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Biogeosciences Discussions

Interactive comment on "Carbon fluxes forced by anticyclonic mesoscale eddies generated by islands at the subtropical NE Atlantic Ocean" *by* S. Lasternas et al.

Anonymous Referee #1

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The Lasternas et al. manuscript is a nice study that addresses phytoplankton biomass and cell health at stations within anti-cyclonic eddies (AE), cyclonic eddies (CE), and offshore regions. Their use of cell vitality stains to measure the proportion of living cells, as well as cell lysis rates bring a novel approach to the study that allows them to hypothesize that AE's and CE's are systems evolving in different directions due to divergent upwelling and nutrient characteristics. However, there are several ways in which I believe the manuscript could be strengthened.

1) Vertical distributions. The authors measured their parameters at either 2 or 5 depths, yet data is seldom differentiated by depth. In particular, in the graphs and charts it

is unclear whether the authors are referring to surface concentrations, a mixture of surface and deep concentrations, or vertically integrated biomasses. It would be very useful to see vertical distributions of the measured parameters, or (for parameters that were only measured at surface and DCM) a separate graph for surface and DCM values.

2) Cellular lysis rates. The patterns of cellular lysis shown by the authors are compelling and lend credence to their hypotheses about the underlying mechanisms controlling the AE and CE ecosystems. However, I am concerned about their assumption of a constant PEA:chl ratio and believe that this should be addressed. In particular, in oligotrophic regions with greater water clarity, cells often have reduced pigmentation and thus a higher C:Chl ratio. If esterase concentration scales with biomass rather than pigmentation, this would lead to an increased PEA:Chl ratio, suggesting that utilization of a constant PEA:Chl ratio is underestimating the cellular concentration of esterases. Underestimation of PEA would lead in turn to an overestimation of cellular lysis in the oligotrophic (AE) region. Since this potential artifact could drive the trend that the authors find with respect to lysis, I think they need to address the sensitivity of their results to a variable PEA:Chl ratio. Luckily, I believe the suite of parameters they measured allows them an opportunity to address this question. In particular, their flow cytometry and microscopy measurements should allow them to assess phytoplankton carbon biomass (from biovolume and appropriate conversions – e.g. Menden-Deuer & Lessard, 2000; Garrison et al., 2000) and hence test for cross-system variability in the C:Chl ratio and thus put bounds on the potential variability of the PEA:Chl ratio.

3) The manuscript title begins with "Carbon fluxes", yet the only carbon fluxes measured and discussed in the manuscript are total primary production and particulate/dissolved primary production, and the only values given are for the ratio of dissolved organic carbon production to total organic carbon production. I believe that the authors should attempt a slightly more comprehensive carbon budget, since they likely have the data for it. In particular, what was the total primary production at each site?

How does this compare to total phytoplankton biomass (i.e. what was the turnover rate of the phytoplankton). How does DOC production compare to cellular lysis rates?

4) Primary production measurements – Some specific details need to be included with these measurements, especially since only 3 hour incubations were used. In particular, at what time of day were the incubations conducted? How were 3 hour incubations (potentially taken at different times of day) used to estimate daily PP rates for comparison across different systems? Time of day could potentially also affect the fraction of viable cells.

5) Exudation v. lysis. The authors seem to attribute the increased proportion of PP to the dissolved phase in the AE to increased cellular lysis. This seems unlikely to me, since during three hour incubations only a small fraction of cellular carbon will become labeled. Cellular lysis would thus be leading to production primarily of unlabeled DOC. Given the high concentration of 14C passing to the dissolved pool, it seems more likely to be a result of cellular exudation (which might primarily be of recently fixed – and hence 14C rich - sugars) in response to nutrient stress (e.g. the "paradox of the phytoplankton" – Bratbak & Thingstad, 1985).

6) Nitrogen limitation – While there is clearly decreased nitrate in the AE, the highly elevated ammonium levels actually lead to relatively similar total DIN concentrations between AE and CE. Since ammonium is typically preferentially taken up by phytoplankton, it thus is not apparent that there is nitrogen limitation in the AE. Do the authors believe that other nutrients are ultimately limiting? They mention silica, but this only holds for diatoms. A more explicit discussion of limitation, potentially broken down into different groups, would be useful. Which groups dominated biomass (not abundance) in each region and what were their nutrient requirements/cellular health?

7) Do the authors have any zooplankton measurements? It seems like a comparison of grazing rates or even zooplankton biomasses would help greatly in their interpretation of system dynamics.

Other Comments: The manuscript seems to be a bit longer than it needs to be. In particular, there is a lot of discussion of the physical generation of eddies that I feel is unnecessary in a manuscript that is really focused on the biology. The authors do not have much physical evidence to address the formation of the eddies, and hence extensive discussion of this topic only dilutes the focus of the manuscript. In particular, I think they could cut back on much of the introduction (for instance lines 10-30 on page 10243) and some of the discussion (for instance line 22 on page 10256 to line 6 on page 10257).

The methods section can be shortened a bit. In particular some of the details of the live/dead staining is repeated/scattered and can be more effectively combined.

End of abstract: "weakness of the carbon pump" – this is a (justifiable) assumption, but is not directly supported by their measurements and hence probably not appropriate in the abstract.

The authors should be explicit when referring to total cells, live cells, and dead cells. I often found myself wondering (when they mention total nano-microphytoplankton) whether they were referring to total cells or to live cells.

Pg. 10257, line 18. "mortality" was not measured. Mortality differences are inferred from differences in the proportion of non-living cells, but this assumes a similar turnover rate for non-living cells in the different systems. Likely true, but still an assumption.

The manuscript has many grammatical errors, too many to enumerate individually. While they do not impair understanding of the science, they are distracting. I would recommend having a native speaker proofread the manuscript.

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