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Interactive comment on “Increased ocean carbon export in the Sargasso Sea is countered by its enhanced mesopelagic attenuation” by M. W. Lomas et al.

M. W. Lomas et al.

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We thank this for reviewer for seeing the importance of this type of analysis, and for his/her important comments. Below we have addressed all of the major and minor comments. Where we don't necessarily agree with reviewer we provide our rationale. Major comments:

1. Impacts of NAO on nutrients. Indeed temporal variability of subtropical mode water nutrients maybe an important factor in explaining our results. The mechanism proposed by Palter et al (2005), enhanced mixing during NAO negative phases leading to lower nutrient concentrations would in principle serve to reduce the possibility of

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increased primary production that we have observed. The premise for Palter et al's hypothesis is the observation of lower primary production in the 1958-1963 period relative to BATS. It is worth pointing out that some of the early primary production data are thought to be too low due to metal toxicity (see papers by Michaels et al. 1994, 1996, Jenkins et al. 1992), and therefore this comparison is perhaps more complicated than stated by Palter et al. Regardless, we plot nutrient concentrations on the 26.28-26.32 isopycnal range (Figure 2) which is shallower than the core of the STMW but still within the STMW. This data record doesn't show the multi-year decline in nutrients hypothesized by Palter et al. Perhaps this is due to the fact that we aren't looking at the core of the MW which is deeper than current mixed layer depths or perhaps the shift in the NAO isn't strong or constant enough to impart the hypothesized effect (the late 50's NAO was consistently very negative). We have expanded the discussion in the paper to address this important point raised by the reviewer however it is unlikely to be significant in explaining the observed patterns. The following paragraph was added: "There are non-local impacts of changes in NAO as well. For example, Palter et al. (Palter, 2005 #5948) suggest that the phase of the NAO controls the nutrient reservoir in the North Atlantic subtropical mode water in a counterintuitive manner. They suggest that an NAO negative phase during mode water formation results in lower nutrient concentrations within the mode water which may reduce downstream primary production dependent upon convective mixing of nutrients from the mode water (i.e., at the BATS site). In contrast, NAO positive phases during formation of mode water results in higher nutrient concentrations and possibly higher downstream primary production. Nutrient concentrations shallower than the core of the mode water (mode water core is at $\sigma_\theta \sim 26.4 \text{ kg m}^{-3}$), but still within the mode water do not show any significant trend with time (Fig. 2). Perhaps this is due to the discrepancy in depths between the mode water core and MLD, or perhaps the shift in the NAO is not strong or constant enough to impart the effect hypothesized by Palter et al. While important, this non-local phenomena seems to be of minimal importance in explaining the observed patterns."

2. Linking aggregation to enhanced remineralization. At the outset, we have changed

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'would explain' to 'could explain'. We agree with the reviewer that relationships between particle 'type' and density (one controlling factor in its settling rate) are very complicated. A further complicating factor, and the one that sits at the heart of the reviewer's question as we see it, is if the particle is actually a fecal pellet or if it is a largely intact phytoplankton either solitary (larger eukaryotes) or as an aggregate. We don't have data on the relative contributions of these two mechanisms; although we do know that metabolism of zooplankton (release of dissolved material) is a minor (~15%) contributor to POC fluxes at BATS (Lomas et al. 2002). Our rationale for the statement made is as follows. If small particles aggregate the settling velocity is more than likely going to increase (settling speed of a solitary prokaryote is effectively zero) which could explain the increasing POC flux with increase in abundance of small prokaryotic cells. The link between aggregation and enhanced remineralization has to do with the fact that the small prokaryotes don't have mineral shells and are likely to have a faster remineralization rate as a result. This enhanced remineralization could be due to the lack of a mineral test, the slower (but still positive) sinking speed of non-mineral aggregates, some combination of both, or even the nature of the particle (fecal pellet vs. aggregate). This is the concept behind the mineral-ballast hypothesis (Armstrong et al. 2002), and a number of studies (e.g., Berelson et al. 2001, Buesseler et al. 2007) suggest that remineralization profiles are stronger for non-mineral ballasted material than mineral-ballasted material. In answer to the reviewers question of a change in the POC/N ratio, no there is no change with time; with depth PON is preferential remineralized so that the POC/PON increases with depth. It is important to note that bacterial utilization isn't the only process utilizing POC below the euphotic zone. The work of Debbie Steinberg in the Pacific (Steinberg et al. 2008) shows that zooplankton are quantitatively as important a consumer of POC below the euphotic zone as bacteria. We feel the discussion section on the observed increase in remineralization is sufficiently clear and referenced that we have not modified the text, in light of the fact that a discussion of particle source and decomposition is beyond what the available data can support.

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3. The reviewer is absolutely correct and we apologize for this misstatement. It has been corrected in the manuscript, and the sentence now reads – “Absolute haptophyte biomass, a fraction of which is attributed to the coccolithophore *Emiliana huxleyi* in this region (Haidar and Thierstein, 2001), has not significantly changed over time (Mann-Whitney Rank Sum, $P = 0.55$; Fig. 3B, Table 2). . .”.

4. Integrated abundances of *Prochlorococcus* do not show a significant increase, although there is the trend for higher abundances with time. We suspect there are several possible explanations. First, not all *Prochlorococcus* strains can use nitrate (Martiny et al. 2009) and so if only that subset is responding to the hypothesized enhanced NO_3 availability it may not be sufficient to result in a significant increase for the entire population (which is what was included in the correlation). Also, *Prochlorococcus* is at its annual minimum during the winter/spring period, quite likely to do substantial environmental selection for a subset of all strains, and these conditions may not be optimal for growth for these strains and therefore limit growth potential. Lastly, it is known for the Sargasso Sea as well as the Costa Rica Dome upwelling that *Synechococcus* competes very well for nutrients and this competition may further reduce the ability of *Prochlorococcus* to show a significant growth response. All of this is educated conjecture as we don't have any firm data to support any of these explanations. We have not included an explanation in the manuscript for the lack of response in *Prochlorococcus*, but wanted to share our thoughts with the reviewer on this point.

5. Inclusion of $\text{Si}(\text{OH})_4$ as a panel in figure 2. The recent paper by Krause et al. does a much better job of discussing the relationship between silicate concentrations and declining diatom abundances than we have room to do here.

Minor comments:

1) Bandpass filters wavelength range. The ‘+’ part was there, but we didn't catch this error during proofreading. This has been corrected.

2) Explain QC/QA. We use 3000m water as an internal standard and set a range of

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acceptable values to help in our evaluation of instrument performance. We do not use this for rigorous QA/QC testing as would be done with other standards. We have removed this from the sentence to avoid confusion.

3) Inorganic carbon into photosynthesizers. While photosynthesizers are a subset of particulate organic matter, we see the reviewer's point and have modified this sentence.

4) 'Manuscript' has been replaced with 'study'

5) The reviewer is absolutely correct in that strong seasonality for us is minor for the higher latitudes. We have rewritten that sentence as follows: "In addition to the seasonal pattern in Sargasso Sea biogeochemical processes (Steinberg et al., 2001), underlying multi-year trends in biological carbon pump parameters are apparent and statistically robust. "

6) Assimilation number is indeed chl_a normalized, this has been changed.

7) We have revised this sentence as follows. "Supporting the observed year-over-year increase in TChl-a, primary production and POC export was increasingly greater consumption of NO₃- and PO₄- (Fig. 2)."

8) Statement on changes in trap collection efficiency. We see trap collection efficiency as variable (despite the constant trap design), with coefficients of variation among 3 traps side by side as high as 30% on occasion. In addition we have no reliable information on sub-euphotic zone currents and how they might change over time, these currents will impact collection efficiency. As a result we were trying to be 'honest' and recognize possible confounding variables related to direct measurements of carbon flux that we can't account for.

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