Biogeosciences Discuss., 6, C4091–C4095, 2010
www.biogeosciences-discuss.net/6/C4091/2010/

Biogeosciences Discussions

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

M. W. Lomas et al.

michael.lomas@bios.edu

Received and published: 22 January 2010

Anonymous Referee #2 Received and published: 16 December 2009

The authors present a time series of phosphorus measurements at the Bermuda Atlantic time series, including inorganic and organic phosphorus, particulate phosphorus, both suspended and sinking, and rates of alkaline phosphatase activity. This is a unique and insightful dataset, with the main conclusion being that the exogenous DOP pool supports 30-60% of primary production in the western subtropical Atlantic. The importance of DOP as a substantial P source has been postulated by a number of studies over the past 5 years (and this is recognised by the authors) and thus the con-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

clusions of this study are not new. However, findings from this study support previous modeling output and observations, which have focused primarily on the eastern tropical Atlantic and therefore this study extends the current understanding of phosphorus dynamics in the North Atlantic to the western subtropical Atlantic.

I have a few questions and comments. 1. Page 10153: The authors estimate that horizontal advection of DOP could supply 43 mmol môĂĂĂ2 yôĂĂĂ1 of DOP, or 32% of the annual phosphorus demand. Are the authors assuming that 100% of the DOP transported into the region of BATS is bioavailable? Other studies that have made similar calculations have divided the DOP pool into labile, semi-labile and refractory with some time scale of utilisation. If the various estimates of DOP supply are to be compared, it is important to know what the assumptions are.

This is an excellent point, one that we neglected to adequately address in the first version. The calculated DOP was indeed assuming 100% bioavailability. We do not have any direct data on composition of the DOP pool so we can't with confidence partition to labile, semi-labile and refractory. We assume that the 75% of the DOP pool that is comprised by esters is labile on the annual scale (the time scale considered in building this P budget), and now clearly state this. However, we have also added a sentence that recent molecular evidence suggests phosphonates are degraded in support of growth by marine cyanobacteria and so classifying phosphonates as 'refractory' may be inaccurate. We have re-written this paragraph in the discussion as follows:

"We have compiled available DOP transect data for the western subtropical North Atlantic. In contrast to the eastern North Atlantic, there is an increasing gradient from 20 – 38 oN (Fig. 8). We do not have data from the equator and 20 oN where Mahaffey et al. (2004) show a DOP gradient in the eastern North Atlantic to be from the equator to 10oN. Using the average (1992-2002) Ekman transport from just north of the BATS region $\sim$0.05 x 106 m3 s-1 (34oN, data from Gordon and Giulivi, 2008) and the DOP concentration at 34oN, $\sim$150 μmol m-3 (Fig. 8), we estimate the DOP transport rate into the BATS region (here defined as 34-28oN, 70-60oW) as $\sim$43 mmol m-2 y-1. The

BGD

6, C4091–C4095, 2010

precise chemical composition of the DOP pool is unknown and therefore one cannot separate the DOP pool into labile, semi-labile and refractory designations with great confidence. Previous studies (eg., Clark et al., 1999; Kolowith et al., 2001) suggest that roughly 75% of the DOP pool is comprised of labile esters, with the remainder being 'refractory' phosphonates. We assume this to be the case for BATS as well and we have revised the DOP transport rate downward to ∼32 mmol m-2 y-1 to reflect only the labile fraction of advected DOP. It is worth pointing out however that recent research shows that phosphonates may be utilized by autotrophs to support growth (Dyhrman et al., 2006; Moore et al., 2005; Palenik et al., 2003), and considering them as refractory may be inaccurate. This southward Ekman flux of putatively labile DOP (∼32 mmol m-2 y-1) could account for ∼25% of annual phosphorus demand (∼135 mmol m-2 y-1) at BATS. A similar calculation for SRP but assuming all SRP is bioavailable (Lomas et al. unpubl. SRP concentrations) results in an estimated SRP transport rate of ∼22 mmol m-2 y-1. The flux of nutrients (mol time-1) approaches zero with the decrease in Ekman volume flow to 28oN (the BATS site is at 31o 40'N). It is worth noting that DOP measurements are needed for the western tropical North Atlantic closer to the equator to determine if there is a northward DOP flux as there is in the eastern North Atlantic. Compiling all of the terms calculated in this section and data from the literature a first order phosphorus budget can be completed for the BATS region (Fig. 8). The remaining term is atmospheric deposition. Atmospheric inputs of phosphorus, while perhaps important as singular events, are a minor source of phosphorus to the euphotic zone over the year (Baker et al., 2003; Michaels et al., 1996). While all of the input terms in this budget have large uncertainties, their sum (∼85 mmol P m-2 y-1) roughly balances the biological phosphorus demand of 135 + 58 mmol P m-2 y-1 (Fig. 8). It is worth pointing out that any increase in autotrophic particulate N:P ratio above Redfield would serve to bring the demand estimate closer to the supply estimate, thus strengthening this first-order budget. Therefore, we conclude that most of the necessary inputs of phosphorus have been accounted for and that exogenous DOP supports 25 - 50% of primary production in the western subtropical North Atlantic."

BGD

6, C4091–C4095, 2010

3. There is insufficient detail on the incubation conditions used to determine APA activity. What volume of seawater was used? Were the samples incubated in the light or the dark? Why was a saturating rather than ambient concentration of phosphorus added? Why was a kinetic study not performed? Adding 10uM of phosphorus to a community that is adapted to nM concentrations must have some stimulating effect? Is incubating for a short period of time a way of avoiding measuring the response of a community to high phosphorus?

We have added additional details to the APA incubation conditions, specifically the volume and the light conditions. In answer to the question of saturating additions, this was done to achieve a maximal response, following previously published protocols (Ammerman et al.). Kinetic studies were indeed done previously at BATS and more recently in other regions of the western N. Atlantic, and this data is used for calculations in the discussion. Kinetics curves on the BATS cruises were not done due to time and sample processing constraints. Re: response to 10uM organic phosphorus addition, close examination of the time course of fluorescence from 1 to 6 hours always showed a linear response suggesting that over that timeframe indeed there was no enhancement of APA induced by such a large addition. In addition, the method used is consistent with previously published work in the western North Atlantic.

4. Page 10154: I would like to draw the author's attention to a recently published paper: Torres-Valdes et al., 2009. Distribution of dissolved organic nutrients and their effect on export production over the Atlantic Ocean. Global Biogeochemical Cycles. This is a modeling study following on from work by Roussenov et al., 2006, which shows that DOP is responsible for driving up to 70% of the export production in the North Atlantic. This is a useful comparison to this study.

We thank the reviewer for mentioning this paper. It was indeed helpful and relevant when working on this revision to the current manuscript.

5. 10154: How do the authors know that the changes observed in the phosphorus

BGD

6, C4091–C4095, 2010

pools are not due to changes in analytical techniques? As pointed out earlier by the authors, the MAGIC-SRP techniques used to generate recent phosphorus data was not used by Case in the earlier studies.

We don't know with 100% certainty that the decrease in DOP with time is due to net export of P. It is possible that the HTC method used by Case et al. and the PO method used in this manuscript resulted in different oxidation efficiencies for DOP, and therefore is a confounding effect. However, the methods review by Monaghan and Ruttenberg 1999, suggest that for most compounds there is no difference in the oxidation efficiency between ash hydrolysis methods and acid persulfate methods. Moreover, the recoveries for standard compounds given by Torres-Valdes et al 2009 for the UV oxidation method are virtually identical to the recoveries that we observe for the acid persulfate method (unpubl. Data). This suggests to us that any method differences are likely to be minor. That said in the original manuscript we state the decrease in time due to export is 'plausible' and not a firm conclusion.

6. Do the authors think it is important to know which organisms are responsible for producing APA and thus accessing the DOP pool?

Most marine microbes have the ability to hydrolyze P-esters, so perhaps a more appropriate question is what are the rates of APA for different groups of organisms? Understanding who is utilizing P-esters is important but beyond the scope of this specific work. There are other works, published and in press, Sebastion and Ammerman 2009, Casey et al. 2010, Orchard et al. 2010, arising from the project that funded this work that are focused on which groups of organisms are hydrolyzing P-esters and at what rates.

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