

Interactive comment on “Top-down, bottom-up and physical controls on diatom-diazotroph assemblage growth in the Amazon River Plume” by M. R. Stukel et al.

Anonymous Referee #1

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The manuscript "Top-down, bottom-up and physical controls on diatom-diazotroph assemblage growth in the Amazon River Plume" by Stukel et al. uses a regional biogeochemical model to investigate the factors controlling the development of diatom-diazotrophs assemblages (DDA) blooms. The authors find that DDA blooms are associated with prolonged retention time within the silica-rich waters from the Amazon River Plume (ARP) and a reduced grazing pressure.

The study is interesting and deals with important issues related to the controlling factors and fate of DDA that are still poorly understood and, as such certainly deserves publication. On the whole the paper is well written. However, I have some concerns that leave place for improvement:

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General comments:

1) The authors claim (page 13933, line 13) that a bottom-up mechanism suggested by Subramaniam et al. (2008) cannot explain the the DDA dominance over UMD. Thus, they hypothesise a top down control that limits UMD growth relative to DDA in low-nitrogen mesohaline waters. However, the inter-specific competition for nutrients (see equ. 4 and 8) may well explain DDA dominance over UMD (given enough P and Si, as the authors state on pg 13950 line 27/28) and the successional patterns or competitive-exclusion (page 13952 line 4/7) observed along the ARP.

I suggest that an assessment on how competitive outcomes shape community structure is needed to conclusively differentiate between the contribution of bottom-up and top-down controls.

2) Although salinity may be convenient parameter to compare model results with observational data (Subramaniam et al. 2008) the analysis of this parameter promotes the idea that salinity gradients affects directly DDA and mesozooplankton growth (eg. pg 13947/8). Salinity gradients fail to explain the mechanisms that are at work, and for an in-depth analysis of the controlling factors that promote DDA blooms nutrient gradients should be discussed instead. The authors do recognise that salinity gradients are "coincidental and not causative" however they fail to discuss the "causative" factors. One example is (pg 13947 line 16 onwards): the emphasis on DDA growth and mortality rates covariances with salinity distracts the reader from the mechanism controlling the reduction of mesozooplankton biomass that initiates the DDA bloom. The causes of this reduction are not not comprehensively discussed. The authors suggest that physical dilution is playing a role however, the contribution of other factors, such as the decline of diatoms biomass which is in turn ultimately likely related to Si decline, are not discussed. The authors do not provide a comprehensive analysis and enough convincing arguments in support of their conclusion.

I think that the paper would benefit from from an in-depth discussion of the factors that

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directly affect community structure and not on spurious correlations with "coincidental" parameters.

3) The study is motivated by the important potential role DDA have in enhancing C export. However, this is nowhere discussed and it would be nice to see some discussion on this issue. Also, how do the model results compare to regional N₂ fixation rates?

Specific Comments:

1) Section 3.3: To investigate the successional patterns along ARP It would be nice to see how nutrients, phytoplankton and zooplankton biomass develop along the float trajectories. I would suggest adding plots of these parameters similarly as done in Fig. 6b for DDA.

2) As discussed above, to assess the "causative" factors that control DDA bloom formation Figure 7 and 8 would be more informative if showing parameter variability as a function of Si concentrations, N/P ratios and diatoms concentrations along the Lagrangian float trajectories

3) Section 3.4: The authors put a lot of effort in making sensitivity experiments. However the results presented in Figure 9 are very condensed and hard to follow. I would suggest to put more focus on the parameters that are central to the paper (DDA, mesozooplankton, diatoms).

Most parameters appear insensitive to changes in the phosphate half saturation constant and in phosphate river supply suggests that phosphate concentrations were never limiting phytoplankton growth? How does that compare with observations? I think this merits further discussion.

4) Page 139937 line 10: there is an inconsistency, the term Θ_L should be is θ_L (small caps).

5) Table A1: The DDA growth penalty symbols are inconsistent and have been switched?

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