



Interactive
Comment

Interactive comment on “Net community production of oxygen derived from in vitro and in situ 1-D modeling techniques in a cyclonic mesoscale eddy in the Sargasso Sea” by B. Mouriño-Carballido and L. A. Anderson

Anonymous Referee #3

Received and published: 31 May 2009

This paper investigates the metabolic balance in the Sargasso Sea during an eddy event through the use of in situ dissolved oxygen measurements combined with a 1D model and through in vitro bottle incubation studies. The aim of the paper was to see if there was a discrepancy between the two methods when they are conducted in an eddy and thus whether a frequent explanation of in situ vs in vitro differences due to mesoscale variability is reasonable. The authors found that the two methods disagree in the eddy. This paper was interesting and well-written and I recommend publishing it after minor revisions.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive
Comment

My largest disagreement with the paper is that because the two methods disagree in this particular eddy the authors state that mesoscale features are not responsible for causing the difference between the in vitro and in situ techniques. However, the authors also state that this eddy is in a state of decline. Thus perhaps it is not surprising that the in vitro technique, which has a shorter time scale and a more local spatial scale, shows net heterotrophy reflective of just the decline phase of the eddy while the in situ technique which averages over longer temporal and spatial scale may be reflecting production during both the growth and decline of the eddy. Thus while this work is certainly valuable, I do not think the authors can necessarily conclude that in general eddy features cannot explain the difference between the techniques. Thus statements like the one on page 3251, lines 15 to 17 are too strongly worded.

I appreciated the details given about how the model was constructed and the various formulations used. Such detail is necessary in order for a reader to evaluate the work. Have the authors considered the possibility that their optimization technique may be finding a local minimum rather than truly the biological oxygen flux? In Table 2, the fluxes for advection at 100 m and horizontal advection are both very large – much larger than the NCP. How are errors in the two fluxes coupled? How much uncertainty is added to NCP estimates because of the uncertainties associated with these fluxes?

Overall, a more extensive discussion of the uncertainties would be good. It seems like the uncertainty in the in situ NCP is given as the standard deviation between the three cases but what about systematic uncertainties associated with using a 1D model? Or with the gas exchange parameterizations? With uncertainties in the oxygen measurements?

A little more detail on why those particular stations were selected would be useful.

Sometimes K is given in units of $\text{m}^2 \text{ s}^{-1}$ and sometimes in $\text{cm}^2 \text{ s}^{-1}$. It would be better for the authors to be consistent.

Interactive comment on Biogeosciences Discuss., 6, 3237, 2009.