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## ***Interactive comment on “Sargasso Sea phosphorus biogeochemistry: an important role for dissolved organic phosphorus (DOP)” by M. W. Lomas et al.***

**Anonymous Referee #3**

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This manuscript describes the cycling of the dissolved and particulate phosphorus pools over the upper 200–500 m in the Sargasso Sea, with time-series data extending backwards to 2004. While there have been a number of studies that have suggested the importance of DOP cycling in this region based on transect and modeling results, this paper is the first to provide a full mass balance of phosphorus to really investigate the specific sources and sinks of P in detail. It is a marvelous addition to the literature on P in the Sargasso Sea. I have only a few comments that should be addressed prior to publication.

Methods: Many of the techniques used in the manuscript are for extremely low levels

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of P. Therefore, please provide more information on the blank corrections used for each technique? For the particulate P analyses (pg 10142) please provide an estimate of the possible contribution of DOP adsorption to the particulate P measurements - is it 10%? 50%? APA is based on what % of labeling? Please also provide more information on how P was measured in Case (2001) (Not listed in the References) in order to fully evaluate the time series discussion? It is not clear if changes with time in SRPxs data are due to the (unlikely) changes in methodology between the two studies?

POC/PPhos ratio: Page 10150-10151: It makes complete sense that the arithmetic mean of the POC/PPhos ratio is substantially different (higher) than that determined from the linear regression. Closer examination of the linear regression data (Figure 7), however suggests that the relationship between the two datasets is not all that strong – a few points seem to drive a relationship that otherwise looks more like a “shotgun blast” – at least for the POC. No R<sup>2</sup> or p-value are provided for the data relationship given in Figure 7. Please provide and consider adding the 95% confidence limit to the Figure. I find it difficult to believe that the linear regression results in an only 8% error based on the data provided? Please clarify.

#### P Budget and P Export Fluxes:

I am a big fan of mass balance models and applaud the authors for making the attempt at a mass balance budget for P in the Sargasso Sea. Errors are large, but the authors have done a very good job of at least starting to put constraints on the various input/export “arrows” when it comes to P cycling. I just have a few concerns/comments that I would like to see addressed. First and foremost, looking at the arrow “directions”, the authors have significantly more P flowing into the Sargasso Sea 150 m box than leaving it. In other words, you have a 135 mmol P/m<sup>2</sup>/y P demand (presumably based on the linear regression model, pg 10151 line 7, but this is not quite clear in the text), but a source of P of only 96 mmol P/m<sup>2</sup>/y. Within errors, this is pretty darn good. What is a bit more interesting/alarming is that the export of P at 150 m is only 12.8 mmol P/m<sup>2</sup>/y. If P is advecting IN from the sides, eventually TOTAL P concentrations would

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have to increase OR there is loss that is not accounted for. In the time-series discussion, increases in SRP are balanced by DOP decreases, so apparently, no net change in total P standing stocks (dissolved or particulate) have occurred. So where does the P ultimately go? It cannot accumulate indefinitely - especially as it is hypothesized that even more DOP and SRP are advecting into the "Sargasso Sea" box from the South?

Related to this, are the export fluxes of particulate P. I find it amazing that so much of the P is lost to the trap solutions. Of course, this is likely due to such little fluxes of P containing material to the Sargasso Sea trap cups as much lower rates of P remineralization in trap cups have been reported (see Buesseler et al., 2007 and O'Neill et al., 2005). That having been said, it seems likely that the sediment trap samples may be underestimating P export from the system, either due to hydrodynamic effects, under collection associated with low export events, or due to other mechanisms of P transport to depth, i.e zooplankton migration (see recent paper of Hannides et al., 2009 for the N. Pacific). How would such an increase in particulate P fluxes impact the DOP utilization estimates??

On a side note, I was also interested in the rapid decrease in DOP concentrations with depth associated with the increase in SRP. This suggests rapid remineralization (most likely) or advective loss of DOP (less likely) with depth. Have you gone through the exercise to determine if mass balance is reached on annual time scales (or even just from 2004-2008)? A similar calculation could be done with the decrease in export fluxes with depth from 150 to 500 m. This would be a great addition to providing insight into the remineralization processes that occur beneath the euphotic zone, but yet ultimately influence the potential source of P to the euphotic zone.

References cited that may be of interest here:

Buesseler, K.O., A.N. Antia, M. Chen, S.W. Fowler, W.D. Gardner, Ö. Gustaffson, K. Harada, A.F. Michaels, M. Rutgers van der Loeff, M. Sarin, D.K. Steinberg and T. Trull (2007). An assessment of the use of sediment traps for estimating upper ocean particle

fluxes. *Journal of Marine Research*, 65(3): 345-416.

Hannides, C.C.S., M. R. Landry, C. R. Benitez-Nelson, R. M. Styles, J. P. Montoya, D. M. Karl (2009) Export stoichiometry and migrant-mediated flux of phosphorus in the North Pacific Subtropical Gyre, *Deep-Sea Research I*, 56, 73-88.

O'Neill, L.O., C. R. Benitez-Nelson, R. M. Styles, E. Tappa, and R. C. Thunell (2005) Diagenetic effects on particulate phosphorus samples collected using formalin poisoned sediment traps. *Limnology and Oceanography Methods*, 3, 326-337.

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