

## ***Interactive comment on “Bioavailable atmospheric phosphorous supply to the global ocean: a 3-D global modelling study” by Stelios Myriokefalitakis et al.***

**Anonymous Referee #1**

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The atmospheric deposition of P provides an important source of P to the terrestrial/marine ecosystems with notable effects. This manuscript studies the atmospheric cycle of P using a global 3-D chemistry-transport model. In particular, this study accounts for the P mobilization from mineral dust, which is found to be an important source of dissolved P. I feel that this paper can be published after addressing some comments below. Specific comments: 1) Line 23 (p1): BP and DP are confusing in the abstract. 2) Line 25 (p1): It is unclear how the <50%> uncertainty is quantified in the results. 3) Line 26-29 (p1): It is better to give the dissolution fluxes, rather than one percentage. 4) Line 30 (p1): "dissolution flux of P" and "P mobilization flux" are confusing in the abstract. 5) Line 9 (p5): Using a coarse-resolution model to get the horizontal

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distributions of P concentration and P deposition can lead to substantial biases in the model-observation comparison, which should be considered. 6) Line 10 (p5): It seems that the model is only run with meteorology for one year (2008). In this case, the question is that there is inconsistency if the observations are derived for other years. It means that the interannual variation of P emissions from mineral dust and sea-spray (related to wind) and the episodic transport of P in the atmosphere (related to wind and wet precipitation) cannot be represented in this study. I expect that the model can be run for more years to get an unbiased estimation of P emissions from mineral dust and sea-spray. 7) Equation 1: I am curious to know how the authors get the fraction of P in the emitted dust. The global soil mineralogy datasets are developed for the clay and silt fractions of soils. Therefore, according to my knowledge, the mineralogy of soil is different from the mineralogy of dust. Please explain how the mineralogy of soil is transferred to the mineralogy of emitted dust in the model. 8) Line 29 (p5): I am curious to know what aerosol scheme is used to treat the size evolution of P-containing particles in this model. Is it following a modal method or a sectional method? I expect that the size (0.34  $\mu\text{m}$  or 1.75  $\mu\text{m}$ ) is not fixed in the model. If the size was fixed, I would have to say that the authors should take a more advanced scheme before the paper can be published. 9) Line 30 (p5): Please explain how the solubility of P (10%) is derived? 10) Line 1-5 (p6): Please list the detailed P/BC mass ratios in the emissions (in the Supplementary Materials). I would expect different ratios for emissions from different types of fuel (e.g. coal, oil and biomass, etc). 11) Line 8-11 (p6): Please explain how the initial size distributions of P emissions are treated in the two estimations 1) based on P/BC ratios and 2) based on the new estimation by Wang et al. 12) Line 14 (p6): What is "TP coarse aerosol emissions"? Please replace this with a more rigorous term. 13) Line 20 (p6): I do not think that this assumption is justified. Carbonaceous aerosol (e.g. BC) is mainly contained in fine particles and thus the ageing via coagulation and condensation is very fast. However, P could be more concentrated in coarse particles (as noted by the authors) and the ageing should be slower. 14) Line 23 (p7): Please compare the global total sea-spray emissions output from TM4-ECPL to other

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estimations before using the data. It seems that the super-coarse mode of sea-spray has not been considered in this model, and it might lead to significant underestimation of P concentrations when comparing with cruise measurements over the oceans. 15) Line 30-35 (p7): The authors are right to use the surface concentrations of Na and PO<sub>4</sub>. I am curious to know how deep is defined as the surface water for Na and PO<sub>4</sub>. It is better to clarify this, although it is not easy to make sure that they are consistent. 16) Line 14 (p8): I am not against what is done here, but I would like to make it clearer that we should always be very careful when taking this kind of assumption. For example, the authors took the sulphur emissions from Andres and Kasgnoe (1998) while adopting the size distribution proposed by Dentener et al. (2006). Are they consistent? 17) Equation 6: I am curious to know what H<sup>+</sup> activity is used in this equation. The authors seem to be clear that the H<sup>+</sup> activity in aerosol water is different from that in cloud droplet. Which one is used here?, or both are used. Please clarify it. 18) Section 3.2: Please give maps of the geographic distributions of sites measuring P-containing aerosols concentrations and dry deposition fluxes in the Supplementary Materials to show the spatial coverage of the observational data used in this study. 19) Line 8-13 (p11): Since the model is run with meteorology for only one year (2008), I am curious to know how the authors can compare the modeled P concentrations in 2008 with that measured in other years. If the measurements were also all measured in the year 2008, it should be fine. 20) Line 8 (p11): It is unclear if the authors evaluated the dry deposition fluxes, or they have evaluated both the dry and wet deposition. Please make it consistent. 21) Line 20-23(p11): Here, I am not convinced of the conclusion. Accordingly to our knowledge, the emission inventory is important for the modeling of P, but it is not the only factor that matters. For example, the treatment of aerosol scheme and the initial size distribution of P in the emissions can also influence the concentrations and transport of P. Unfortunately, the authors do not provide necessary information on these in their methods, making it hard to judge whether the conclusion is right or not. As I know from Wang et al., they have accounted for three size bins of P-containing particles in their model, rather than the two size bins in your model. So, it is at least

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necessary to repeat the treatment of size distributions by Wang et al. and discuss on the impact. 22) Line 27 (p11): I would expect that the authors compare the modeled P concentrations with that from cruise measurements for the same days (see Figure 8 in Wang, R. et al. *Atmos. Chem. Phys.*, 15, 6247-6270, 2015). 23) Line 27 (p13): "SO<sub>x</sub>, NO<sub>x</sub> and NH<sub>x</sub> anthropogenic" -> "anthropogenic SO<sub>x</sub>, NO<sub>x</sub> and NH<sub>x</sub>". 24) Section 4: This part is interesting and novel. It will be better if the authors can add discussion on what can be improved to get a better understanding of this impact in the future studies or what is the most uncertainty.

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