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Interactive comment on "Norwegian Sea net community production estimated from O_2 and prototype CO_2 optode measurements on a Seaglider" *by* Luca Possenti et al.

Anonymous Referee #1

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Summary

The authors present oxygen, CO2, and chlorophyll fluorescence measurements obtain from a glider occupation of a transect in the Norwegian Sea. The observations are utilized to compute net community production (NCP) along the transect via temporal budgets of oxygen and DIC (calculated from CO2 measurements and an algorithmic estimate of total alkalinity). The data are novel as they may be the first collected from a CO2 optode deployed on a glider. As such, the results of this study have important implications for enhancing biogeochemical observations using autonomous measurements and, therefore, long-term observation capabilities of biological production and

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ocean acidification. Thus, this study provides a potentially important contribution to the scientific literature. Given the importance of this study, the methods of sensor calibration and NCP calculation should be carefully scrutinized. There are some questions and concerns regarding the calibration and correction of the O2 and CO2 data. There are also a number of major concerns regarding the calculation of net community production that the authors must address and clarify in order to minimize doubt and error; chief among these concerns in an apparent lack of attempt to separate spatial and temporal variability in the O2 and CO2 measurements.

General comments

1) Parameterization for deriving phosphate and silicate concentrations along the glider track from 'spot' samples collected during four cruises over the deployment (March, May, June, and October). Sampling restricted to the southern half of the transect. And yet, the uncertainties were only 1.3 and 0.13 umol kg-1 for silicate and phosphate? I hope this parameterization is discussed in detail (in the text or an appendix). I also hope that some sensitivity analysis was completed regarding the impact of differing nutrient concentrations (within a reasonable range for the region & study period) on CO2SYS calculations. I'm also concerned about the use of chloroform to preserve nutrient samples.

2) For the lag correction to the CO2 optode, data from the glider ascents are compared against those from descents. However, there is significant horizontal distance between a glider ascent and descent, unlike what one might expect for a CTD cast from a ship. By minimizing the differences observed between glider ascents & descents, you are loosing information and I'm not sure the lag correction is necessarily reliable. I would suggest comparing potential temperature and salinity in glider ascents & descents. Do they match? If so, then perhaps this method is OK. If not, the authors may need to revise the lag correction method.

3) Why is the correlation between the discrete samples and optode output CO2 partial

pressure (Figure 6) so much better when using CO_2 concentration vs. partial pressure (from the discrete samples)? The authors should at least offer some educated guesses or speculation.

4) I am concerned about the potential impact of advection on the NCP calculation. The study focuses on a SE-NW transect, in a region where waters are transported in a meridional direction along well known currents (NwAC and NCC, as shown in Fig. 1). Can the authors be certain that the time rate of change in O_2 and DIC does not reflect advection of water through the transect? What steps did the authors take to ensure that changes in O_2 and DIC were truly a function of time and not space? Differentiating temporal vs. spatial changes in measured variables from gliders is not a trivial task and prior studies have typically used repeating spatial patterns to form a 'box' in order to compute O_2 and/or carbon budgets for the estimation of NCP. In this study, the glider did not survey a box but a transect in a region of potentially meandering currents and a frontal region separating two water mass regimes. The authors need to do a better job justifying their methods and eliminating (or at least minimizing) doubt that spatial variations and/or advection contribute significantly to the observed changes in oxygen over the study period.

5) The authors indicate a separation of the NCP calculation based on water masses with a cutoff at S = 35 that distinguishes between the two primary water masses influencing the study area: Norwegian Atlantic Current (NwAC) water and the Norwegian Coastal Current (NCC) water. It is also stated that salinities between 32 and 34 were encountered in the top 50 m, signifying influence of NCC water. I'm curious whether the authors took mixing into account between the two water masses in the region where NCC was encountered. How might this impact the NCP calculations? Also, I would have appreciated more information regarding the separation. Was NCP calculated separately for each of the two regions? Were they then averaged together to present a single NCP number for O_2 and DIC?

6) Integration of oxygen & DIC over a specific depth range for the calculation of NCP

may be subject to vertical heaving of isopycnals. What steps did the authors take to ensure that such vertical displacement did not impact the calculations? What about vertical mixing from the bottom up? The authors calculate an entrainment flux that focuses on periods when the mixed layer depth exceeded the limit of integration (45 m), but do not discuss the possibility of mixing across the bottom boundary. Admittedly, this is probably minimal, unless there were periods of isopycnal heaving (which looks probable, from the temperature distribution shown in Fig. 8), but the possibility should have been investigated and (at least briefly) mentioned in the manuscript.

Specific comments

1) Please clarify units of N(CT) and N(O2). Are they both expressed as mmol C m-2 d-1 or do they differ (e.g., mmol C m-2 d-1 vs. mmol O2 m-2 d-1)? After getting to section 3.6, it's clear they were reported in different units, but readers shouldn't have to wait that long to be sure.

2) Preservation of nutrient samples with chloroform is not a recommended procedure. . .

3) Figure 2 indeed shows that, on average, the oxygen concentration at higher latitudes was greater (by 10-15 umol kg-1) than those measured at lower latitudes. However, the oxygen concentration decreases fairly linearly with time in both regions (lower and higher latitudes). Why is this the case? I wouldn't think it was short-term drift as such drift should be minimal in oxygen optodes. Does this results, perhaps, from a longitudinal gradient in oxygen concentrations?

Figure 3 shows a similar 'drift', or time rate of change, in the gain factor computed to correct the optode oxygen. I am surprised there is such an apparent, continued drift in the optode sensor response. I would have expected a large, initial drift ('storage' drift') but then would have thought the optode response to be relatively stable over a deployment period of $\sim$8 months. Can the authors show the individual, median oxygen concentrations and standard deviations from the discrete data? I'm curious how stable the oxygen concentrations are in this density/depth range (∼427 to 1000 m).

4) Line 269: "The thermal lag of the glider conductivity sensor was corrected for..." What?

5) Can the authors please define cN(Chl a)? Is this the computed chlorophyll concentration, using factory-defined coefficients?

6) Line 363: "...because after this dive, the CO2 optode stopped sampling..."

7) Line 364: "...raw c(O2) data was calibrated and drift-corrected and c(CO2) was drift- and lag-corrected and recalibrated, then used to..."

I'm not going to focus my review on grammar corrections, so I suggest the authors carefully re-read the manuscript to avoid any additional grammar or spelling mistakes that should be addressed prior to publication.

8) Plot isopycnals on panels of Fig. 8. I'd also recommend plotting the mixed layer depth and highlighting zlim (dotted line?).

9) Line 375: What is "against year-day"? Please re-word this sentence.

10) Lines 456-457: Can the authors please expand on how NCP was calculated? It is stated that, "The two Ns were calculated as the difference in inventory changes between two transects when the glider was in the same water mass." Two transects? So, is one transect equivalent to the glider moving over the entire transect in one direction and the second transect is the glider moving back over the transect in the opposite direction?

Is the NCP calculated only for the NwAC water mass? So any changes within the NCC water mass are removed from the analysis?

11) It is important to compare NCP estimates with those of previous studies; however, it is difficult to know how comparable the numbers are in Table 3 because it is not clear where in the Norwegian Sea these various studies took place. It is also difficult because

zlim varies largely among the studies. The fact that three of the four compared studies used zlim >= 100 m also calls into question why exactly the current study decided on zlim = 45 m, particularly since the mixed layer depth varied so largely and often exceeded zlim.

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