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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: Kemena et al. present 12 long-term global warming simulations of the UVic Earth System Model of Intermediate Complexity to assess how projected P and O2 inventories depend on implemented weathering and sedimentary fluxes. The focus is on biogeochemical feedbacks, as the physical response is almost identical across the simulations. They suggest weathering fluxes contribute most to projected



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increased P inventories. I consider this to be a welcome contribution to the field of long-term Earth system projections. I have several questions regarding the methodological approach and conclusions which should be considered to improve the readability and focus of the manuscript.

-Bur simulations: I find it hard to judge the “meaningfulness” of the Bur simulations given that they release P from an unlimited reservoir. The Bur_Res simulation seems to indicate an upper limit on the potential P release, which all other Bur simulations appear to surpass. So all other simulations release more P than can be assumed to be in the sediments, correct? Should the reservoir constraint not apply to all simulations? You might want to consider disqualifying those simulations a bit faster in the text and highlight the importance of this constraint a bit clearer, including the abstract and method/results section. In the Bur simulations, we investigate uncertainties in the release of P from the sediment in the future by analyzing different parameterizations for benthic P fluxes. These transferfunctions are used in various studies as a state of the art approach (e.g. Bohlen et al., 2012; Niemeyer et al., 2017; Wallmann, 2010), because transferfunctions are less cost intensive and easier to implement than full complex sediment models (Soetaert et al., 2000). In this study we like to push forward the development of these transferfunctions, but at the same time we like to point out how large uncertainties of such simple transferfunctions can be, therefore we believe that the results of all model simulations should be published.

Wallmann K (2010) Phosphorus imbalance in the global ocean? Global Biogeochem Cycles 24:1–12. doi: 10.1029/2009GB003643 Bohlen L, Dale AW, Wallmann K (2012) Simple transfer functions for calculating benthic fixed nitrogen losses and C:N:P regeneration ratios in global biogeochemical models. Global Biogeochem Cycles. doi: 10.1029/2011GB004198 Niemeyer D, Kemena TP, Meissner KJ, Oschlies A (2017) A model study of warming-induced phosphorus–oxygen feedbacks in open-ocean oxygen minimum zones on millennial timescales. Earth Syst Dyn 8:357–367. doi: 10.5194/esd-8-357-2017 Soetaert K, Middelburg JJ, Herman PMJ, Buis K (2000)

On the coupling of benthic and pelagic biogeochemical models. Earth-Science Rev 51:173–201. doi: 10.1016/S0012-8252(00)00004-0

-Weathering simulations: Why do these simulations not have a burial formulation? Of course, addition of P to an otherwise "closed" ocean just increase its P inventory. Would it not be important to assess the feedbacks associated with deposition and redissolution in this context, as increased P supply to the upper ocean stimulates NPP, export and deposition? I could not find a discussion on this matter in the manuscript.

In these simulations just anomalies (W_P=W_(P,0)·(f(NPP,SAT)-1) with f(t=0)=1) for weathering were applied. In a future study we could imagine to investigate above mentioned possible negative feedbacks. However, all simulations show an increase in NPP, export and the herewith associated increase in burial of P can be seen in all Bur simulations, especially in simulation Bur_high and Bur_Res with net global P loss at the end of the simulation. However in this study the low N availability is the predominant process that prevents the ocean from further deoxygenation (and not high burial rates in P). This can also be found in model simulations where such a negative feedback could by P are possible (Niemyer et al., 2017).

Niemeyer D, Kemena TP, Meissner KJ, Oschlies A (2017) A model study of warming-induced phosphorus–oxygen feedbacks in open-ocean oxygen minimum zones on millennial timescales. Earth Syst Dyn 8:357–367. doi: 10.5194/esd-8-357-2017

-It is not clear to me which simulation corresponds to the best estimate reported in the abstract and conclusion. Do you just add the result of the Weath0.15, Bur_Res and Anthr simulations? You may want to guide the reader a bit better here, and explain why these simulations are simply additive? That seems strange to me, as for instance, the Bur_Res simulation would suggest a W0=0.41 TmolPa-1, much higher than the 0.15 TmolPa-1 considered in Weath0.15.

We added these values together for following reasons: The anthropogenic input of P is prescribed and is extracted from Filippelli (2008). The weathering input depends

on environmental parameters and parameters of the weathering equations. The environmental parameters are just affected by the climate and therefore the changes in atmospheric CO2 concentrations. In our simulations, the climate develops in all simulations almost in the same way, so this would not affect the addition. However, you are right the export is much higher for simulations with larger oceanic P inventories. We hope that the additional removal of P will not affect the oceanic P inventory too much on this timescales. We shortly discussed your concerns in section 5: “In this simple addition of the P inventories we cannot account for feedbacks, which would appear in a fully coupled model. For such high P inventories we would expect larger suboxia and therefore more P release from sediments and at the same time a stronger export of organic P that lead to increased P burial.”(Line 471)

Filippelli GM (2008) The Global Phosphorus Cycle: Past, Present, and Future. Elements 4:89–95. doi: 10.2113/GSELEMENTS.4.2.89

-Language: I find the language at times ambiguous. Most importantly, the usage of benthic fluxes, burial and sediment release appear confusing, and it’s not clear whether gross or net exchange is meant. Please consider explicitly introducing these terms and using them consistently.

To avoid confusion we replaced “benthic P fluxes” in by “benthic P burial” or “benthic P release” if possible. Benthic P burial and benthic P release is defined in section 2: “P burial in the sediment (BURP) was determined in every grid box with sediment from the difference between the simulated detritus P rain rate to the sediment (RRP) and the benthic release of dissolved inorganic P from the sediment (BENP): ãĂŰBURãĂŮ_P=ãĂŰRRãĂŮ_P-ãĂŰBENãĂŮ_P (1) where RRP is the detritus flux from the ocean (in P units).” (Line 138) We used these definitions consistently throughout the manuscript. P burial/release leads to a net loss/gain of P in the global P inventory. We are not sure what do you meant with “gross exchange”. “Benthic fluxes of P” is used as a more general term and it describes fluxes of P across the benthic boundary layer.

-Balance between presentation of P and O2 response: Even though deoxygenation is mentioned in the title, there is very limited mentioning of deoxygenation in the abstract and discussion. The most important driver of ocean deoxygenation appear to still be circulation changes, and the assessed biogeochemical feedbacks should be presented in this context. There are several papers worth citing/discussing in the introduction and discussion on the matter of long-term projections of ocean deoxygenation, e.g.: Battaglia & Joss 2018, ESD, Yamamoto et al. 2015, GBC, Schmittner et al. 2008, GBC As such, the modeled circulation response may be compared to other long-term projections.

The focus of this manuscript is to assess uncertainties in the projections of the P inventory and how this could affect deoxygenation. We agree that a reader of the title could expect a more general investigation of deoxygenation processes, therefore we decided to change the title of the manuscript to: “Ocean Phosphorus Inventory: Large Uncertainties in Future Projections on Millennial Timescales and its Consequences for Ocean Deoxygenation” Furthermore deoxygenation is always analyzed in relation to changes in the P inventory as it was already mentioned in the introduction: “Here, we build on this study and test the sensitivity of the marine P and O2 inventories in a climate change scenario on millennial timescales to different model formulations of P weathering and benthic fluxes.”(Line 74) We removed the sentence from section 3 to avoid the impression that just uncertainties in P land-ocean and P sediment-ocean fluxes can be the drivers for the large range of P fluxes: “We found that the large range in P fluxes was not related to differences in the climate or atmospheric CO2 forcing, but rather to differences in parameterizations of P land-ocean (Sect. 3.1) and sediment-ocean (Sect. 3.2) interactions.”

Specific comments:

Line 27: “this is in contrast to paleo reconstructions”: not clear what is meant from the text We made here a very general statement, so we decided to remove this sentence.

Line 29: “more reliable projections of ocean deoxygenation”: context of ocean deoxygenation does not emerge from the abstract. How do the biogeochemical feedbacks assessed compare to uncertainties in circulation changes? Introduction: centers round CO2-driven ocean deoxygenation. Please include recent literature on ocean deoxygenation, and the fact that circulation changes are crucial for ocean deoxygenation associated with global warming, not CO2 per se.

We completely agree with you, circulation changes are crucial for ocean deoxygenation too, but there are a series of factors e.g. changes in oxygen solubility, stratification, wind and upwelling, respiration, circulation and mixing processes that effect deoxygenation (Levin, 2019; Oschlies et al., 2018). We added a sentence in the beginning of the introduction: “Many different processes affect the oxygen balance in the ocean (e.g. oxygen solubility, stratification, respiration, circulation, Levin, 2019; Oschlies et al., 2018).” We focus here on very long time scales and here we were not able to find an answer in literature to the question: “How do the biogeochemical feedbacks assessed compare to uncertainties in circulation changes?”. However, in this study we focus how uncertainties in benthic fluxes of P can affect the deoxygenation of the ocean and for this reason the temporal evolution of the ocean circulation is kept the same in all model simulations. In one paragraph of the introduction we focused on CO2, because the increase in atmospheric CO2 and consecutively climate warming is the most likeliest driver for OAEs. Actually changes in P supply (the same for circulation) are just a consequence of the CO2 induced global warming.

Levin LA (2018) Manifestation, Drivers, and Emergence of Open Ocean Deoxygenation. Ann Rev Mar Sci 10:229–260. doi: 10.1146/annurev-marine-121916-063359 Oschlies A, Brandt P, Stramma L, Schmidtko S (2018) Drivers and mechanisms of ocean deoxygenation. Nat Geosci 11:467–473. doi: 10.1038/s41561-018-0152-2

Line 57: “could”: will? We replaced “could” with “will” as suggested.

2.3/2.4: Please consider presenting weathering first, consistent with presentation of

results/Table 1 We changed the order of the experiments in Table 1 with first the burial experiments and than the weathering experiments to be more consistent with the order in the “Model and Methods” section.

Line 126: “every grid box”: every bottom grid box? We clarified the text here, because in the sub grid bathymetry benthic fluxes are not just limited to the bottom grid box: “every grid box with sediment”

Line 137: “all organic C is remineralized in the deepest ocean layer”: statement must be wrong? Eq. 3a-c: potentially include z1000m on the respective lines for clarity. Eq. 4: Is there only a O2-feedback on P fluxes? Should the C burial/redissolution not also be O2-dependent? Potentially worthy of discussion.

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 170: Please add numbers in parenthesis.

We clarified the text passage. The burial is by definition one magnitude larger see also Eq. 3b, c. Factor 0.14 and 0.014. We slightly rephrased the sentence: “In the standard formulation, C burial is by definition one magnitude larger in slope and shelf regions compared to the deep ocean (see Eq. 3b, c).” (Line 168)

Line 183-200: Hard to understand. Examples below: We improved the paragraph as suggested (see below).

Line 187/188: “for the continental shelf and slope”: how was this done for all other grid cells? We added a sentence to clraify this in the text: “In accordance to Flögel et al. (2011), release of P from the deeper ocean (>1000 m) cannot exceed the rain rate of organic P to the sediment. For the continental shelf and slope,. . .” (Line 191)

Line 194: “local inventory”: what do you mean with this? Do all cells have this inventory? Or is this an upper limit for inventories globally?

Meant here is that 100% of the total solid P can be released. We rephrased the sentence and moved the introduction of RESP to the next sentence. As the local inventory is given in $\mu$mol cm-2 it scales also with increasing sediment coverage (for every grid cell with sediment coverage). 113 $\mu$mol cm-2 is the upper local inventory, we name it in the text now maximum local inventory (RESP,max):” Together, these assumptions convert to a maximum local inventory of total solid P in the active surface layer of RESP,max = 113 $\mu$mol cm-2 (Eq. 6a).” (Line 198)

Line 195/196: reservoir can be replenished, but excess P is permanently buried? Is the $\Delta$RESp the replenished P or excess P? Eq. 6: are the > and > correct?

Yes excess P is permanently buried. We replaced $\Delta$RESP by $\Delta$RESP/$\Delta$t to indicate that this is the change of RESP over time. The valid range of RESP, with 113 $\mu$mol cm-2 its upper limit, is defined in Eq. 6b.

Line 197: “depending on environmental conditions”: what do you mean with this statement? Depending how? Earlier you mentioned local inventories of 113 umol/cm2?

RESP is the actual local P inventory and variable over time. During the spin-up simulation the local P inventories adapt to the environmental conditions like oxygen concentration or rain rate of P. I hope this is now clearer by introducing the maximum local inventory of P RESP,max and the actual local inventory of P RESP.

Line 277: “the way sediment P reservoirs are represented”: if represented at all or not. 3.2: Preferably start section with PI RR (lines 295-299) and PI burial rates (lines 279- 283). Then, would strongly suggest changing the tone of this section, in that Bur simulations without a reservoir constraint are not realistic. Potentially exclude those runs from Figs.5-8, Figures are very busy anyway and lines are hard to tell apart. Or explain why those are considered for assessment of ocean deoxygenation, still.

This question is answered in the comment (-Bur simulations) of the reviewer.

Line 308/309: Please add more citations of long-term O2 projections.

We additionally cited Matear and Hirst (2003) as well as Shaffer et al. (2009).

Line 374: “recovered”: strange language. O2 inventory is still increasing and simulation has not reached steady state yet.

We replaced “recovered” with “reached present day values again”

Line 377: How is AOU/O2sat calculated? Potentially discuss/mention Ito et al., 2004, GRL?

We added a description how AOU/O2sat is calculated and discussed Ito et al., 2004: “AOU is calculated from the difference between the O2 saturation concentration and the in situ oxygen concentration assuming that all ocean water leave the surface layer saturated in O2. The calculation of AOU is in general biased to higher values, because in polar regions surface water leaves the surface water in respect to oxygen in a undersaturated state due to reduced air-sea gas transfer inhibited by sea ice (Ito et al., 2004). In UVic this leads to an overestimation of AOU by 30% (Duteil et al., 2013). For a warming ocean sea ice cover reduces which converts into an underestimation of the AOU anomaly in Fig. 9c.” (Line 387-393)

Line 390: Are preformed nutrients carried as explicit tracers? Please introduce how signal is separated.

We described the calculation of preformed nutrients in the figure caption of Fig. 10: “Preformed nutrients are calculated as the difference between remineralized and total nutrient content. The calculations assume that all ocean water leave the surface layer saturated in O2.”

Line 397: “are likely”: how come you are not sure about this?

We removed “likely”.

Line 398: “global N inventory constant”: does not appear to be the case in Fig. 9a?

We discuss here changes in the parameters for the time period from the beginning of the simulation to simulation year 2200. To be more clear we slightly rephrased the sentence: “Until the year 2200, changes in circulation and climate are likely the main cause for the reduction in preformed N and P in the Ref simulation since global N and P inventories were almost constant in this time period (Fig 9a, solid red and black line).” (Line 412)

Line 399: Physical response: potentially summarize physical response earlier, perhaps right after 4., as this is the baseline response of the scenario which applies to all variables?

Thank your for your suggestion. Here in this manuscript we mainly focus on changes in the P cycle and therefore we would prefer to discuss changes in the meridional overturning later.

Line 400: is this the max of the global meridional overturning?

Yes it is the maximum. We replaced “meridional overturning” by “meridional overturning maximum”.

Line 402: “consistent with reduction of export”: in section 4.1 you appear to conclude that warmer temperatures enhance remineralization in the shallower ocean which reduces export?

We do not agree with your thoughts. An increased remineralization rate could also lead to faster recycling of nutrients from the shallow ocean to the surface ocean. This could increase the export production, because of the faster “recycling” of nutrients from the shallow ocean to the surface in comparison of the recycling of nutrients from the deep ocean to the surface for lower remineralization rates. The stratification due to the continuous warming inhibits exchange of surface waters with the deep ocean and increases the residence time of water in the ocean. Until year 2200 highest rates of

warming appear in the ocean and lead to a strong stratification of the ocean, but after this CO2 emission decline and the rise in surface temperatures of the ocean is much weaker.

Line 404-407: “speculate”? This reasoning, also in line 415, I do not understand. I would speculate that these changes are associated with older water masses. AOU and ideal age probably are highly correlated, indicating that more O2 is consumed in older waters. See literature.

We completely agree with your thoughts and with your conclusions, which was also reflected by the text: “We speculate that a weaker overturning increased the residence time of water and nutrients in the surface ocean. Nutrients staying longer in the euphotic zone are with a higher probability biologically consumed. This implies more efficient utilization of nutrients and, hence, the reduction in preformed nutrients and an increase in AOU.”(Line 420) I am sorry that I do not understand your concern could you please explain yourself a bit more.

Table 1: Bur_low and Bur_high: reference to equation (4) rather than (3)? Bur: potentially also add coefficients here for consistency with Bur_low, Bur_high

We improved the table as suggested.

Figure 2a: net flux? What are the step-like increases in the Bur simulations associated with? Those are also present in subsequent figures?

We mention this already in the manuscript: “In Bur, a rapid increase in the benthic P release appeared in areas where the water turned suboxic and thus drove a positive benthic feedback between P release, productivity and deoxygenation. A limited supply of P from the sediment (Bur_Res) dampens this feedback.”(Line 298)

Figure S2/S3: might benefit from an improved aspect ratio.

We improved the aspect ratio.

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