

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

Anonymous Referee #2

Received and published: 5 July 2016

Review of Myriokefalitakis,

This is a potentially publishable paper looking at an interesting topic. With a few revisions in the presentation, the paper should be publishable.

Major points: 1. This is the first study which explicitly models the evolution of the state of the phosphorus in the atmosphere, which is an important innovation. I think you should highlight this in the abstract, introduction and conclusions. 2. More careful consideration of size. As shown by the contrast between Wang et al., 2014 and [J Brahney et al., 2015], there is a large sensitivity in the budgets of P to assumptions about aerosol size. Please discuss in the methods section the sizes you are considering within the model. Please also discuss the deposition data, and whether it includes

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sizes of particles you don't include, and so some part of that mass should be neglected (e.g. Brahney et al., 2015 discussion of sizes). 3. More description of the observations and how you are comparing to them. Please add a section of the methods talking about your observations. It's unclear in your scatter plot where the data comes from and how you are comparing the deposition data (e.g. are the sizes consistent?). Could you show on one of your plots where the data comes from in these scatter plots (a little x?), and maybe show your observations from the cruises in a different color than the observations from the station on the scatter plot? Please discuss a little bit the differences in these observations and their value for your comparison (e.g. temporal variability vs. cruises showing spatial+temporal variability). Also, please include a description of your metric within the methods section (not a reference to another paper in the results section). 4. Compare % soluble observations vs. model? You get really high solubilities far away from the sources— is there any evidence of this? Perhaps if you compare % solubility instead of soluble P amount, it might make your case more compelling that you are doing the solubility right? In a sense the P amounts are dominated by getting the P sources, and the right size comparisons, but the solubility, which is the real innovation in this study, might be better explored by the % solubility in the obs. Vs. model? Even if there is no evidence of these high solubilities, you are underestimating soluble P, so maybe underestimating % solubility close in, so maybe we should believe these high solubilities? 5. You make the case that your results suggest a more important soluble P sources from biogenic aerosols. Why do you get a larger source than previous studies? Is it because you assume more of the biogenic are bioavailable, or are your sources larger? Just add a sentence or two on this.

Details: “primary and secondary sources of P” Are there any secondary sources of P in your approach?

“Okin et al. (2011) evaluated the impact of Fe and P atmospheric deposition to the ocean in increasing N₂-fixation and found that Fe deposition is more important than P deposition in supporting N₂-fixation, while they pointed out the large uncertainty in the

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bioavailability of atmospherically deposited P.” There are also ocean biogeochemical model studies which show this same results either: [A Krishnamurthy et al., 2010] or [R Wang et al., 2015] which also suggest that atmospheric deposition of P doesn't matter because of large P reservoirs in ocean.

“Wang et al. (2014) taking into account the potential volatilized-P produced during combustion processes, calculated about 30 times higher global atmospheric P emissions from biomass burning and anthropogenic combustion processes (0.7 Tg-P yr⁻¹ and 1.8 Tg-P yr⁻¹ respectively).” This is not accurate. Tipping et al., 2003 put together a compilation of deposition in ecosystems, and indicated that the observations suggest this deposition dominated by locally generated primary biogenic material, in the aerosol mode >10µm which is not long range transported. Wang et al., 2014 used the mismatch in size between their <20µm modeled aerosols and the observations in the source regions and assumed that this mismatch was only from combustion sources. Thus there is a serious methodological problem in the Wang et al., study, and they don't bother to compare against the available concentration data which would have revealed this problem (as you do here), nor the observation-based source apportionment in Mahowald et al., 2008, which was consistent with the much smaller combustion sources. Instead one should say perhaps: Wang et al. (2014) taking into account the potential volatilized-P produced during combustion processes by assuming that all mismatches between observed deposition (<1000µm aerosols) and modeled-long-range transported (<20µm) deposition was due to combustion, estimated about 30 times higher global atmospheric P emissions from biomass burning and anthropogenic combustion processes (0.7 Tg-P yr⁻¹ and 1.8 Tg-P yr⁻¹ respectively).” Or simply don't refer to that paper here or mention it in passing, since it is deeply methodologically flawed. [J Brahney et al., 2015] discusses how to compare to the Tipping et al., data in a more explicit aerosol size manner, and extends the Mahowald et al., 2008 study, showing that one can match deposition and concentration observations at the same time.

“where EDu is the on-line calculated dust emissions in the model, F880 is a factor

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applied to adjust the P emissions to the global mean P content of mineral dust in the model domain of 880 ppm per weight as observed by Zamora et al. (2013), and EP is the resulted inorganic P emissions from mineral. P-containing minerals associated with dust particles are emitted in the fine and coarse mode with mass median radii (lognormal standard deviation) of $0.34 \mu\text{m}$ (1.59) and $1.75 \mu\text{m}$ (2.00), respectively. The apatite emissions from mineral dust calculated for the year 2008 amount to $1.034 \text{ Tg-P yr}^{-1}$ with 10% of it ($0.103 \text{ Tg-P yr}^{-1}$) in the dissolved form (Table 1).” How does this approach compare to the size resolved methods used in [J Perlwitz et al., 2015] for this mineral?

Section 2.0: model description; can you describe your aerosol size bin or modal structure for the primary aerosols in P?

Section 2.1.3: how do you include bits of insects and plants that would be part of pbap? How important is the neglect of these terms to your budgets do you think?

Please fix English by adding preposition (e.g. of): “(i.e. Nigeria downwind the Sahara Desert, Pakistan downwind the Thar Desert and China downwind of the Gobi desert).”

“In Fig. 4b, PO_4 deposition fluxes (wet and dry deposition) from the Vet et al. (2014) compilation and from observations at Finokalia Station (Mihalopoulos and co-workers, unpublished data) are also compared with the model derived fluxes for the PRESENT simulation.” What is the size distribution of the PO_4 in the deposition? Is it the same size as the modeled boxes? I also think you should present the data you are going to compare against as a section in the methods, and describe the characteristics of the data, especially as some of the data is from unpublished sources. We also need to know where this data comes from physically: is it all in Greece, or elsewhere?

“(MNB; see definitions of statistical parameters in Myriokefalitakis et al. (2015))” You also need to describe your methods in the methods section: it is not ok to refer us for basic information to another paper.

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Figure 7: maybe you want to reformat so that there won't be too much white space in the final figure for this?

“The present-day P solubility of deposited aerosols (hereafter SP = %DP/TP) is calculated to vary spatially significantly (Fig. 7a),” vary spatially significantly is awkward: please rephrase and only use significantly if you mean statistically significantly.

For your past and future estimates: Your P is strongly dependent on dust, and yet you don't include any changes in dust. I don't think you need to add much here, but just some statements that dust appears to vary strongly and perhaps be sensitive to humans climate change and/or land use [P Ginoux et al., 2012; N Mahowald et al., 2010; J Prospero and P Lamb, 2003], and thus that would also be an important drivers of changes in P and SP.

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