recognized that this sentence could imply that we simulate a dynamic dust cycle. Instead spatial dust limitation is based in UVic on present day observations. We decided to remove this sentence to not give this wrong

impression.

- Line 79. "and improved representations of" Corrected.

- Line 96: "as the switching point" Corrected.

- Line 98: "so that" (instead of "such that") Corrected.

- Line 111 and Table 1: are benthic P fluxes equated to burial? I don't see the term benthic flux in Table 1. The terms need to be used consistently. We refereed here to benthic P fluxes across the ocean-sediment boundary in general. To avoid confusion we replaced "benthic P fluxes" with "benthic P burial". In other text passages "benthic P fluxes" was replaced by "benthic P burial" or "benthic P release" respectively.

- Line 112: in a print, the purple and blue are very difficult to differentiate I am not sure, if you refer here to the color in Table 1 or to the general usage of purple and blue. However, we increased the color intensity of the purple in Table 1. We avoided using the combination of the colors green and red in all plots to improve the distinguishability of the colors for persons with green/red color blindness.

- Table 1: please add much more detail on the abbreviations in the text (or can you think of an easier notation?). Now it is very hard for the reader to keep the various model scenarios apart. Note that the sequence doesn't match the text and that the anthropogenic flux not quantified. In brackets short expressions were added to let the reader easier identify how the abbreviations are related to changes in the model formulations: Bur_res (i.e. restricted release or P reservoir); Bur_Dun (i.e. burial parameterization from Dunne et al. 2007); Weath0.05, Weath0.10, Weath0.15, Weath0.38 (i.e. the number represents the preindustrial weathering flux); Anthr (anthropogenic) For the model simulation Anthr we referred to Fig. 2, because we used a temporal variable weathering flux: "The weathering flux in simulation Anthr is variable over time (Fig. 2a)." (Table 1)

- Line 136-138. Please explain this section on the organic C burial better: it seems

contradictory that all organic C is remineralized but that there is still organic C burial. We adapted the text to clarify that we calculate a 'virtual' C burial to determine the P burial. In this model no C burial is applied: "Virtual is meant in the sense that there is no explicit burial of organic C in the model, and instead all organic C is remineralized in the deepest ocean layer. In this study we do not focus on changes in the C inventory and therefor BURC is just calculated to determine BENC in Eq. (2)." (Line 148)

- Line 258. "is essentially equal" Corrected.

- Lines 225-226. It is very well known already for a long time that total P in rivers can be mobilized in the coastal zone and forms a key input of P to the coastal zone. These are not new findings of Benitez-Nelson, Compton et al. and Ruttenberg as suggested here, so I would suggest to rephrase this sentence. We rephrased the sentence and added a reference to older studies: "...(see Fig. 1, Benitez-Nelson, 2000; Compton et al., 2000; Ruttenberg, 2003). These studies give a range of total P fluxes to the oceans, which are higher than interfered from dissolved inorganic P fluxes shown already in previous studies (e.g. Martin and Meybeck, 1979; Rao and Berner, 1993)..." (Line 229)

- Same section: you could consider including a reference to the weathering & anthropogenic flux of P calculated from the Global News models (see the work of Seitzinger et al. 2010; Global Biogeochemical Cycles) and follow-up studies (e.g. Beusen et al. 2016; Biogeosciences) We cited in this paragraph Seitzinger et al. (2005) to reference to the Global News model.

- Lines 258. Change to "essentially" Corrected.

- Line 267. Change to "after the year" (note that "the" is missing before year in more places) Corrected.

- Line 327: Change to "a consequence" Corrected.

- Line 350. Change to "shown" Corrected.

- Line 358-359. This is an important piece of information that also should be given in

the abstract, see comment above. We added this information to the abstract. "This is because low temperatures and iron limitation inhibited the uptake of the extra P and growth by nitrogen fixers in polar and lower latitude regions."

- Don't start a sentence with "So that" Improved.

- Line 440-443. Needs rephrasing since benthic P release is known to be important in the coastal zone from both experimental field studies and modeling. Yes we agree with you. However, we focused here how a release of the actual inventory of sedimentary P could contribute to an increase in the oceanic P inventory. The deposition of fluvial particulate P to the sediment and its release was not simulated. We rephrased the text passage to state this more clear: "This could imply that benthic release of P is actually negligible in comparison to the weathering fluxes of P, but the UVic model does not resolve coastal processes such as the deposition of reactive particulate P from rivers on the continental shelves and its dissolution and release to the water column. For a more honest comparison of benthic and fluvial P fluxes, a more detailed representation of coastal processes would be necessary to simulate deposition and release of fluvial P from the sediments at the shelf. However, we can conclude that the actual local inventories of P are too small to sustain a positive benthic P feedback over several millennial."(Line 457)

- Line 459. The term "palaeo study" is vague. Please provide more information i.e. on the type of setting and time period. We provided more informations to the palaeo study in the text: "In the time period of the OAE1a and the OAE2, a substantial increase in N2-fixation was also inferred from measurements of sediment nitrogen isotope compositions typical for newly fixed nitrogen conditions and from high abundances of cyanobacteria indicated by a high 2-methylhopanoid index (Kuypers et al., 2004)."(Line 481)

- Line 460. "not able to compensate: : :." Unless you are underestimating the Fe input to the ocean. A few lines on the uncertainties there (e.g. shelf Fe input, dustfield) would be useful. We added a discussion in the result and discussion section: Section

4.2: “We acknowledge that in the current study we did not account for potential future changes in iron concentrations (from atmospheric deposition, shelf inputs) and that the lack of a fully prognostic iron model may lead to a different sensitivity of the response of diazotrophs.” (Line 374) Discussion: “Sources of bioavailable Fe are still not well quantified and how these sources change under climate change is under debate (Hutchins et al., 2016; Mahowald et al., 2005). A more realistic representation of a dynamic iron cycle in UVic would affect N2-fixation in many areas of the global ocean (Fig. 8).”(Line 495)

- ‘line 480. Could be changed to “on benthic P fluxes in this model is eventually: : :” These results really depend on how the N and Fe cycles are parameterized – should be discussed in a few more sentences in the main text. We added a short paragraph to section 5 and section 4.2 to discuss this (see also the last comment related to the Fe cycle): Section 4.2: “In our model, diazotrophs are limited by P and Fe and are not limited by N. Their growth rate, which depends on temperature being zero below 15°C, is slower relative to non-fixing phytoplankton. These characteristics allow them to succeed in warm, low-N and high–P environments that receive sufficient iron.”(Line 362) “Similarly we did not account for the ability of phytoplankton to adapt to changing N:P ratios, that may affect marine biological productivity and in turn deoxygenation. These would require further studies.”(Line 377)

Discussion: “As a next step it would be reasonable to investigate how different parameterizations of the N cycle and a full dynamic iron cycle will affect the utilization of the added P. For example benthic denitrification is not simulated in the UVic model. Model simulations showed for this century, that the enhanced denitrification in the water column could be compensated by less benthic denitrification (Landolfi et al., 2017), which could reduce the N-limitation and therefore enhance the effect of P fluxes on the biological pump.”(Line 490)

- Fig. S2: I would suggest to move this figure to the main paper. We moved Fig. S2 to the manuscript and adapted the numbering.

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