Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-892-RC1, 2018

Atmospheric Chemistry and Physics Discussions

Open Access

ACPD

Interactive comment

Interactive comment on "Phosphorus solubility in aerosol particles related to particle sources and atmospheric acidification in Asian continental outflow" *by* Jinhui Shi et al.

Anonymous Referee #1

Received and published: 18 October 2018

General Comments: I applaud the aim of this manuscript and I feel once it is modified slightly, that it will make an important contribution to the literature. I should say that a couple of years ago, we tried to do exactly the same data treatment using a data set collected in Crete. We had in total ∼100 data points and we were unable to find significant patterns. This manuscript has 170 data points and has managed (just) to see some real patterns albeit the correlations they find are often statistically significant but with correlation coefficients of ∼0.3!. In other general words, while the conclusions are interesting, they are not actually very strong. In particular the authors seem to divide the particles into anthropogenic or dust only. They do not include the importance of acid processing of inorganic particles (dust or anthropogenic) as wet aerosols asso-

Printer-friendly version

Discussion paper

ciated with clouds (high relative humidity) as a potentially important process. In fact they do discuss this in the text and state on page 10 that "Unfortunately , we were unable to quantitively distinguish the contributions of aerosol source and acidification to phosphorus solubility at this stage." Yet the text elsewhere minimises the possible contribution of acidification and emphasises instead anthropogenic particles which had high P solubility at source. This reviewer feels the manuscript would benefit by taking a more even balance between these two possibilities. As a final general point, we have just published a paper in Global Biogeochemical Cycles (Herbert et al., 2018), which the authors obviously could not have seen. However it does predict that acid processes in China could be an important source of. Bioavailable P as a plume which passes over location such as Qingdoa and on to the western Pacific.

Herbert, R. J., Krom, M.D., Carslaw, K.S., Stockdale, A., Mortimer, R.J.G., Benning, L.G., Pringle, K., Browse, J., (2018) Quantifying the effect of atmospheric acid processing on the global deposition of bioavailable phosphorus from dust. Global Biogeochemical Cycles. (5.79) https://doi.org/10.1029/2018GB005880

Specific comments: Line 16 of Abstract and elsewhere: The convention for what is called in this manuscript DP, is actually TDP (Total Dissolved P). That is the P measured after persulphate oxidation in solution. Please change to TDP throughout.

Line 24: The authors suggest that humidity plays an important role in converting refractory P to bioavailable P. The most likely mechanism is that suggested in Nenes et al., (2010) which is the acidification of particles as they cycle from clouds, where the pH is rather high, to wet aerosols (where the pH is very low) and back again (see Stockdale et al., (2016).

Introduction Line 5 Add in offshore areas and regions where P limits. . ...

General: Even in systems where N is the immediate limiting nutrient P can increase phytoplankton growth by moving the entire system to higher productvitiy.

Introduction page 2 line 9: The authors should comment/introduce the idea that anthropogenic processes can include the production of atmospheric acids, which can cause previously unreactive p to become bioavailable DIP. They discuss this possibility at length towards the end of their manuscript.

Methods page 4 line 7 Remove ‘in number of particles’ and Replace monitored with measured.

Page 5 line 19: What was the assumed value of Al in mineral dust that allowed the authors to assume that the particles were 8% by mass? I may have misunderstood what was written, in which case the authors should make it clearer.

Page 5 line 25 What is ‘floating’ dust? A dust storm?

Page 6 line 11 (and various other places including table 1) Aqaba is spelt wrongly. It is a b and not a d

Page 6 lines 31-34: If TP had high correlations with major elements (dust) and with heavy metals (anthropogenic) at the same time, is that not ambiguous?

Page 7 line 3: The actual correlation data is not given (or at least not given here). This reviewer is a little confused as to what the authors mean by ‘higher correlations’ and whether that also means lower p values.

Page 7 line 13 Are the authors convinced that soil dust (from deserts?) are an important source of DOP?

Page 7 Line 25 The authors might consider quoting Carbo et al., (2005) which presents the P solubility data for the Eastern Mediterranean in a more comprehensive manner than Herut et al. (2002). Carbo, P., Krom, M.D., Homoky, W.B., Benning, L.G., Herut, B., 2005. Impact of atmospheric deposition on N and P geochemistry in the southeastern Levantine basin. Deep-Sea Research II Volume 52: Nos 22-23, 3041-3053.

Page 8 line 24: The data in that graph is non-linear

Printer-friendly version

Discussion paper

Page 8 line 27 And because anthropogenic P is more likely to have interacted with pollutant gases to produce more bioavailable P

Page 9 line 4 Remove obviously

Page 9 line 14 I had the same problem with Sholkovitz's paper too. It ignores the possibility that anthropogenic acids can interact with mineral dust to produce bioavailable P (or Fe). The authors of this article suggest this might be an important process themselves in line 31 "which more efficiently serves as a sink . . . .derived elements." And later on page 10 "Unfortunately we were unable to quantitatively distinguish the contributions of aerosol source and acidification to phosphorus solubility at this stage". That means both should be retained as possible sources. In reality the answer is probably that both more soluble P in anthropogenic particles at source and more P made soluble by acid processes in air masses from polluted sources occur and are in different proportions in different air masses.

Page 11 line 29 There seems to be a mistake in the first half of the line. I read it several times and could not decide what was meant.

Page 11 line 34 How did the authors define 'acidification degree of 150 nmol nmol-1? nmoles of what?

Page 12 line 3 Remove obviously

Page 12 Conclusions Very well written and this reviewer entirely agrees with the conclusions.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-892, 2018.