

Interactive comment on “Preformed and regenerated phosphate in ocean general circulation models: can right total concentrations be wrong?” by O. Duteil et al.

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We thank the two reviewers for their generally positive and constructive comments on our manuscript. In the following we address all points raised by the two reviewers and organise our response letter along the individual points of reviewer 1 (which include the points raised by reviewer 2). The reviewer comments are in italics and our replies in plain characters,

- *‘The study compares the nutrient partitioning between preformed and regenerated phosphate in the observations with a range of global biogeochemical models. The mis- match between the model distributions and observations is greater*
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for either the pre- formed or regenerated phosphate, rather than the total phosphate concentrations. This mismatch suggests that the general circulation models have different relative strengths in the physical and biological pathways of nutrients. These differences are important as they affect the biological drawdown of carbon from the atmosphere. The study is carefully conducted and the error misfit clearly set out with appropriate figures.’

We thank the reviewer for this positive evaluation.

- *‘The discussion paper interprets the mismatch between the model distributions and the climatology mainly as reflecting model error. While this spread in model and data agreement almost certainly is dominated by the model performance, there are also valid concerns about the reliability and representativeness of the observational data. There are inherent errors, measurement and sampling errors, in the composite of the climatological data. In particular, the lack of seasonality in the climatological data and the bias of any observations to the summer period at high latitudes, especially in the Southern Ocean, needs to be mentioned. While this aspect is touched on in the Conclusions, it is preferable to more fully address this aspect earlier in the manuscript. In addition, how large might the natural variability in the observed phosphate distributions and their partitioning likely to be?’*

The paragraph introducing the World Ocean Atlas is (P12428 L1-2): ‘All models were integrated for at least 2000 yr to reach a steady state quasi-equilibrium. Annual mean output from these models is compared against objectively analysed annual means of the World Ocean Atlas (WOA) 2005 (Garcia et al., 2006a,b).’

In the revised manuscript it will be complemented by : ‘When using WOA as the ground truth against which we compare the different models, one has to keep in mind that there are inherent errors in this annual-mean composite of observations, specifically due to measurement errors, seasonal and regional sampling biases and interpolation effects. In particular, the scarcity of observations during

winter time at high latitudes, especially in the North Atlantic and the Southern Ocean, might lead to a bias in the annual mean climatology of the upper ocean towards summer. In the deep ocean, however, phosphate and oxygen concentrations as well as temperature and salinity show little to no seasonal variations (e.g. Conkright et al., 2000). Here, seasonal bias in sampling is hence of minor importance. Our analysis considers concentration distributions over the entire water column, of which the upper ocean represents only a relative small fraction. For the objective of this study, potential upper ocean sampling biases are therefore assumed to have only a very small effect.

Conkright, M.E, Gregg W.W, S Levitus S.: Seasonal cycle of phosphate in the open ocean, *Deep-Sea Research I*, 47, 2000, pp. 159-175

- *'In some parts of the text, the arguments could be set out slightly clearer and some terminology or statements tightened up and made more precise. These detailed points are set out below.'*

Detailed points:

P12425 L24: Do not refer to 'so-called preformed phosphate', as a clumsy phrase. Instead define preformed at the outset.

The initial paragraph was: 'Remineralisation of organic matter by bacteria and zooplankton releases nutrients such as PO₄ and consumes oxygen. This phosphate is called regenerated phosphate. Biotically unutilized surface phosphate can also enter the ocean by subduction during water mass formation. According to observations, this so-called preformed phosphate makes up for about 64 % of the global ocean's phosphate inventory, whereas regenerated phosphate derived from remineralisation of organic matter makes up for the remaining 36 %'

The text has been changed to: 'Remineralisation of organic matter by bacteria and zooplankton releases nutrients such as PO₄ and consumes oxygen. This phosphate is called regenerated phosphate. The remaining phosphate is the biotically unutilized surface phosphate, which enters the ocean by subduction

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during water mass formation, and is called preformed phosphate. According to observational estimates, the preformed phosphate makes up for about 64% of the global ocean's phosphate inventory, whereas regenerated phosphate derived from remineralisation of organic matter makes up for the remaining 36%'

- *P12426 L3-L9: These two sentences could be made much clearer to read, they cut to the heart of the interpretation, but are over long.*

The two sentences were : 'A more active biology increases the transfer of phosphate from surface to intermediate depth by remineralisation and consequently depletes surface waters, decreasing the amount of phosphate subducted at high latitudes and thus decreasing preformed PO₄ and increasing regenerated PO₄ concentrations. An accelerated circulation results in higher supply rates of nutrients from the deep ocean to the euphotic zone, often increasing surface nutrient levels and thereby tending to increase the preformed PO₄ and decrease the regenerated PO₄ pool.'

They have been rephrased into : 'For example, a more active biology increases the transfer of phosphate from surface to intermediate depths by remineralisation, depleting surface nutrients. Consequently, the amount of phosphate subducted at high latitudes, i.e. the preformed phosphate, decreases as well. As another example, a more vigorous overturning results in higher supply rates of nutrients from the deep ocean to the euphotic zone, which will increase surface nutrient levels. Thereby, the preformed PO₄ pool increases whereas the regenerated PO₄ pool decreases.'

- *P12427, L4: Ito and Follows (2005) should also be cited for this point, as they first set out the link to atmospheric CO₂.*

The citation has been added.

- *P12427, L8: Cut 'Without the intention to perform a complete model intercomparison', as a model intercomparison is performed here.*

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This part of the sentence has been deleted.

- *P12428, L6-7: As you have raised the issue of the different oxidants, it would be useful to quantify how important they are. How minor are they? At present, you raise an issue, then dismiss it, but without providing any quantification.*

In the revised manuscript, we have now included a more detailed estimate of the importance of the respective demineralization pathways as stated below (after P12428, L7). Also note that we quantitatively deal with this issue later in the paper, for those models that separate nitrogen and phosphorus cycles (see P 12435, 12438, Fig. 4 (yellow and purple symbols).

'Organic matter remineralisation using oxygen occurs everywhere in the ocean, except in places where oxygen is essentially depleted (suboxic regions). In this case, NO₃ and NO₂ may be used as electron acceptors for the oxidation of organic matter. The global importance of oxygen vs. NO₃ as electron acceptors can roughly be quantified from comparing current estimates of denitrification and the global export production in the ocean interior. The estimated loss of N is about 100 Tg Nyr⁻¹ (Codispoti et al., 2001), which represents about 1e13 mol C-equivalents as roughly 1 mole NO₃ is consumed during the oxidation of 1 mole carbon (Koeve and Kähler., 2010). The export flux is estimated to be about 10 GtC yr⁻¹ (or about 1e15 molC) (e.g., Schlitzer, 2002). The amount of phosphate regenerated under suboxic conditions is equal to the ratio between these two quantities and accounts for about 1 percent of the aerobically remineralized phosphate. Oxidation by other compounds (sulfate, some metals) occurs only in enclosed seas (Black Sea, Baltic, some fjords) and in sediments in shelf and coastal waters. It may locally be important relative to aerobic demineralization, but in the open ocean's pelagic realm investigated here is not considered to play a significant role.'

Codispoti, L.A., J.A. Brandes, J.P. Christensen, A.H. Devol, S.W.A. Naqvi, H.W. Paerl and T. Yoshinari. The oceanic fixed nitrogen and nitrous oxide budgets:

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Moving targets as we enter the anthropocene? *Scientia Marina* 65 (Suppl. 2), 2001: 85–105

Koeve and Kähler. Heterotrophic denitrification vs. autotrophic anammox – quantifying collateral effects on the oceanic carbon cycle. *Biogeosciences* 7, 2010: 1813–1837.

Schlitzer R. Carbon export fluxes in the Southern Ocean: results from inverse modelling and comparison with satellite-based estimates. *Deep Sea Res. II*, 2002, 49, 1623–1644.

- *P12428, L20: Fixed stoichiometric ratio, P:-O₂. There are really two issues here, one is the value assigned, which is discussed in the following text. The other issue is whether the value varies at all. Certainly this choice of a fixed ratio is plausible, but useful if any further discussion is provided as to its validity.*

The original paragraph read: 'This ratio has been set to 1/172 in OPA-PISCES, MPIOM-HAMOCC, 1/170 in CCSM3-BEC, CSIRO, MOM P2A, UVIC2.8 and 1/150 in om1p7-BLINGv0. We use these model values when analyzing the respective models outputs. A common P:-O₂ value of 1/170 is used for the analysis of WOA data independently of the value considered in the different models. Indeed, generally accepted values derived from ocean data are close to 1/170 (Takahashi et al., 1985; Körtzinger et al., 2001, Anderson and Sarmiento., 1994, Li et al., 2000)'

In the revised version, it has been complemented by : 'It has been shown that this ratio is nearly uniform with location and depth. Using TTO/NAS and GEOSECS data in Atlantic and Indian Ocean, Takahashi et al. (1985) found no evidence for a significant difference between the estimate ratios of oxygen consumption to phosphate release for thermocline waters in the Atlantic and Indian Oceans. Similar -O₂/P values have also been observed in the deep Pacific (about 2.7 to 3.5 km deep), deep Indian Ocean (about 3.5 km deep), the Red Sea, and the

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Norwegian Sea (Broecker et al., 1985)

Broecker, W. S., Takahashi, T. and Takahashi, T.T. : Sources and flow patterns of deep ocean waters as deduced from potential temperature, salinity, and initial phosphate concentration. J.Geophys. Res., 90, 1985, 6295-6939

- *P12429 L4-5. The assumptions about the rapidity of air-sea gas exchange are rather vague. Instead please provide likely exchange or equilibration timescales for dissolved oxygen. What is rapid or not is subjective.*

The original sentence read : 'We note that, despite the rapidity of the air-sea gas exchange, equilibrium of surface water with atmospheric oxygen is not perfect and may be prevented by physical processes, such as surface heat loss or entrainment of oxygen depleted deep water'

It has been modified to :

'With a piston velocity for oxygen of several meters per day, the timescale needed for the oxygen concentrations in the ocean's surface mixed layer, typically several tens of meters thick, to equilibrate with the atmospheric can reach a few weeks (e.g. Broecker and Peng, 1982). We note that, despite the relative rapidity of the O₂ air-sea gas exchange (rapid when compared with about one order of magnitude longer equilibration timescales for the strongly buffered CO₂), equilibrium of surface waters with atmospheric oxygen is actually not perfect and may be prevented by physical processes, such as ice cover, surface heat loss or entrainment of oxygen depleted deep water'

Broecker, W.S. and Peng T.H: Tracers in the Sea. 1982, New York: Eldigio press.

- *P12429 L10-15. I do not really see why the models could not be used to provide some insight as the skill of the AOU approach. This surely is the benefit of using a model, rather than just the data. As this information is not provided here, please add a quantification of the errors taken from Ito et al. (2004).*

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We thank the reviewer for stressing this important point.

Model studies like the work of Ito provide a quantification of the uncertainty of the AOU approach in that given model. In their model, the deviation of AOU from 'true oxygen utilization' (TOU) can reach up to 20 percent in some parts of the ocean, as stated in the conclusions (P12436 - L25 to 27). Relevant to our data-model comparison experiment, however, is whether differences between AOU and Ito's true oxygen utilisation (TOU) are similar/identical in all models and in the real ocean, or not. The limited observations which we discuss in the conclusion section indicate so far no obvious bias between model experiment of Ito and the real ocean, which justifies our usage of AOU.

In consequence we have added the following paragraph in the conclusion (P12437 - L1), which replaces the current paragraph (P12437 - L4 to L10). 'For doing better, two things are needed and likely will be available in the near future. First and most importantly, more unbiased winter time observations from high latitudes are required. Provided that current sensor calibration issues can be solved, our understanding of winter time oxygen saturation at high latitudes can be expected to improve considerably within a few years from now. In models, the skill of the AOU approach can be tested explicitly, following the methodology of Ito et al. (2004). However, computing the TOU requires the use of an explicit tracer for preformed oxygen. This tracer is fixed at the surface at every time step to the total oxygen concentration and then transported passively into the ocean interior without being subject to biotically induced fluxes. Experiment with such tracers, however, were not available for the models presented here'.

- *P12435, L10. In the Southern Ocean, there is also an issue of how representative the data is.*

This is now stated in the revised text (see first point)

- *'In summary, the paper sets out the view that the skill of ocean biogeochemical*

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models can be critically assessed in terms of the performed and regenerated partitioning of nutrients. I recommend acceptance subject to the above minor comments being addressed.'

We thank the reviewer for her/his effort and hope that we have appropriately addressed all comments.

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