

***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 #1**

Received and published: 20 May 2009

This paper compares two disparate techniques for determining net community production in a defined water mass, the center of an eddy. The authors demonstrate that the two techniques yield different answers and conclude that the difference cannot be due to the passage of eddies through the area, as has been suggested for previous studies exploring discrepancies between these techniques. The paper presents an important comparison, but requires revision to highlight its main message and provide additional comparisons.

The authors mention that 14-C incubations were also performed on this set of cruises.

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As a separate measure of productivity performed in incubated bottles, this would be a very valuable comparison to the two other estimates given in this paper. A figure showing this data and discussion of the comparison to the NCP results would improve the paper.

In general, the main point of this paper gets buried in the many modeling details it presents. It would help to bring out the point even more in the abstract and introduction. In the abstract, the authors should point out not only the list of things that could cause the differences observed, but specifically say that mesoscale variability due to the passage of eddies cannot be the cause. At the end of the second paragraph in the introduction, the authors should elaborate on how mesoscale processes have been hypothesized to cause differences in these two techniques. By presenting a specific mechanism here, the authors would set up their refutation of it later.

A number of discussions regarding model details could be eliminated or shortened and moved to an appendix in order to reduce reader distraction from the central point of the paper: the trial and error discussion of a form for  $K$  spanning pages 3242–3243, the discussion of an aborted attempt to model salinity at the end of page 3243, the parameterization of the gas exchange coefficient and  $O_2$  Schmidt number on page 3244 if these are just from cited papers, speculation regarding the appropriate value of diffusivity in large-scale budgets at the end of section 3.2. It's unclear why the nitrate and chlorophyll data is presented, when it doesn't get discussed except for the depth of the nitricline which could just be stated in text. The discussion of non-Redfield oxygen production to nitrate uptake on page 3248 seems odd as this area is already known for recycled production based on ammonia or urea.

Some additional details are warranted in this paper, if explained briefly. How were eddy center characteristics chosen (line 25 page 3240)? What is the effect on the model of using a constant solar flux and how consistent was this flux between different time periods? Was the non-solar heat flux computed by the model reasonable compared to the meteorological measurements or weather models? When several profiles within

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a short time period were available, how were these treated in the model (averaged together, considered separately for an estimate of error, etc)? Why not use the modeled surface temperature rather than linearly interpolating in time to obtain O<sub>2</sub> solubility estimates?

Figures 1-2 and Table 2 are very difficult to see at this resolution. Figure 1 could be expanded by removing nitrate and chlorophyll to make only four panels. Figure 2 just needs to be bigger.

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