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Interactive comment on "**Imbalanced phosphorus and nitrogen deposition in China's forests**" *by* **E. Z. Du et al.**

Anonymous Referee #1

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This is a potentially publishable article about an interesting topic (N:P ratios in deposition in China), but there are a few points that need modification and/or justification before the paper can be accepted.

Major point: How can you deduce dry deposition from throughfall and precipitation concentrations? This needs to be better explained or justified, and the following points are where I get confused.

"Phosphorus and N fluxes in the bulk precipitation and throughfall were estimated according to the volume-weighted mean concentration and annual precipitation." So you are taking your measured concentrations in each precip and throughfall and multiplying by annual precip? But not all the precip comes out in throughfall does it?

Canopy captured dry deposition was estimated as the difference between throughfall

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deposition and bulk deposition." How can you get dry deposition from the difference in two wet deposition numbers?

Later on you try to use this: "Based on the P enrichment in throughfall versus bulk precipitation, the mean canopy captured dry P deposition was estimated as high as 0.46 kg P ha-1 yr-1 in China's forests, being significantly higher than bulk deposition (0.38 kg P ha-1 yr-1) (paired t-test, df = 32, p < 0.01)." So I think you need to be a bit clearer: maybe make a skematic of your assumptions?

Again you say something here that doesn't make sense to me: "Although throughfall may still underestimate total deposition due to canopy uptake (Draaijers et al., 1996; Sparks, 2009), it is a better proxy of total N deposition than bulk deposition because bulk deposition only accounts for 63% of throughfall deposition."

Are you only concerned with soluble P?

It is important to clarify these points. If you are going to argue that from two wet deposition numbers you can deduce dry deposition and total deposition, then maybe you want to verify at a point where you have all three numbers to show that your approach works?

Your paper could be publishable with only wet deposition numbers, but I think you just need to be clear about what your results are.

Secondary points: In the comparison to the model results there are two issues:

i. "Anthropogenic sources have been traditionally thought to make only a small contribution to P deposition (Okin et al., 2004; Mahowald et al., 2008). In contrast, our results emphasize that anthropogenic sources near large cities can have significant impacts on the spatial patterns of regional P deposition. The urban hotpot of P deposition might be derived from intensive combustion-related emissions near urban areas (Wang et al., 2015) and a short-distance transfer of P-containing aerosols from P-rich farmland soils (Anderson et al., 2006)." The problem with this conclusion is that it con-

tradicts the observations here that the highest P deposition comes from regions close to the arid sources of dust: so your gradient suggests the dominant sources are dust related. Even using the smaller anthropogenic source of Mahowald et al., 2008, anthropogenic sources should dominate near cities in China (Figure 5h: Mahowald et al., 2008). So I would actually argue that your data supports Mahowald et al., 2008's budgets quite well. In contrast, from Wang et al., 2015, you shouldn't be seeing a P deposition gradient across China (Figure 3) from the arid regions, but only the combustion hot spots.

You could actually be a bit quantitative and do the comparison against the model output from these two models, to discriminate between them? See if there is a statistically significant difference in the comparison?

ii. Both the model of Mahowald et al., 2008 and Wang et al., 2015 only include long range transported P, so less than 10um or 20um particles. Your results, in contrast, should be including all P (unless you are only looking at soluble P, which should then not be compared to Wang et al., 2015 and only the soluble P in Mahowald et al., 2008). In forests, most of the P is in much larger mode particles, locally generated, according to previous observational synthesis (and the observational studies within them; e.g. Tipping et al., 2014). So it is likely you have a mismatch in the P deposition you are comparing (see Mahowald et al., 2008; Neff et 2009; Brahney et al., 2015 for more discussion on how to compare observational P fluxes across sizes to models or not). Notice that Wang et al., 2015 is very careful about the size of their emitted combustion P particles, but not at all careful about the size of the particles in the deposition P dataset they use (Tipping et al., 2014; which is dominated by large local generated primary biogenic particles, according to that study), compared to the particles that they are modeling (long range transported particles <20um). They actually use the mismatch in particle size and the resulting mismatch in deposition magnitude to deduce the extra-large combustion source they postulate, which makes that study problematic in its conclusions.

Minor points:

Section 3.1: I am not sure what I am supposed to get out of knowing that the data follows a log-normal distribution for P or N or N:P. Isn't this what we expect? I think this would be better illustrated with a figure of the distribution, if you think it's important enough to discuss?

"Spatial patterns of P deposition via bulk precipitation and throughfall showed large heterogeneity" This is a factor of 4. Globally, the deposition varies by several orders of magnitude, so I'm not sure I would call this a large spatial hetereogeneity. Maybe just say the values vary by a factor of 4.

Some English errors, especially in the few last lines of the introduction, should be fixed.

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