Replay to anonymous Referee #1

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 though 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. 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.

Comparison of in vitro oxygen-derived net community production (NCP) and primary production rates estimated from the ^{14}C incorporation technique (PP) is not straightforward. NCP accounts for the difference of gross primary production minus community respiration. Compilation of measurements carried out during the Joint Global Ocean Flux Study (JGOFS) indicated that ^{14}C uptake measures net primary production (gross primary production – autotrophic respiration) in dawn-dusk incubations (Marra, 2002). Studies comparing in vitro O_2 fluxes with ^{14}C production rates during JGOFS cruises determined that ^{14}C productivities incubated during 24 hours were about 45% of gross carbon production rates calculated from gross O_2 production (Bender et al., 1999; Laws et al., 2000). In contrast to the 24-h incubations used in the Bender and Laws studies, ^{14}C incubations carried out during the EDDIES cruises are dawn-to-dusk. Therefore EDDIES ^{14}C rates are likely to be closer to gross primary production than the factor of two indicated by Bender and Laws. However, it is not possible to quantify this relationship in these cruises because ^{14}C and ^{18}O methods have not been compared in this region. PP measured at C1 cyclone center during WB0409 (24 Jun – 2 Jul 2004) and WB0413 (2 Aug – 11 Aug 2004) cruises were already reported in Ewart et al (2008) (See table 1 and figure 6). We have updated this reference but we do not consider necessary to include a new figure showing PP. It would detract from the main point of the paper (a comparison of in vitro versus in situ oxygen NCP estimates) to get involved in the complex discussion of how either compare with ^{14}C PP, which this paper cannot resolve, as the ^{14}C PP measurements were very few in number (2 during WB0409 and 1 on WB0413) and not co-located with the in vitro oxygen NCP measurements.

"No significant changes in net primary production rates estimated from the ^{14}C incorporation technique (Johnson et al. 2006), bacterial biomass or bacterial production (Ewart et al. 2008) were observed from the beginning to the end of the sampling period. Particle export fluxes calculated from ^{234}Th method showed a decrease in the magnitude of carbon fluxes (Buesseler et al. 2008)."

Modified to:

"Primary production rates estimated from the ^{14}C incorporation technique (^{14}C PP) did not change significantly from the beginning to the end of the sampling period (Ewart et al. 2008). No important changes in bacterial biomass or bacterial production were observed between cruises (Ewart et al. 2008). Particle export fluxes calculated from ^{234}Th method showed a decrease in the magnitude of carbon fluxes (Buesseler et al. 2008).
Note that comparison of the in vitro NCP and the ^{14}C PP measurements is not straightforward. NCP estimated gross primary production minus community respiration over 24 hours, whereas dawn-to-dusk ^{14}C PP measures gross primary production minus autotrophic respiration (Marra,

2002). Since the two types of measurements available in this study are few and they were not made at the same times or locations, a precise intercomparison of both techniques can not be resolved with this dataset."

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.

The abstract has been modified to:

"It has been proposed that the disagreement traditionally reported between in vitro incubation and in situ estimates of oxygen net community production (NCP) could be explained, at least partially, by undersampling episodic pulses of net autotrophy associated with mesoscale dynamics. In this study we compare in vitro incubation estimates of net community production with in situ estimates, derived from oxygen profiles and a 1-D model, within a cyclonic eddy investigated in the Sargasso Sea in summer 2004. The in vitro NCP rates measured at the center of the eddy showed a shift from net autotrophy (7 ± 3 mmol O_2 m^{-2} d^{-1}) to net heterotrophy (-25 ± 5 mmol O_2 m^{-2} d^{-1}) from late June to early August. The model-derived NCP rates also showed a temporal decline (17 ± 6 to -4 ± 7 and 9 ± 8 mmol O_2 m^{-2} d^{-1}), but they were systematically higher than the in vitro estimates and reported net autotrophy or balance for the sampling period. In this comparison episodic pulses in photosynthesis or respiration driven by mesoscale eddies can not explain the discrepancy between the in vitro and in situ estimates of NCP. This points to methodological artefacts or temporal or submesoscale variability as the mechanisms responsible for the disagreement between the techniques, at least in this dataset."

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.

The only way that eddies can cause a difference in the two techniques is by undersampling. The in vitro technique examines samples on small space and time scales while the in situ technique intregrates over long time- (and consequently space-) scales. Therefore if mesoscale eddies cause transient spikes in NCP, the in vitro technique will underestimate NCP if it undersamples these events. This hypothesis was already presented in the second paragraph of the introduction:

"The two approaches differ in their space and time scales, and there are obvious problems in relating measurements made over a few hours in small volumes of water with the geochemical approach, which integrates over larger temporal and spatial scales. It has been proposed that the measured net heterotrophy could be an artifact of undersampling increases in oxygen concentration associated with episodic pulses of net community production (Karl et al. 2003). One of the proposed mechanisms that could generate pulses in the balance between photosynthesis and respiration is associated with mesoscale and submesoscale processes (Maixandeau et al. 2005, Nicholson et al. 2008, Mourino-Carballido 2009)."

Also in the last paragraph of Section 1 we have changed "if mesoscale eddies could explain" to "if undersampling of enhanced NCP within mesoscale eddies could explain" to better clarify the hypothesized cause of the difference.

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 O2 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.

Given that we have more strongly emphasized the central point of the paper, as the Referee suggested above, we feel the model details are now less distracting. Whether to put the discussion of K (pages 3242-3) and salinity (page 3243) in an appendix is simply a matter of organization. Since they are only one paragraph each, and are vital considerations for physical model formulation, that must always be examined, we feel it is best to leave them at the end of Methods section 2.2.1. The parameterization of gas exchange (p 3244) is not reproduced in full in one of the other references, which is why we include it here. We have eliminated the speculation at the end of Sec. 3.2, as suggested.

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 nitrate and chlorophyll data are useful to illustrate the temporal evolution of hydrographic properties at eddy center, and to compare with the depth- and time-dependent *in situ* and *in vitro* NCP estimates in Figs. 2 and 3.

These data are described in the first paragraph of Section 3.1:

"The nitracline and deep chlorophyll maximum were located at about 100 m. Nitrate and chlorophyll appeared to deepen from 21-22 June to 27-28 July and then shoal from 27-28 July to 3-4 August, but because of the coarse vertical resolution of the nitrate and chlorophyll data, and the observed small-scale horizontal or temporal variability in nitrate and chlorophyll data on 27-28 July (Figure 1), we are not completely confident in their apparent temporal evolution. High spatial resolution data from the Video Plankton Recorder towed across C1 showed the chlorophyll distribution to be patchy in the vicinity of eddy center (McGillicuddy et al. 2007)"

And the last paragraph of Section 3.1:

"Although we can not discard the possibility that part of the changes observed in C1 were associated with imperfect sampling of submesoscale or day-to-day variability, most evidence suggests that during the sampling period the eddy was in a state of declining biological response. This is consistent with the reduction in NCP measured in the photic layer of C1 by the in vitro technique."

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.

Under the Redfield approximation, in which autotrophic and net heterotrophic processes use the same stoichiometric equation, recycled production causes no net change in oxygen, DIN or DIC. So oxygen-derived NCP equals new production plus any non-Redfieldian imbalance. We now clarify this in Section 3.3 (previously p 3248):

"Periods 1 (21 Jun – 1 Jul) and 3 (28 Jul – 4 Aug) estimate some net oxygen production near the nitracline (ca. 100 m), related to nitrate-based new production, while Period 2 (1 Jul – 28 Jul) does not. Yet in all periods and cases the NCP occurs primarily far above the nitracline, indicating either new production by nitrogen fixation or atmospheric deposition, or autotrophic

and heterotrophic processes using different stoichiometric ratios. Consequently Redfield oxygen-to-nitrogen conversions in these NCP estimates may not be appropriate. These results are in agreement with observations of nitrate uptake and carbon fixation across the photic layer of the subtropics (Painter et al. 2007)".

Some additional details are warranted in this paper, if explained briefly. How were eddy center characteristics chosen (line 25 page 3240)?

This is now mentioned at the very beginning of Section 2.2:

"The location of eddy center as a function of time was estimated from a combination of Sea level Anomaly (SLA), expendable bathythermograph (XBT) and Acoustic Doppler Current Profiler (ADCP) data (McGillicuddy et al. 2007). Of the CTD stations within 20 km of this estimated location, stations were chosen as representing eddy center based on their temperature and salinity (T-S) properties and the vertical displacement of their main and seasonal thermoclines (Table 1, Figure 1). Data profiles within four different 48-hour periods were horizontally averaged into mean profiles. From these, the time-mean depth-dependent NCP rates were estimated within the three intervening time intervals as follows.."

What is the effect on the model of using a constant solar flux and how consistent was this flux between different time periods?

True, the solar flux was not constant. Shipboard estimates are 315 and 227 W m^{-2} for Periods 1 and 3, while NCEP NARR-A atmospheric model estimates are 304, 222 and 224 W m^{-2} for the 3 periods. The impact of this variability on the model NCP estimates is small however. For example, if 224 W m^{-2} is used instead of 274 in Period 3 Case 1, K_{deep} (optimized to 2 digits) is still 3.2 x 10^{-5}, causing no change in NCP. In Case 3 the optimal w_{max} becomes -0.56 m/day, increasing the NCP estimate from 8.1 to 8.4 mmol O_2/m^2/day, which is negligible relative to other sources of error. This is now mentioned in Section 3.4.

Was the non-solar hear flux computed by the model reasonable compared to the meteorological measurements or weather models?

This is now mentioned in the last paragraph of Section 3.3:

"It is felt that Cases 2 and 3 are closest to the truth. The temperature model computed the 43-day non-solar surface heat flux as 39, -150 and -178 W m^{-2} for Cases 1, 2 and 3 respectively, which the NCEP NARR-A atmospheric model estimated independently as -182 W m^{-2}. Thus the heat balance indicates Case 1 as unlikely, but Cases 2 and 3 as plausible."

When several profiles within a short time period were available, how were these treated in the model (averaged together, considered separately for an estimate of error, etc)?

The data profiles were horizontally averaged together. This is now mentioned at the beginning of Section 2.2. To estimate the error due to the uncertainty in these mean profiles, individual profiles were excluded from the mean. This is mentioned in Section 3.4.

Why not use the modelled surface temperature rather than linearly interpolating in time to obtain O2 solubility estimates?

Primarily because we did not want the oxygen model results to be dependent on the temperature model results, aside from the calibrated values of κ_{deep}, w_{max} and w_{grad}. Also, the modelled surface temperature is nearly linear. Departures from linearity cause differences in O_2 solubility smaller than the O_2 calibration error.

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.

Nitrate and chlorophyll profiles in Figure 1 are useful to show consistency in the temporal evolution measured in different variables at eddy center from the beginning to the end of the sampling period (see below). Both figures have been modified to make them bigger. Note, however, that the layout format of Biogeosciences is different from that of Biogeosciences Discussion, such that these figures will appear much larger in BG than they do in BGD.