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Biogeosciences Discussions

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

Anonymous Referee #1

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This work presents a large set of data on the P cycle at the BATS station, in particular focusing on the importance of the largest pool, the DOP. A main conclusion is that this pool plays an active role in the P-cycle, not only in the photic zone but also in the export – import budget. The large dominance of the DOP pool in the upper water column seems to be a common characteristic of the P-stressed oligotrophic marine regions. Even a small turnover-time for this dominant pool may therefore contribute extensively to the P-flux through the much smaller pool of P in the biota. I therefore consider this an important piece of work.

I have only some minor comments and questions: 1: The conclusion that biological

production occurs at Redfield ratio is based on regression lines such as those in Fig.7. I cannot find a description of whether these regressions are Type I and Type II. I suspect they may be Type I, but believe they should be Type II since there presumably is error in both X and Y. If the regressions are Type I, does it affect the conclusions if the regressions are changed to Type II?

2: There is very little description in the M&M on how blanks were made for the different analyses of phosphate. I thought this was one of the problems with the nanomolar measurements of phosphate and a more detailed description therefore warranted.

3. It is stated that: "The similarity in seasonal and depth distributions for [PPhos] and whole-community APA suggests that much of the measured whole-community APA is associated with particles and not in solution". They way I read this, I do not see the necessity in the logic. Could you not get a lot of free APA in combination with a good correlation between particulate-P and APA if organisms release all the AP produced into the water, but the life-time of free AP molecules is short compared to the mixing processes of the water?

4. The DOP concentrations are low at the deep (500 m) measured, particularly in winter-spring. To me this seems like another good argument for the degradability of the DOP, at least over seasonal time scales. I could not find this argument being used by the authors, and if I have not just missed a point here, I would like to know why the authors have chosen not to use this?

Interactive comment on Biogeosciences Discuss., 6, 10137, 2009.