Biogeosciences Discuss., 6, C477–C479, 2009
www.biogeosciences-discuss.net/6/C477/2009/

Biogeosciences Discussions

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 #2

Received and published: 21 May 2009

GENERAL COMMENT

This is a well-written piece of work that presents a meticulous geochemical assessment of the net community production of dissolved oxygen in a cyclonic mesoscale eddy and compares it with in vitro measurements. The manuscript concludes that the irreconcilable differences between the in situ (geochemical method) and in vitro (oxygen light–dark bottle method) approaches usually observed at large spatial (103 km) and temporal (103 days) scales are also applicable to mesoscale structures (102 km and 102 days).

I have only one major concern with the manuscript: an individual assessment of the uncertainty of the estimation of every physical process involved in the computation of in situ oxygen changes has not been properly preformed. My feeling is that a correct evaluation of all those uncertainties would lead to an error of the estimates large enough to produce in situ NCP rates that are not significantly different from the in vitro estimate.

On another matter, given that the authors have w and k values provided by the 3D model of Ledwell et al. (2008): i) why they do not include the oxygen model into the more realistic 3D model instead of creating a new 1D model?; ii) what would be the resulting in situ NCP if the w and k values directly provided by the 3D model were introduced rather than tuned in the 1 D model?

SPECIFIC COMMENTS

Section 2.1. The authors state that samples were collected for the determination of dissolved inorganic nitrogen, but only nitrate is presented. What about the distributions of ammonium and nitrite? The presence of significant concentrations of both reduced nitrogen forms, which is not unusual in isolated water parcels, could justify an extra oxygen consumption in the bottles due to the oxidation of ammonium and nitrite to nitrate by the marine nitrifiers enclosed in the oxygen flasks.

In the same section, could you please describe in more detail the deck incubator, oxygen bottle volumes, number of replicates, conditioning of the incubated water (was it prefiltered?), etc..

Section 2.2.1. the temperature model is incomplete. Why you have not considered the heat loss by evaporation?

Section 3.1. I cannot see in Figure 3 the value of 1.6 $\pm$ 0.1 mmol O2 m–3 d-1 reported in the text.

In the same section, if no changes are observed in primary production measured with the 14C incorporation technique from the beginning to the end of the sampling period it

BGD

6, C477–C479, 2009

would mean that the observed O2 changes can only be justified by a temporal evolution of the O2/C stoichiometric ratio, which can vary from 1.0 to 1.6. In that case, what is the value of estimating NCP from in situ or in vitro oxygen measurements? Please discuss this crucial point in more detail.

Section 3.2. It does not make any sense to introduce the last sentence of this section.

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