Suggestions for revision

The revised manuscript has made a great effort in simulating the calculation of different processes of CO in the surface mix layer, including microbial oxidation, photoproduction and vertical diffusion. The revised manuscript is significantly improved and well presented. However, I still worry about the results about the accuracies of CO measurements, microbial oxidation and dark production. Also, there are a lot of assumptions (speculations) through the manuscript (e.g. CO photoproduction), which will also affect the budget and advection transport of CO. My detailed comments are below:

- Thank you for your review on our revised manuscript, again. We will carefully review your comments and address any specific suggestions you have provided.

1) In my opinion, the estimated CO fluxes of physical transport in the ES and BS should be presented in the abstract, conclusions sections.

- We understand this as a suggestion to quantify the imbalance in the CO budget (Table 2) for ES and BS and estimate the physical lateral transport accordingly. We agree that quantifying these values in the abstract and conclusion sections would be highly appropriate. Therefore, we have included these values in both sections (see Lines 16-18 and 582-583 as below).

"…While the CO budget in the surface mixed layer of NP was in balance, the CO production surpassed the consumption in ES, and vice versa in BS. The significant imbalances in the CO budget in ES (25±17 μmol m^{-2} day^{-1}) and BS (40±19 μmol m^{-2} day^{-1}) are suggested be compensated by external physical transport such as lateral advection, subduction, or ventilation…."

"… Estimated external physical transports in the ES and BS, derived from imbalances in the CO budget, were 25±17 μmol m^{-2} day^{-1} and 40±19 μmol m^{-2} day^{-1}, respectively…"

2) Lines 11-13: Change "Microbial consumption rates were 30(±8) μmol m-2 day-1, 24(±5) μmol m-2 day-1, and 63(±19) μmol m-2 day-1, and CO photochemical production rates were 56(±15) μmol m-2 day-1, 27(±3) μmol m-2 day-1, and 26(±2) μmol m-2 day-1 in ES, NP and BS," to "CO photochemical production rates were 56(±15) μmol m-2 day-1, 27(±3) μmol m-2 day-1, and 26(±2) μmol m-2 day-1, while microbial consumption rates were

30(±8) μmol m-2 day-1, 24(±5) μmol m-2 day-1, and 63(±19) μmol m-2 day-1 in ES, NP and BS,".

- Revised (see Lines 11-13).

3) Lines 111-113: I am still worry about the in-situ CO concentration in the water column based on the measurement procedure. Previous study suggest the system can reach equilibrium between headspace and seawater within ~ 5 min of vigorous shake (Xie et al., Mar. Chem. 2002), and it's not necessary to wait for 1 hour. Based on the microbial dark incubation experiments, the microbial CO consumption rate is no less than 0.1 nmol L-1 hr-1, which is significant in the following calibration and estimation. I mean that the authors should correct this uncertainties to each discrete water samples after measurements.

- Regarding the equilibration time for CO between the headspace and seawater in the glass jars, while Xie et al. (2002) suggest an equilibrium time of approximately 5 minutes, it's important to note that their study did not specify the exact temperature conditions under which this equilibrium was achieved. Additionally, based on findings by Chipman et al. (1993)* for CO_2, which is more soluble than CO, it may take over an hour to reach equilibrium without bubbling, and more than 5 minutes even with bubbling. Considering the lower solubility of CO compared to CO_2, it is reasonable to expect that CO may require a longer equilibration time.

  Furthermore, the temperature of the in-situ samples collected during our campaign varied widely, ranging from -1.6°C to 22°C. To ensure thermal equilibrium at a constant temperature, we immersed the glass jars in an isothermal water bath for approximately one hour. Our calculation using a simple heat flux model (e.g., Fourier's law) indicated that thermal equilibrium would require more than an hour. This step was taken to ensure both thermal equilibrium between the seawater in the glass jar and the water bath, and CO gas equilibration between the headspace and seawater in the glass jar.

  We appreciate the reviewer's concern regarding potential microbial oxidation of CO during the equilibration period at 20°C. While we initially assumed that microbes would not be able to adapt to the sudden change in temperature, we acknowledge the possibility of microbial activity influencing the results. This concern could be addressed in future

experiments by comparing equilibration times with and without poisoning organisms in the glass jar.

Additionally, it's worth noting that microbial activities in the in-situ samples may differ from those at 20°C, even if microbes were able to survive in the temperature of the thermostat water bath. Given the wide range of temperatures encountered in the samples and lack of information on the microbial activities at a variety of temperature, we believe it is appropriate to report the values as they are in the text.

**Chipman et al. (1993) Primary production at 47 N and 20 W in the North Atlantic Ocean – A comparison between the C-14 incubation method and the mixed layer carbon budget, Mar. Chem. V.40: 151-169.*

4) Lines 370-371: The authors mentioned the fluctuated CO concentrations (Figure 4) might be related with significant dark production and other processes. However, the authors still did not mention if there is dark controls during their onboard experiments with another sample poisoned to remove microbial consumption, but only dark production. This statement is self-contradictory to the assumption in lines 169-170, which will also affect the CO budget estimation.

- We acknowledge the reviewer's concern regarding the absence of a dark control experiment and our assumption regarding the negligible contribution of dark production to the observed CO concentrations. While we did not conduct a specific dark control experiment, the unexpected increases in CO concentration during the dark incubation led us to suggest the possibility of dark production as one of the potential explanations. However, it is important to note that the observed increase may not be solely attributed to dark production, as it could be influenced by various factors, including dark production, particulate production, and the existence of a CO consumption threshold, as we have hypothesized in the text. Furthermore, the dark production process itself remains poorly understood, making it challenging to pinpoint its specific contribution based on our present results.

  In our previous response, we clarified that we considered all data points for calculating the linear regression coefficients (k_{co}) used in our study. This approach ensures that the observed variations during the dark incubation are all accounted for in our calculations. Therefore, the statements in Lines 370-371 represent our hypotheses regarding the potential reasons for the observed increases during the dark incubation, rather than definitive conclusions. Importantly, our calculated k_{co} values are based on the

comprehensive consideration of all data points and fluctuations, thereby ensuring that they accurately reflect the overall CO dynamics in our study area. Consequently, we maintain confidence in the robustness of our CO budget estimation methodology, which incorporates the complexities of the observed variations.