

Interactive comment on “Evidence for aggregation and export of cyanobacteria and nano-eukaryotes from the Sargasso Sea euphotic zone” by M. W. Lomas and S. B. Moran

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We agree with the reviewer that it is unfortunate that the 1-10 μ m fraction was not collected (and this is being corrected on current sampling efforts), but we don't believe it “significantly compromises the usefulness” of the pump data. The only assumption made associated with the pump data is that the difference in pigment concentrations between bottle and pump is due to single cells in the 1-10 μ m fraction. All the other assumptions (pigment/cell and POC/cell to calculate carbon concentration associated with the pigments) had to be done regardless of what size fraction of pigments were measured. So while it does increase the ‘shakiness’ of the estimate, the reviewer is OK

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with that (as s/he states), and future efforts will provide a more well-constrained estimate of pico- and nano-plankton contributions to export by removal of this assumption.

1) trap samples and filling of the tubes with brine. It is the practice (and has been for the decade that Lomas has been a co-PI of BATS) that PITS tubes are filled to near the bottom of the baffles at the top of the tube (roughly 4-5” from the true top of the tube). So while a false bottom is created is not as dire as the reviewer might think (or remember from prior participation in BATS cruises). Furthermore, a recently concluded project (Buesseler et al.) comparing NBST traps and PITS traps show that they yield similar POC fluxes with no systematic differences (Owens et al. abstract presented at Chapman conference in 2009, ALSO meeting 2010). The NBST traps are not subject to hydrodynamic bias and did only have brine in the very bottom \sim 2” of the tube. This is further evidence that the PITS trap POC fluxes used in this are not biased by a false bottom as originally expected and so we don't feel those data are compromised either.

2) Similar pigment composition despite particle size mismatch suggesting aggregation. Yes, we agree this is quite interesting, and in fact it was this observation led us to think more deeply about the processes of aggregation within the constraints of this dataset.

3) Figure 6 and ballasting. We expected to see an increasingly large fraction of pigments due to mPF due to mineral ballasting of diatoms, which we didn't really see in the data. As we don't have any other data on diatoms (other than pigments) and the focus of the paper isn't actually on diatoms, we've just removed the reference to ballasting; it does not impact the paper in a meaningful way.

4) “Surprise” at correlation between *Synechococcus* and export. We weren't really surprised by the correlation (we don't think the sentence is written in a way to convey surprise and we are sorry for the confusion), rather at the outset we didn't know if this was just a seasonal co-variance or a real mechanism linking export of *Synechococcus* to POC export. We have added a phrase to that sentence to state it could be both, but then go on to explore the later (as the former, co-variance, isn't all that exciting).

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5) Size of fecal pellets. We don't think that fecal pellets are always larger than 53um. Geochemists traditionally use a 53um filter on in situ pumps to define the 'sinking' particulate fraction and we were just trying to speak to both the geochemical readers and the biological readers, who are not always of the same mindset. We have in the original manuscript a short paragraph talking about smaller size fecal pellets. As well we've now add an explicit sentence on how pellets might be get broken into smaller pellets.

6) Differential degradation rates of Chlorophyllide and Chlorophyll. We state in the original version that the change in ratio is due to a 10-fold larger loss of Chlorophyllide. The obvious (at least to us) explanation is that Chlorophyllide is less stable than Chlorophyll; we have now added an explicit statement that the different decreases with depth are likely due to different stabilities of the molecules.

7) Shaky estimate, although the reviewer doesn't mind. We agree, there are assumptions that need to be made, but we feel they are reasonable assumptions, and it seems the reviewer feels that as well. We feel the importance of getting some real observational numbers on the pico-/nano-plankton flux contribution question outweighs the use of assumptions; we are glad the reviewer agrees.

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