

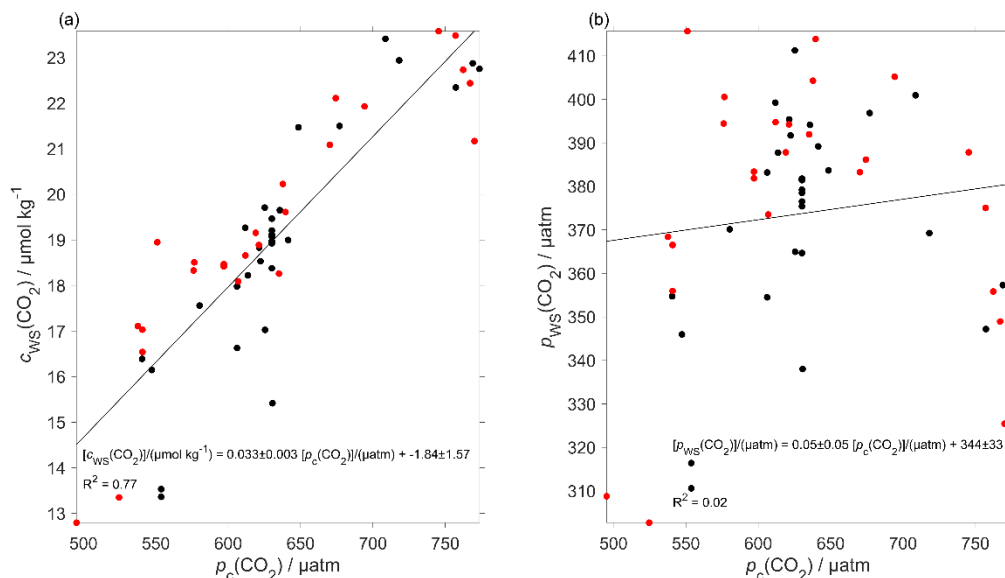
## Response to Reviewer 1

We would like to thank Reviewer 1 for providing constructive and insightful comments. We will incorporate their suggestions into a revised manuscript. Reviewer 1's comments have been reproduced below in black, with the authors' response in blue.

### General comments

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  $\mu\text{mol kg}^{-1}$  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.

The reviewer's comment highlighted that our method description has been too brief on nutrient analysis. In fact, we collected 58 discrete samples along the glider transect in March, May, June and October, which were analysed for total alkalinity ( $A_T$ ), dissolved inorganic carbon ( $C_T$ ) as well as nutrient concentrations. In addition, we used 52  $C_T$  and  $A_T$  discrete samples collected at a weather station (OWSM) located 270 km northeast of the glider transect (Figure 1 of the discussion paper) in March, May, June, August and November. For the latter samples, no nutrient measurements were undertaken. Instead, we filled in these gaps with the nutrient concentration nearest in time and depth from the 58 along-transect samples, which we expected to be sufficiently close to the actual concentration at OWSM. We then derived the  $\text{CO}_2$  concentration ( $c(\text{CO}_2)$ ) using the MATLAB toolbox CO2SYS (Van Heuven *et al.*, 2011), using the phosphate and silicate concentrations to account for their contributions to  $A_T$ . This  $c(\text{CO}_2)$  was used as reference to calibrate the glider output (Figure 1, in red the samples collected along the transect and in black at OWSM). The nutrient concentrations were not used in any further calculations.



**Figure 1:** Calibration of the  $\text{CO}_2$  optode using the samples collected along the glider transect (red) and at OWSM (black) a)  $\text{CO}_2$  concentration of the discrete samples ( $c_{ws}(\text{CO}_2)$ ) against the glider output with the linear regression line and b)  $\text{CO}_2$  partial pressure of the discrete samples ( $p_{ws}(\text{CO}_2)$ ) against the glider output with the linear regression line.

To assess the uncertainty of the final silicate and phosphate concentration we calculated the uncertainty in the calculation of the OWSM  $c(\text{CO}_2)$  using the interpolation uncertainty 1.1 and 0.12  $\mu\text{mol kg}^{-1}$  for the silicate and phosphate concentration, respectively. The uncertainty was calculated as

the root mean square error of the interpolation, which estimates the standard deviation of the error distribution. The interpolation was the following for phosphate:

$$c(\text{PO}_4) = 0.0003 \times z/\text{m} + -0.0003 \times t + 220.18 \quad (1)$$

where  $z$  is the depth and  $t$  the date. In the case of silicate the equation was:

$$c(\text{Si}) = 0.0054 \times z/\text{m} + -0.0049 \times t + 3626.3. \quad (2)$$

The derived nutrient concentration led to a mean error of  $0.04 \mu\text{mol kg}^{-1}$  in the calculation of  $c(\text{CO}_2)$ .

We followed the procedure described by Hagebo and Rey (1984) to collect and preserve the nutrients. We are not aware of any problems with the use of chloroform to preserve nutrient samples. The 2019 GO-SHIP Repeat Hydrography Nutrient Manual only discourages the use of acid (which would require neutralisation before analysis) and mercuric chloride (a long-term environmental hazard) (Becker *et al.*, 2020).

For the lag correction to the  $\text{CO}_2$  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 losing 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.

The  $\text{CO}_2$  optode lag was corrected using the algorithm of Miloshevich, (2004) that uses the sensor response time ( $\tau$ ). The  $\tau$  was calculated minimising the difference between each glider ascent and descent (see Figure 5 of the discussion paper). In the equation, we used the median of all the  $\tau$  values (1384 s). The correction decreased the difference between the glider and descent of the raw  $p(\text{CO}_2)$  from  $(71 \pm 30)$  to  $(21 \pm 26) \mu\text{atm}$ . Using the median  $\tau$  is a robust indicator for the lag time, even there were short-term variations (e.g. due to internal tides or waves) between the descent and ascent.

To assess if the glider was in the same water mass between ascent and descent we looked at potential temperature and salinity. The mean difference between the descent and ascent was  $(0.13 \pm 0.33) ^\circ\text{C}$  for potential temperature and  $0.02 \pm 0.04$  for salinity. These small mean differences for potential temperature and salinity show that the method is sufficiently robust.

Why is the correlation between the discrete samples and optode output  $\text{CO}_2$  partial pressure (Figure 6) so much better when using  $\text{CO}_2$  concentration vs. partial pressure (from the discrete samples)? The authors should at least offer some educated guesses or speculation.

The better correlation with  $c(\text{CO}_2)$  was probably related due to an inadequate temperature-parameterisation of the sensor calibration function. The sensor output depends on the changes in pH that are directly related to the changes of  $c(\text{CO}_2)$  in the membrane and – indirectly –  $p(\text{CO}_2)$ , via Henry's Law. Sensor and external water  $p(\text{CO}_2)$  should be in equilibrium. The calibration is supposed to correct for the temperature-dependence of the sensor output (Atamanchuk *et al.*, 2014). The observation that the sensor output correlated better with  $c(\text{CO}_2)$  than  $p(\text{CO}_2)$  is perhaps due to a fortuitous cancellation of an inadequate temperature-parameterisation and the temperature-dependence of the Henry's Law relationship between  $c(\text{CO}_2)$  than  $p(\text{CO}_2)$ .

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  $\text{O}_2$  and DIC does not reflect advection of water through the transect? What steps did the authors take to ensure that changes in  $\text{O}_2$  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  $\text{O}_2$  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.

We have assumed that the main processes controlling the surface dissolved inorganic carbon and oxygen concentrations are biological production and respiration as well as air-sea gas exchange and vertical transport. Even though there are well-known currents, horizontal gradients are reduced due to constant stirring from winds and tides and therefore net advective fluxes are likely to be small (Gislefoss *et al.*, 1998; Falck and Gade, 1999). Previous estimates of net community production in the Norwegian Sea have also neglected advective fluxes (Falck and Gade, 1999; Skjelvan, Falck, Leif G. Anderson, *et al.*, 2001; Falck and Anderson, 2005; Kivimäe, 2007). For example, Gislefoss *et al.*, (1998) considered minimal the effect of horizontal advection on  $N(C_T)$  during the summer because  $C_T$  changes were largely controlled by biology and air-sea interactions. For NCP estimates on shorter timescales (days to a few weeks), advective fluxes and water-mass movement would have to be taken into account, but this would require a different survey design, involving multiple platforms (Alkire *et al.*, 2014), beyond the scope of the present study.

However, the query from the reviewer prompted us to revisit our NCP calculation, which showed that at the glider turn-around points, inventory changes were calculated over relatively short time-scales of a few days. Therefore, to minimise the effect of horizontal advection in the new version of the manuscript we will extend the time interval used to calculate the inventory changes from less than a week to an average of 50 days. This was achieved by calculating the concentration difference between two transects when the glider moved in the same direction (e.g. transects 1-3, 2-4 and 3-5 all in N-S direction) instead of two consecutive transects.

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<sub>2</sub> and DIC?

The daily value of the net community production ( $N$ ) was calculated separately for NCC and NwAC. The annual  $N$  was then calculated combining the two water masses to be consistent with the previous studies (Falck and Gade, 1999; Skjelvan, Falck, Leif G Anderson, *et al.*, 2001; Falck and Anderson, 2005; Kivimäe, 2007).

Here again, the reviewer's comment inspired a revision of our calculation method and for the new version of the paper, we will calculate daily and annual  $N$  without separating NCC from NwAC. We change the methodology to be consistent with previous studies, to extend and homogenise the time difference used to calculate inventory and entrainment and to minimise the impact of horizontal advection.

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.

We thank the reviewer for the interesting suggestion. Vertical heaving has an effect entraining the water from below the integration. We do already consider the effect of entrainment in the calculation of the net community production, in the form of terms  $E(O_2)$  (Eq. 10) and  $E(C_T)$  (Eq. 16).

In response to the reviewer's comment, we have also estimated the diapycnal mixing flux and will incorporate this into the new version of the manuscript.

The diapycnal mixing flux ( $F_v$ ) was calculated from the vertical oxygen concentration gradient. In the calculation, we used a vertical eddy diffusivity ( $K_z$ ) of  $10^{-5} \text{ m s}^{-2}$  derived for the Nordic Seas by Naveira Garabato *et al.* (2004). The effect of  $F_v$  for  $\text{O}_2$  was calculated at  $z_{\text{mix}}$  when it was deeper than the integration depth  $z_{\text{lim}}$  and at  $z_{\text{lim}}$  when  $z_{\text{mix}}$  was shallower than  $z_{\text{lim}}$ , using the following equation:

$$F_v(\text{O}_2) = -K_z \frac{\partial c(\text{O}_2)}{\partial z} \quad (3)$$

The net community production ( $N$ ) incorporating  $F_v(\text{O}_2)$  (scaled in the same way as the flux at the air-sea boundary) would be:

$$N(\text{O}_2) = \frac{\Delta I(\text{O}_2)}{\Delta t} + \phi(\text{O}_2) \frac{\min(z_{\text{lim}}, z_{\text{mix}})}{z_{\text{mix}}} - E(\text{O}_2) + F_v(\text{O}_2) \frac{\min(z_{\text{lim}}, z_{\text{mix}})}{z_{\text{mix}}} \quad (4)$$

A positive sign of  $F_v(\text{O}_2)$  means a decrease of the oxygen concentration in the layer of interest between surface and  $z_{\text{lim}}$ ; a negative sign corresponds to an increase.

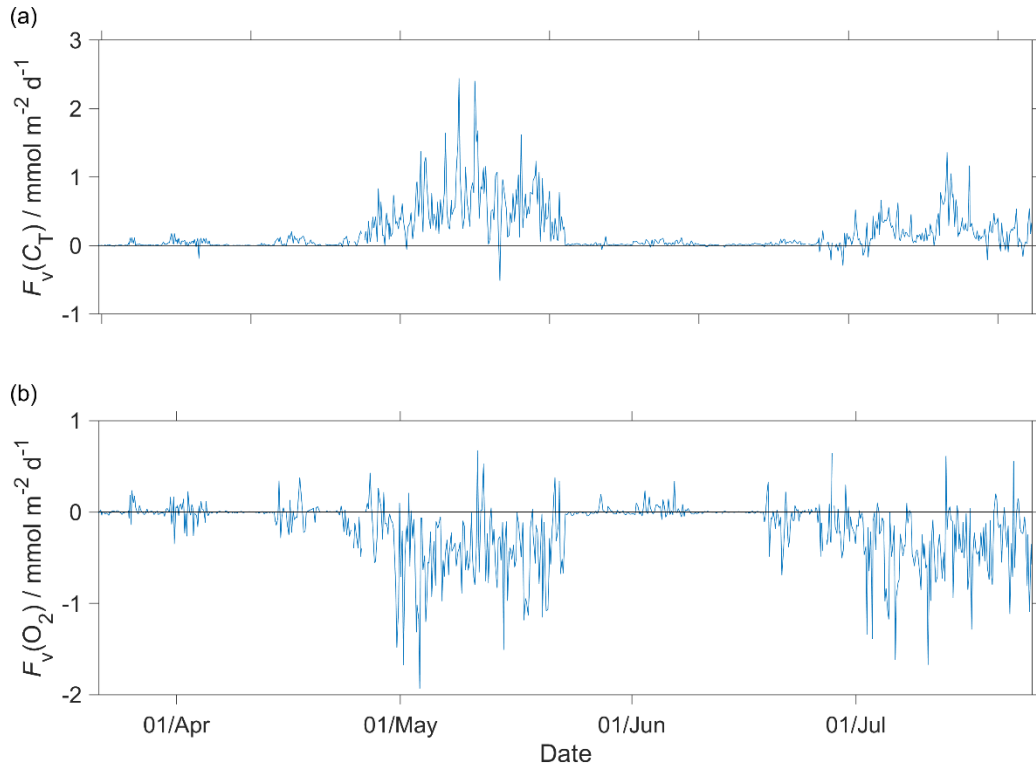
In the case of  $C_T$ ,  $F_v$  was calculated using the equivalent equations:

$$F_v(C_T) = -K_z \frac{\partial c(C_T)}{\partial z} \quad (5)$$

$$N(C_T) = -\frac{\Delta I(C_T)}{\Delta t} - \phi(\text{CO}_2) \frac{\min(z_{\text{lim}}, z_{\text{mix}})}{z_{\text{mix}}} + E(C_T) - F_v(C_T) \frac{\min(z_{\text{lim}}, z_{\text{mix}})}{z_{\text{mix}}} \quad (6)$$

A positive sign of  $F_v(C_T)$  means a decrease of the dissolved inorganic concentration in the layer of interest between surface and  $z_{\text{lim}}$ ; a negative sign corresponds to an increase.

In the new version of the manuscript, we will add a new Figure 2 that shows  $F_v$  as a function of time during the glider deployment.



**Figure 2:** Diapycnal mixing ( $F_v$ ) calculated for the glider descent and ascent for a)  $C_T$  and b)  $O_2$  at the mixed layer depth ( $z_{mix}$ ) when deeper than 45 m ( $z_{lim}$ ) and at  $z_{lim}$  when  $z_{mix}$  was shallower than 45 m. In the calculations, we used a vertical eddy diffusivity ( $K_z$ ) of  $10^{-5} \text{ m s}^{-2}$  (Naveira Garabato *et al.*, 2004).

The results show that  $F_v$  is negligibly small:  $F_v(C_T) = (0.05 \pm 0.3) \text{ mmol m}^{-2} \text{ d}^{-1}$  and  $(-0.02 \pm 0.33) \text{ mmol m}^{-2} \text{ d}^{-1}$  for  $O_2$ . For that reason, diapycnal mixing will not be used to calculate  $N$ .

### Specific comments

Please clarify units of  $N(C_T)$  and  $N(O_2)$ . Are they both expressed as  $\text{mmol C m}^{-2} \text{ d}^{-1}$  or do they differ (e.g.,  $\text{mmol C m}^{-2} \text{ d}^{-1}$  vs.  $\text{mmol O}_2 \text{ m}^{-2} \text{ d}^{-1}$ )? 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.

Both  $N(O_2)$  and  $N(C_T)$  are expressed in  $\text{mmol m}^{-2} \text{ d}^{-1}$ . In the case of  $N(O_2)$ , this is a flux of  $O_2$ , in the case of  $N(C_T)$ , a flux of inorganic carbon.

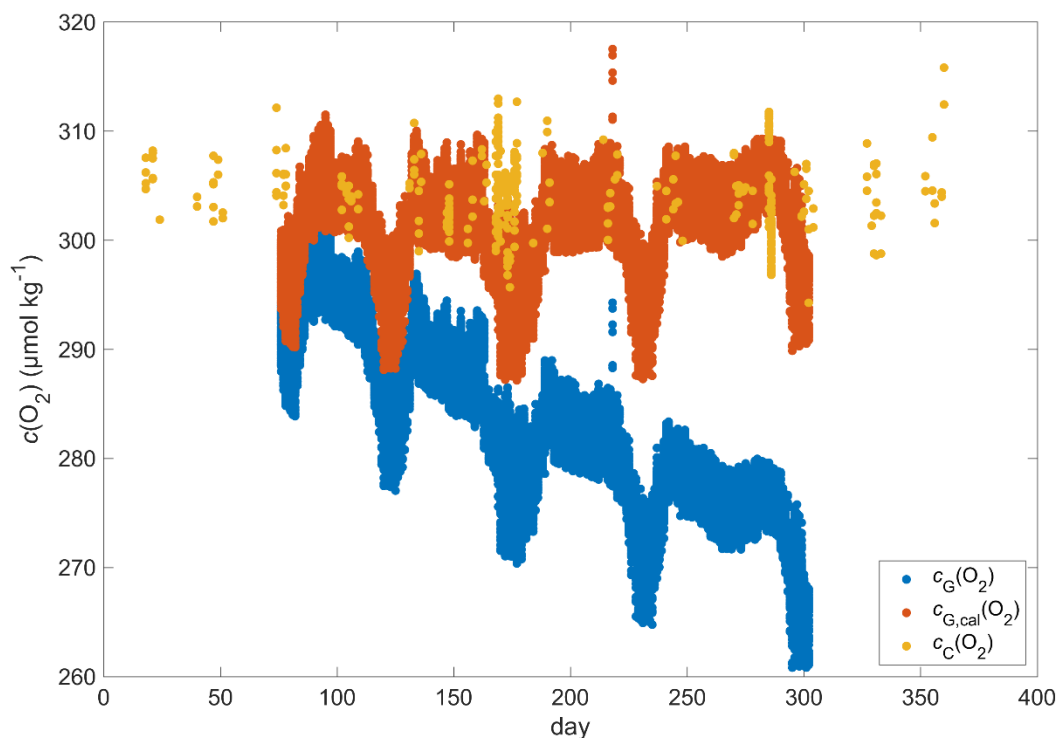
Preservation of nutrient samples with chloroform is not a recommended procedure. . .

See our answer on page 1.

Figure 2 indeed shows that, on average, the oxygen concentration at higher latitudes was greater (by 10-15  $\mu\text{mol kg}^{-1}$ ) 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 ( $\sim 427$  to  $1000 \text{ m}$ ).

The oxygen concentrations for  $\sigma_0 > 1028 \text{ kg m}^{-3}$  decreased linearly in both regions because the oxygen optode drifted continuously during the deployment (Figure 2 of the paper and Figure 3 where in red is the uncorrected oxygen, in blue the corrected oxygen and in yellow the discrete sample used as reference). In the new version of the manuscript, we will add a figure with all the samples collected and the glider data before and after the correction showing how the corrected glider oxygen is within the variability of the discrete samples and how stable the  $O_2$  concentration is in this depth range. It was possible to use waters of these potential densities because were always well below the mixed layer depth and therefore subject to limited seasonal and interannual variability. The salinity of the discrete samples varied from 34.88 to 34.96, with a mean of  $(34.90 \pm 0.01)$  and the temperature varied from 0.45 to  $-0.76 \text{ }^\circ\text{C}$  with a mean of  $(-0.15 \pm 0.36) \text{ }^\circ\text{C}$ . Variations are due to differences in deep-water masses. Therefore, we only used the glider and discrete samples collected at latitudes north of  $64^\circ \text{ N}$  because this reflects the largest part of the transect. Also, the region south of  $64^\circ \text{ N}$  contained just 5 days of archived samples. See also reply 2 to Reviewer 2.

We added in the appendices a plot with all the discrete samples and the glider oxygen before and after the correction:



**Figure 3:** a) Discrete samples  $c_C(O_2)$  (yellow), raw glider oxygen  $c_G(O_2)$  (blue) and drift corrected glider oxygen  $c_{G,cal}(O_2)$  (red) using water density  $> 1028 \text{ kg m}^{-3}$ .

Line 269: “The thermal lag of the glider conductivity sensor was corrected for. . .” What?

The correct phrase should be “The thermal lag of the glider conductivity sensor was corrected using the method of Gourcuff (2014).”

Can the authors please define  $cN(\text{Chl } a)$ ? Is this the computed chlorophyll concentration, using factory-defined coefficients?

Yes, in the conversion from the raw chlorophyll to the chlorophyll concentration, we used the factory-based coefficients.

Line 363: “. . .because after this dive, the  $\text{CO}_2$  optode stopped sampling. . .”

We meant “For the subsequent dives, the  $\text{CO}_2$  optode stopped sampling in the first 150 m (Figure 2.8d).”

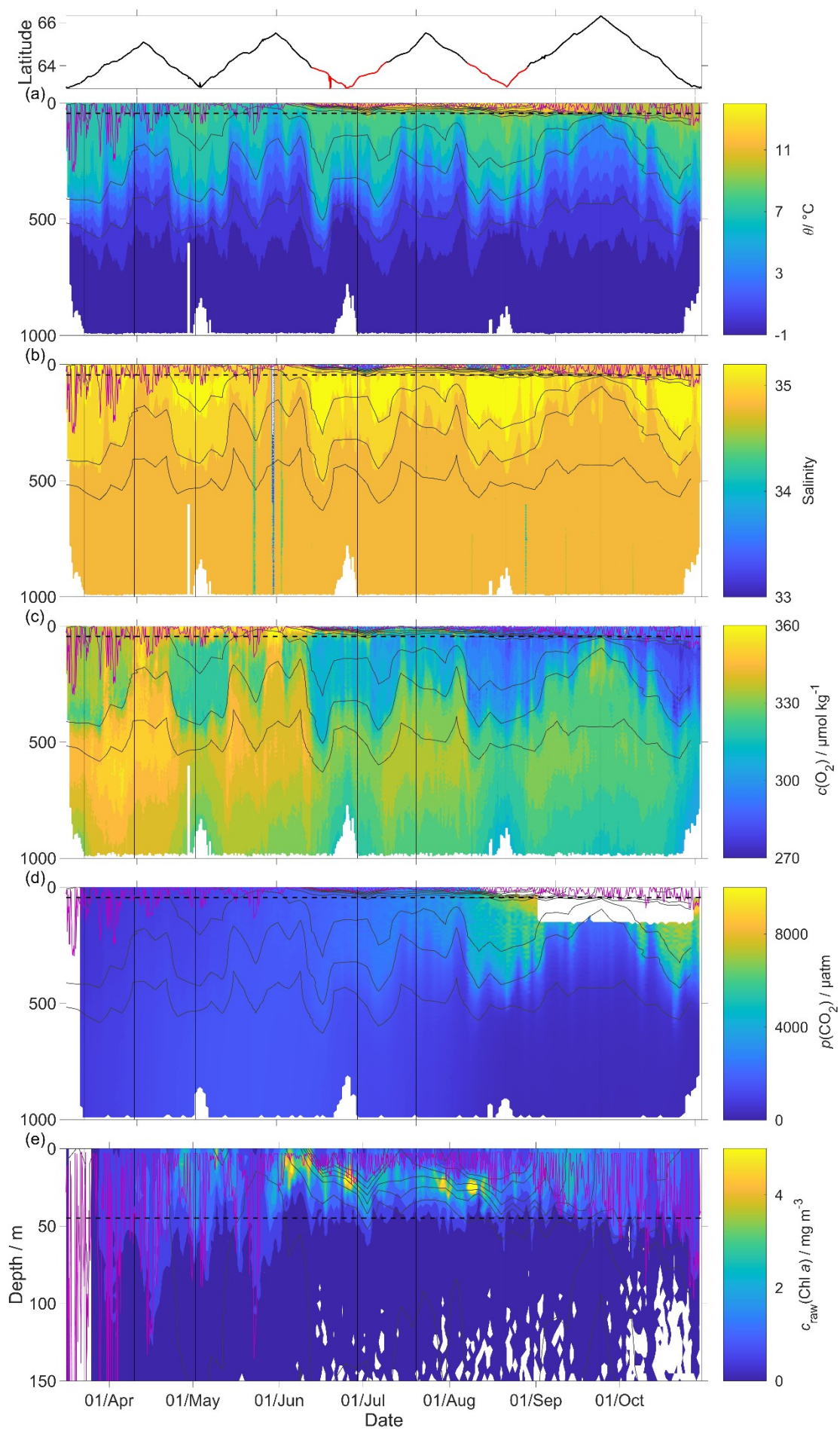
Line 364: “. . .raw  $c(O_2)$  data was calibrated and drift-corrected and  $c(\text{CO}_2)$  was drift and lag-corrected and recalibrated, then used to. . .” I’m not going to focus my review on grammar corrections, so I suggest the authors carefully re-read the manuscript to avoid any additional grammar or spelling mistakes that should be addressed prior to publication.

Apologies if the sentence structure was unclear. We meant to say that “The raw  $c(O_2)$  data were drift-corrected and calibrated. The  $\text{CO}_2$  output was drift and lag-corrected and then calibrated against  $c_C(\text{CO}_2)$  from nearby discrete samples. The calibrated glider  $c_G(O_2)$  and  $c_G(\text{CO}_2)$  were used to calculate inventory changes and air-sea exchange fluxes ( $\Phi$ ) to evaluate the net community production changes.”

Plot isopycnals on panels of Fig. 8. I’d also recommend plotting the mixed layer depth and highlighting  $z_{\text{lim}}$  (dotted line?).

We changed figure 8 adding the mixed layer depth,  $z_{\text{lim}}$  and the isopycnals (Figure 4).





**Figure 4:** Raw glider data for all 703 dives with latitude of the glider trajectory at the top (black: NwAC; red: NCC, separated by a  $S$  of 35). a) temperature  $\theta$ , b) salinity  $S$ , c) oxygen concentration  $c(\text{O}_2)$ , d) uncorrected  $\text{CO}_2$  optode output  $p_u(\text{CO}_2)$  and e) chlorophyll  $a$  concentration  $c_{\text{raw}}(\text{Chl } a)$ . The white space means that the sensors did not measure any data. The pink line is  $z_{\text{mix}}$  calculated using a threshold criterion of  $\Delta\theta = 0.5^\circ\text{C}$  to median  $\theta$  of the top 5 m of the glider profile (Obata et al., 1996; United States. National Environmental Satellite and Information Service, Monterey and Levitus, 1997; Foltz et al., 2003), the black dotted line  $z_{\text{lim}}$  used as depth limit to calculate the net community production ( $N$ ) and black contour lines are the isopycnals.

Line 375: What is “against year-day”? Please re-word this sentence.

Year-day means day of the year and varies from 1 to 365.

We will change all occurrences of year-day in the manuscript to "day of the year".

Lines 456-457: Can the authors please expand on how NCP was calculated? It is stated that, “The two  $N$ s 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?

Yes, it is correct, that we used one transect with the glider moving in one direction and the following transect with the glider moving in the opposite direction. To calculate the net community production ( $N$ ) the data were binned into  $0.1^\circ$  latitude intervals and the inventory changes were calculated as the difference of the integrated  $c(\text{O}_2)$  and  $C_T$  every time the glider was in the latitude bin. The air-sea flux was the instantaneous flux when the glider was in the bin and the entrainment was considered as the concentration changes when the mixed layer deepened between two transects in the same latitude bin.

The daily  $N$  was calculated separating the two water masses (NCC and NwAC) and the annual  $N$  was calculated as the mean of the daily  $N$  considering the two water masses together.

Following the prompt for the reviewer on the possible influence of horizontal advection (see above), the revised version of the manuscript will use an amended methodology to calculate net community production will. We will use the difference of  $C_T$  and  $\text{O}_2$  between two transects when the glider moved in the same direction (e.g. southeast to northwest). We will not use two consecutive transects. This means that inventory changes will be calculated based on a similar time difference between the two samples. For two consecutive transects, the time difference between the two samples would be smaller at the beginning and the end of the transect and larger in the middle.

Also, to correct for the variability of the wind speed, we will use flux-weighted gas transfer velocities for  $\text{O}_2$  and  $\text{CO}_2$  (Reuer et al., 2007), rather than instantaneous fluxes (as before).  $k_w(\text{O}_2)$  and  $k_w(\text{CO}_2)$  will be normalised using the daily wind speed in the latitude bin in the time interval used to calculate the inventory changes. The time interval is the time between two samples used to calculate the inventory changes and entrainment. The air-sea flux is based on the concentration measured at the time of the second transect used to calculate the inventory changes and the entrainment flux.

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  $z_{\text{lim}}$  varies largely among the studies. The fact that three of the four compared studies used  $z_{\text{lim}} \geq 100$  m also calls into question why exactly the current study decided on  $z_{\text{lim}} = 45$  m, particularly since the mixed layer depth varied so largely and often exceeded  $z_{\text{lim}}$ .

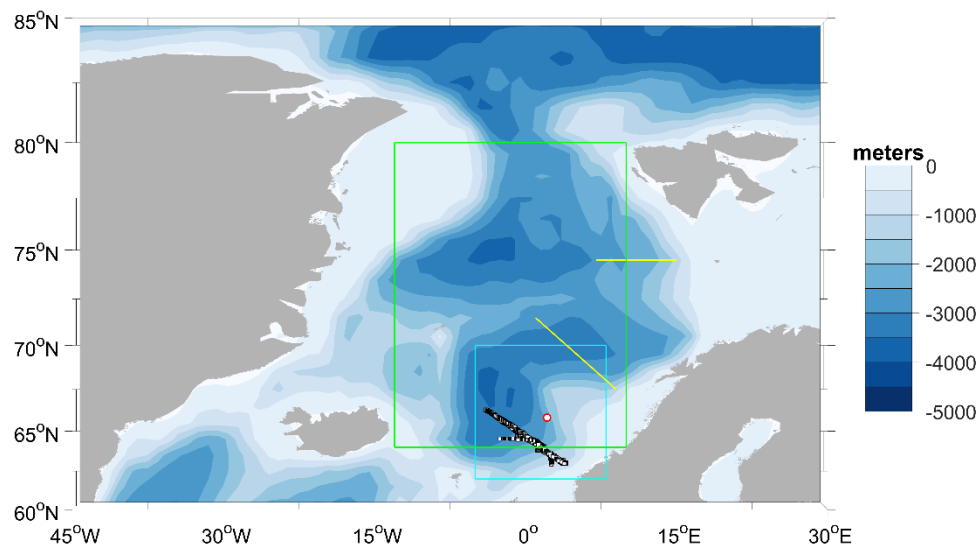
We used  $z_{\text{lim}} = 45$  m because this corresponds to the average depth of the euphotic zone, which is the region of interest for net community production from a biogeochemical and ecological point of view.



Previous studies may have used  $z_{\text{lim}} = 100$  m for operational reasons (e.g. constrained by discrete sampling depths).

To show the influence of  $z_{\text{lim}}$  on  $N$ , we calculated  $N$  for  $z_{\text{lim}} = 30$  m and 100 m.  $N(C_T; 30 \text{ m})$  was 0.6 and  $N(C_T; 100 \text{ m})$  was  $-0.04 \text{ mol m}^{-2} \text{ a}^{-1}$ .  $N(\text{O}_2; 30 \text{ m})$  was 4.6 and  $N(\text{O}_2; 100 \text{ m})$  was  $4.3 \text{ mol m}^{-2} \text{ a}^{-1}$ . In the case of  $N(C_T)$ , the derived two values are lower to the previous studies where  $N(C_T)$  varied from 8.6 to  $2.0 \text{ mol m}^{-2} \text{ a}^{-1}$ .  $N(C_T; 100 \text{ m})$  was negative because the deep integration depth included water below the euphotic zone where the remineralisation of organic matter can increase  $C_T$ . This signal is not present in  $N(\text{O}_2)$  because the changes were largely controlled by  $\Phi(\text{O}_2)$  that was always positive. The calculated  $N(\text{O}_2)$  is in agreement with previous studies, which gave results between 2.6 and  $11 \text{ mol m}^{-2} \text{ a}^{-1}$ .

In the discussion, we will add a section where we explain the location and the period when the previous studies took place. Falck and Anderson (2005) used historical data from 1960 to 2000 collected in the area from  $62$  to  $70^\circ \text{ N}$  and from 1991 to 1994 collected at OWSM. Skjelvan *et al* (2001) used data collected from  $67.5^\circ \text{ N}$   $9^\circ \text{ E}$  to  $71.5^\circ \text{ N}$   $1^\circ \text{ E}$  and along  $74.5^\circ \text{ N}$  from  $7$  to  $15^\circ \text{ E}$  from 1957 to 1970 and 1991 to 1998. Kivimäe (2007) used the oxygen measured at OWSM from 1955 to 2005 and Falck and Gade (1999) used data collected in all the Norwegian Sea from 1955 to 1988. The glider in the transect moved from  $66.3^\circ \text{ N}$   $4^\circ \text{ W}$  to  $63^\circ \text{ N}$   $4^\circ \text{ E}$  (Figure 5 where the black dots are the glider dives, the green box the region used by Falck and Gade (1999), the yellow lines the transects used by Skjelvan *et al.* (2001), the azure box the region used by Falck and Anderson (2005) and the red dot the location that corresponds to the Ocean Weather Station M (OWSM) used by Kivimäe (2007)).



**Figure 5:** Map of the glider deployment showing the previous studies that estimated the net community production in the Norwegian Sea. The black dots are the glider dives, the green box the region used by Falck and Gade (1999), the yellow lines the transects used by Skjelvan *et al.* (2001), the azure box the region used by Falck and Anderson (2005) and the red dot the location that corresponds to the Ocean Weather Station M (OWSM) used by Kivimäe (2007).

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