**Reviewer #1**

**General comments:**

The authors conducted an interesting marine CO cycle analysis in a latitudinal study area from the Korea Peninsula to Alaska, U.S.A.. This study 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 manuscript is well presented and generally sound. However, I deeply worry about the results about the microbial oxidation, and hence the budget and advection transport of CO, since the in situ incubation was not well conducted, and the calculated kCO held large uncertainty.

I have some questions about the incubation experiment: 1) were duplicates or triplicates conducted for each sample? 2) the incubation experiment was conducted in glass jars, but how did the authors collect subsamples at each time point. I mean after collection, how did you fix the space of the subsample in the jar, leave it with atmosphere, seawater sample or others? 3) based on Figure 4, CO concentration fluctuated with time. The authors mentioned it might be related with dark production. So is there dark control with another sample poisoned to removal microbial consumption, but only dark production? 4) I could not obtain the microbial oxidation rate (M) of CO in each province presented in Table 2 based on Equation 9, and the air-sea fluxes presented here were not consistent with those in Table 1. 5) Equation 7, it should it be “1-A” instead of “A”? Also I(l,0-) and I0(l,0-) are without and with normalization using the observed Iobs, respectively. If you use this equation, I doubt the photoproduction rate of CO is also incorrect. So I strongly recommended the authors to recheck their original data and recalculate the photoproduction, microbial consumption and air-sea fluxes of CO in each province, and resubmit it.

: We sincerely appreciate the reviewer's constructive feedback on our study and their interest in our marine CO cycle analysis. We fully understand the concerns raised regarding microbial oxidation and its impact on the CO budget and advection transport estimations. The reviewer's valuable input shall help us improve the quality of our research and address the concerns raised. We are committed to ensure the accuracy and reliability of our findings.

Regarding your specific questions:

**1)** Unfortunately, we did not conduct duplicate or triplicate incubation experiments for each sample. Although we acknowledge that replicating the incubation experiments would have provided a more robust assessment of the microbial oxidation processes, the method we used at that time does not allow us to do replicate experiments because of the limited amount of seawater sample and the number of glass jars utilized. It needs at least 4 glass jars for the experiments. Volume of glass jar was approximately 200 mL, and thus about 2.5 L of seawater samples were used for this experiment only. In case of duplicate experiments, we had to give up measuring other parameters. In future research, we will consider conducting duplicate or triplicate experiments to enhance the reliability of our results by developing a method by which a small amount of seawater samples can be applied.

**2)** We carefully filled the seawater in four glass jars in a row from the same Niskin bottle, ensuring they were nearly identical. The glass jars were wrapped with colored cellulose film to protect from the UV radiating from the fluorescence lamp in the sampling room and in the laboratory. Upon collection, one of these underwent immediate analysis. Subsequently, the remaining three samples were analyzed at distinct time intervals. For the analysis, we created a headspace within each sample bottle by introducing ultra-pure N2 gas (99.9999 %). To remove trace amount of CO in the N2 gas, Schuetze reagent and CO2 trap (Ascarite) were mounted right after flowing from the N2 cylinder. After allowing the samples to equilibrate, we extract the headspace sample for analysis. For a more comprehensive understanding of this procedure, we have incorporated a detailed explanation in Section 2.4 (as below) of the manuscript to enhance clarity.

## *“2.4 Determination of microbial oxidation rate constant*

*Dark incubation experiments were conducted on-board at selected stations to determine the microbial oxidation rate constant of CO () in unfiltered seawater samples collected in the surface mixed layer. At the selected stations (marked with red circles in Figure 1), four aliquots of seawater were subsampled into glass jars from a Niskin bottle. To prevent light exposure during sampling, the glass jars were covered with colored cellulose film. These jars were then placed in an aquarium where surface seawater was continuously supplied to maintain a temperature identical to that in the surface mixed layer. Upon collection, one of these sample bottles underwent immediate analysis to measure dissolved CO concentration. Subsequently, the remaining three samples were analyzed at distinct time intervals following the initial sampling. Before analysis, we introduced ultra-pure N2 gas into each sample bottle to create a headspace, following the same procedure outlined in Section 2.3. After allowing the samples to equilibrate, we extract the headspace sample for analysis.*

*As CO depletion follows quasi-first-order reaction kinetics at ambient CO concentrations (Johnson and Bates, 1996; Jones and Amador, 1993), we fitted the data with the best-fit lines using the following equation:*

|  |  |
| --- | --- |
|  | *(2)* |

*, where t represents time (hr) and kco is the microbial oxidation rate constant for the reaction (hr-1), and [CO]t and [CO]0 denote the CO concentrations at time t and the beginning of the incubation experiment, respectively.“*

**3)** We acknowledge that these fluctuations may be related to dark production. However, it's important to clarify that we did not use a separate dark control sample in the incubation experiment. The purpose of our incubation experiments was to evaluate the natural decreasing rate in CO concentration in seawater over time under no light conditions disregarding other unknown processes which might introduce significant uncertainties in our focus on the source-sink comparison among the different environments in the North Pacific. We kindly suggest to refer our response to the specific comment #2 below as well. However, we also understand the potential importance of unknown processes of CO dynamics, and in future research, we will consider incorporating them to better isolated conditions.

**4)** We would like to provide clarity regarding the differences between the values presented in Table 1 and Table 2. Table 1 displays values derived from the averages of measurements obtained over the entire span of each province, denoted by the red lines in Figure 2. The purpose of Table 1 is to offer an overview of the properties within our cruise track area. In contrast, Table 2 presents values that result from daily integration at individual stations, corresponding to the gray shaded periods in Figure 2. This distinction arises because certain parameters, such as irradiance and wind speed, were continuously measured along the cruise track while parameters like *k*CO and MLD were estimated for specific hydrographic stations at which the seawater samples were collected to determine microbial oxidation rate and CDOM concentrations in addition to auxiliary parameters. To calculate the daily CO budget at each station, as listed in Table 2, we integrated the product of *k*CO and [CO]sea over the course of a day (also coinciding with the gray shaded periods in Figure 2) (see Figure S3 in supporting information, for instance). Furthermore, we adjust the units from μmol m-3 to μmol m-2 by multiplying with the respective station's MLD, ensuring consistency with other budget values (photoproduction and air-sea flux density). To improve clarity, we have modified Eq. (10) to include this multiplication by MLD. Consequently, the variations between the values in Table 1 and Table 2 are a result of these methodological distinctions (same for air-sea flux, *F*). To enhance clarity, we have revised the captions for both Table 1 and Table 2 to explicitly address these distinct estimation approaches.

**5)** We appreciate the reviewer for pointing out the typos. It is important to note that ‘I’ or ‘I0’ in Eq. (5) to (9) indicates irradiance. Thus in Eq. (8), ‘A’ should be replaced with ‘(1 – A)’, and we have verified the correct calculation. As the reviewer mentioned, the photochemical production of CO could have been entirely wrong if ‘A’ were multiplied instead of ‘(1 – A)’. We have also corrected the other typo, which involved attaching subscript, ‘0’ next to ‘I’, as shown in line 183 of the revised manuscript. We sincerely apologize for these misleading typos. However, as demonstrated in Eq. (9), *I*0(λ,0-) was correctly calculated. The revised sentences in the main text is as below:

*“The photochemical production rate (J) was determined by product of irradiance (I0), the amount of CDOM (ac), and apparent quantum yield () of CDOM, an indicator of the CO production efficiency. Since CDOM is not a chemical compound but rather a moiety of material defined by mechanical criterion, varies depending on a variety of environmental conditions. J can be mathematically described as follows:*

|  |  |  |
| --- | --- | --- |
|  |  | *(5)* |

*, where indicates monochromatic solar irradiance just beneath the air-sea interface, diffuse attenuation coefficient, apparent quantum yield of CO. Parentheses in Eq. (5) indicate function of variables, i.e., λ wavelength and z water depth.”*

**Specific comments:**

1) Line 395: For the CO budget, I would suggest placing this part in the “Discussion” section.

: We retained the results of the budget calculation in the Results section (Section 3.6.5) and revised the budget analysis, relocating it to the Discussion section in response to this comment (see Section 4.1).

2) Since the real experiment on CO microbial oxidation hasn’t shown a reduction, and the author estimated the oxidation using a decay function. This estimation method carries risks, potentially leading to an overestimation of the “sink” role of CO microbial oxidation in the study areas. In this section the authors disregard the presence of dark production or a threshold [CO] for consumption. Thus, how about providing a schematic graph illustrating the budget estimation of different processes? By quantifying the sources (inputs) and sink (outputs) of other processes, it would be clearer to identify whether the central bulk is primarily involved to “oxidation”, “production” or a state of balance.

: We genuinely appreciate the reviewer's thoughtful suggestions regarding the potential influence of dark production on our CO budget estimations and the idea of providing a schematic graph to illustrate the budget estimation of different processes. However, we would like to clarify our approach and rationale.

In our study, we did acknowledge the presence of potential unknown processes, including dark production, in our estimation of the CO budget. Figure 4a-c, which depicts the *k*CO at different stations, already reflects this consideration because we included all data points, even those indicating larger values than the previous time step, for calculating *k*CO. The figure presents the linear regression coefficient (slope), that is, the *k*CO, along with the error range, allowing us to identify a clear decreasing trend overall, despite accounting for the uncertainties. Therefore, we believe that a two-way process schematic may not provide significantly more clarity. It's important to note that while taking mean values might introduce some level of uncertainty, this is inherent to such calculations and can both overestimate and underestimate the actual oxidation rate. Our primary focus in this study was to compare CO dynamics in different physico-chemical environments within the vast ocean rather than attempting to unravel the intricacies of uncertain processes affecting the CO budget. We hope this clarifies our approach and the reasons behind not including a schematic graph.

3) Line 400: I would suggest adding the vertical profiles of temperature, salinity, density and kCO for further discussion, thus it would be better to move this section forward.

: We have included vertical profiles of temperature, salinity, and density in our supplementary materials attached (Figure S7). However, it's important to note that we did not measure the *k*CO profile at different depths, assuming that vertical mixing can occur over the microbial oxidation timescale throughout the mixed layer (Gnanadesikan, 1996). We retained the brief description of CO vertical profiles in the Results section (Section 3.7) and further discussed them in the Discussion section (Section 4.2). The revised portions are too lengthy to include here, so we kindly request referring to our revised manuscript for the complete details.

4) Line 420–425: As mentioned before, the CO budget calculations need to be rigorous, as the results of the experiment exhibit high uncertainties.

: Please refer to our response provided in comment #2.

**Minor comments:**

5) Line 9: “Northwestern Pacific Rim”?

: We have revised the term "Northwestern Pacific limb" to "Western limb of the North Pacific" in the manuscript to better convey the intended meaning of the region. The revised line now reads as follows (Line 10).

*“...water column were measured along the western limb of the North Pacific from Korean Peninsula to Alaska,...”*

6) Line 37: The unit of “0.003 to 1.11 h-1” used here should be understood as referring to rate constants, not consumption rates.

: We appreciate clarifying this point. We have revised Line 37 and other parts of the text to indicate that the values with the unit 'hr-1' represent rate constants, not consumption rates.

*“The rate constants governing microbial consumption of CO exhibit considerable variability, ranging from 0.003 to 1.11 hr-1....”*

7) Line 43–50: Please rephrase this paragraph.

: We revised the paragraph with more proper expressions (see Line 45-54).

*“Efforts to estimate the oceanic source strength of CO encounter significant challenges due to the substantial uncertainties inherent in the marine CO budget. Recent modelling endeavors have aimed at estimating the global-scale CO flux from the ocean surface (Conte et al., 2019). However, these estimations grapple with formidable uncertainties, especially in regions characterized by shallow continental shelves. Additionally, attempts have been made to address these challenges by introducing a new production pathway known as dark production (Xie et al., 2005; Kettle, 2005b; Zhang et al., 2008), which seeks to reconcile the discrepancies between modeled and observed oceanic CO source strength. Nevertheless, the widespread occurrence of dark production at a global scale remains a subject of ongoing debate (Zafiriou et al., 2008). The identification of missing components within the CO budget holds paramount importance, as it can significantly enhance our predictive capabilities, allowing for a better understanding of the dynamic interplay between oceanic CO levels and the broader context of global climate change.*”

8) Line 129: “Zhang et al., 2006”? and this reference is not listed at the end of the manuscript. Also there are some incorrect citation, such as Li et al., 2015

: We apologize for the oversight in our citations. Our technical difficulties with the use of 'EndNote' led to these errors. We want to confirm that the citation for 'Zhang et al. (2008)' in our manuscript was indeed correct, and we have taken the necessary steps to add it to the reference list. Additionally, we have carefully reviewed and revised all other citations in our previous manuscript to ensure their accuracy and consistency.

9) Line 155–160: Please explain the calculation of photochemical production rate (J) in detail, providing the formula and parameters.

: We add the details of formulation used in the calculation of photochemical CO production in the Supplementary Information (refer to Text S1 in supporting information attached).

10) Table 1: I would prefer to use (N = xxx) in parentheses to avoid potential misunderstandings.

: We revised the table following this comment (See the revised Table 1 attached).

11) Figure 1: Misspell of “Tsugaru Strait”.

: We modified the misspelled in Figure 1 (attached).

12) Figure 2: Please enlarge the front size here.

: We revised the figure following this comment (See the revised Figure 2 attached).

13) Line 284: “measurements were reliable”

: The phrase has been revised more properly (Line 315).

*“... demonstrating the reliability of our measurements.”*

14) Line 320–325: It should be “Microbial CO consumption rate constants”. Could you explain more about the first-order decay function that you used for the CO oxidation estimation?

: We modified the section title as ‘Microbial CO consumption rate constants’ (line 358). And we added more detailed explanation for the first-order decay function in Section 2.4 as below.

*“....As CO depletion follows quasi-first-order reaction kinetics at ambient CO concentrations (Johnson and Bates, 1996; Jones and Amador, 1993), we fitted the data with the best-fit lines using the following equation:*

|  |  |
| --- | --- |
|  | *(2)* |

*, where t represents time (hr) and kco is the microbial oxidation rate constant for the reaction (hr-1), and [CO]t and [CO]0 denote the CO concentrations at time t and the beginning of the incubation experiment, respectively......”*

15) Line 365: The unit “μmol m-2 d-1” refers to flux density.

: Revised (see Line 413)

*“The air-sea CO flux densities ranged from –0.5 to 19 μmol m−2 d−1 (Figure 2e),.....”*

16) Lines 296-297: why the author relate the low [CO] in the upper ocean to the lower productivity (i.e. Chl a)? CO is mainly produced by photoproduction from CDOM and POM, but not biological origin.

: In the open ocean, a significant portion of CDOM originates from autochthonous sources, primarily phytoplankton (Steinberg et al., 2004). Consequently, we considered Chl-*a* as a potential proxy for CDOM production. To enhance the clarity of our statement, we have modified the phrase in Line 331-334 as below.

*“.....It is important to note that our study area, along with the mentioned regions, all falls under the category of Case 1 waters, where the Chl-a concentration can serve as a proxy for CDOM production, as discussed in Steinberg et al. (2004). Therefore, the combination of low productivity and overcast conditions can partially explain our lower mean CO concentration (Figures 2c&g)....”*

17) Lines 305-306: In many cases, the absorption coefficient is negatively correlated with its spectral slope. So, here you could not conclude the similar biogeochemical process between Pacific and Atlantic.

: It is reasonable to revise the conclusion pointed out (see Line 342-344 as below).

*“....Nonetheless, the values of and S obtained in this study are relatively high and low, respectively, compared to those in the Atlantic, possibly due to the influence of CDOM sources in the marginal seas adjacent to the Pacific Ocean and to relatively short photo-bleaching processes occurred in the open ocean (Vodacek et al., 1997)....”*

18) Line 328: the microbial CO consumption rate constants here were not consistent with those presented in Table 1 (the values and the unit)

: We apologize for the inconsistencies in the microbial CO consumption rate constants presented in our manuscript. Upon careful review, we identified all the errors and have taken immediate steps to rectify them. We recalculated the *k*CO constants meticulously, ensuring accuracy and consistency. The revised values have been updated in the manuscript, including Table 1 (please refer to the attached file) and all relevant sections.

**Reviewer #2.**

The manuscript by Kown et al. quantified the budget of carbon monoxide (CO) in the mixed layer of the East Sea (ES, Sea of Japan), the Western North Pacific (NP), and the Bering Sea (BS). Kown et al. also examined the factors contributing to uncertainties in the CO budget and highlighted the potential importance of physical transport in the oceanic CO cycle. This study aims to enhance our understanding of the CO cycles in these regions. However, there are many defects throughout the manuscript, including the language, logic, figures, tables, references, etc. So, this paper is not suitable to be published in this journal.

: We sincerely appreciate the Reviewer for taking the time to assess our manuscript. We acknowledge the concerns and criticisms, and we are committed to addressing them thoroughly to enhance the quality of our work. We understand the importance of rigorous scientific communication and will diligently revise the manuscript to improve its language, logic, figures, tables, references, and overall clarity. Our aim is to contribute valuable insights into the CO budget in the East Sea, Western North Pacific, and Bering Sea, as well as to explore the factors contributing to uncertainties in the CO budget, including the role of physical transport in the oceanic CO cycle. We remain dedicated to improving the manuscript to meet the standards of this journal and appreciate the opportunity to refine our work.

**Major comments:**

**1) Grammar:** There are lots of grammatical errors throughout the manuscript. It would've been better to send this paper to a professional English editor before submission to a journal for publishing consideration.

: We apologize for any grammatical errors in the manuscript. We have made efforts to improve the manuscript's language and clarity in this revised version, and we hope that the overall quality meets the standards for publication.

**2) Figures:** Some figures show incomplete information. Mark the corresponding information (e.g., concentration, etc.) for discrete samples in Figure 2 to observe whether there is a significant difference in the determination of sample concentration between the two sampling methods. Show the results of the fitted curves (e.g., R2, P-value) in Figure 3b. What does the right Y-axis in Figure 5 represent? What are the units? What do the blue, orange, green, and magenta columns represent? Display the results of the linear fit in Figure 7b.

: We apologize for the missing information in the figures and appreciate your feedback. Here are the revisions and responses to your questions:

* Figure 2b: We have added the concentrations estimated at 7 m depth by linear interpolation to compare with the underway measurements. The discrete samples at the stations 1 – 4 and 8 – 9 tended to show higher concentrations compared to the underway measurements, due likely to strong vertical gradient CO (Figure 6a-c) or horizontal heterogeneous distribution (e.g. Wang et al. (2017)). In addition, the discrete samples represent measurements taken at fixed times and locations, while the underway measurements provide average concentrations over a roughly one-hour period, a certain distance traveled by the ship. This difference in sampling strategy likely contributed to the observed variations, as well. This is discussed not only in the main text (Lines 462-473 as below) but in the Supplementary Information attached (Figure S6).

*“...We assessed the impact of this vertical gradient of the CO to the difference in CO concentration between underway and discrete measurements (Figure S6). Given the coarse resolution of the CO profiles, we first applied curve fitting to the profile and estimated the vertical gradient of dissolved CO at the depth at which the seawater was continuously supplied for the underway observation of surface CO concentrations. As illustrated in Figure S6, the greater the vertical gradient at the depth of the seawater inlet, the larger the difference in the CO concentrations between the underway observation and discrete measurement. In addition to the vertical gradient, horizontal variability likely plays a role in the difference of the CO concentrations between the two methods, as evidenced at Stations 8 and 9 (Figure 2b).*

*Another factor to consider when comparing surface CO concentrations using the two methods is that continuous observation in underway system can mitigate the presence of high or low spikes in dissolved CO concentrations due to the long equilibration time associated with sparingly soluble gases, such as CO (Johnson, 1999). Taking these spatial variabilities in CO concentrations and the dynamic equilibration occurring in the underway system into account, it is possible to obtain different CO concentrations using the two sampling methods for measuring dissolved CO....”*

* Figure 3b: We have added a fitted curve to this figure and included R2 to provide a comprehensive view of the data.
* Figure 5: We have improved the clarity of the right Y-axis title and added an explanation of the meaning of the blue, orange, green, and magenta columns.
* Figure 7b: We have included the linear fit results in this figure and added the correlation coefficient in line 490 to enhance the presentation of the data. We attached all the revised figures above.

**3) Introduction:** There is a lack of connection between the third and fourth paragraphs. Add a paragraph summarizing the budget for CO in the oceans.

: As we acknowledge the lack of coherence among the paragraphs in the introduction, we have revised the second through fourth paragraphs to ensure better continuity among them, with a focus on their interconnectedness (lines 28-54 as below).

*“...In the ocean’s euphotic zone, CO undergoes production through abiotic photochemical reaction of chromophoric dissolved organic matter (CDOM) and particulate organic matter (Xie and Zafiriou, 2009). The annual global CO photoproduction in the ocean spans an estimated range of 10 to 400 Tg CO (Mopper and Kieber, 2000; Zafiriou et al., 2003; Erickson III, 1989; Conrad et al., 1982; Fichot and Miller, 2010; Stubbins et al., 2006b). This wide range of estimations is largely attributed to the uneven distribution of CDOM throughout the world oceans. CO, as the second most substantial inorganic carbon product after CO2 in photochemical conversion of dissolved organic carbon, garners significant attention. It serves as a pivotal proxy for assessing the photoproduction of CO2 and bio-labile organic carbon (Mopper and Kieber, 2000; Miller et al., 2002). Thus, CO holds a prominent position within the context of both the oceanic carbon cycle and the broader realm of global climate change.*

*In contrast, CO within the water column* *experiences removal mechanisms that include microbial consumption, air-sea gas exchange, and vertical dilution. Under normal turbulent conditions at the ocean's surface, microbial consumption emerges as the dominant sink for CO. The rate constants governing microbial consumption of CO exhibit considerable variability, ranging from 0.003 to 1.11 hr-1 depending on factors such as location and season (Conrad and Seiler, 1980; Conrad et al., 1982; Johnson and Bates, 1996; Jones, 1991; Ohta, 1997; Xie et al., 2005; Zafiriou et al., 2003; Jones and Amador, 1993). Notably, dissolved CO in the surface ocean tends to be supersaturated with respect to the atmospheric CO concentrations (Seiler and Junge, 1970)* *resulting in emission from the sea to the air (Conrad and Seiler, 1980). However, the influence of physical mixing within the surface mixed layer becomes apparent when the mixed layer depth exceeds the depth of light penetration depth, rendering photoproduction no longer dominant driver (Gnanadesikan, 1996; Kettle, 1994).*

*Efforts to estimate the oceanic source strength of CO encounter significant challenges due to the substantial uncertainties inherent in the marine CO budget. Recent modelling endeavors have aimed at estimating the global-scale CO flux from the ocean surface (Conte et al., 2019). However, these estimations grapple with formidable uncertainties, especially in regions characterized by shallow continental shelves. Additionally, attempts have been made to address these challenges by introducing a new production pathway known as dark production (Xie et al., 2005; Kettle, 2005b; Zhang et al., 2008), which seeks to reconcile the discrepancies between modeled and observed oceanic CO source strength. Nevertheless, the widespread occurrence of dark production at a global scale remains a subject of ongoing debate (Zafiriou et al., 2008). The identification of missing components within the CO budget holds paramount importance, as it can significantly enhance our predictive capabilities, allowing for a better understanding of the dynamic interplay between oceanic CO levels and the broader context of global climate change....*”

**4) Materials and Methods:** Two sampling methods were used to determine the concentration of CO in seawater. The data in Figures 2b and 6a-c appear to be inconsistent. Please conduct a statistical analysis on the data obtained by the two methods. If there is a significant difference, explain why. What is the concentration of the standard gas? What was the analytical accuracy of the standard gas used for calibration of CO analyzer? What is the detection limit of the CO analyzer? Discuss in the manuscript.

: We added the concentrations from the discrete samples in Figure 2b. The reason for differences between the concentrations from the two sampling methods were described in lines 462-473 (as below) of the main text and Supplementary Information in detail (Figure S6). The detection limit of the CO analyzer, the GC-RGA is estimated to be 6 ppb. Regarding the standard gas information and analytical uncertainties, we described in lines 78-89 (as below) in detail.

* lines 462-473 *:“...We assessed the impact of this vertical gradient of the CO to the difference in CO concentration between underway and discrete measurements (Figure S6). Given the coarse resolution of the CO profiles, we first applied curve fitting to the profile and estimated the vertical gradient of dissolved CO at the depth at which the seawater was continuously supplied for the underway observation of surface CO concentrations. As illustrated in Figure S6, the greater the vertical gradient at the depth of the seawater inlet, the larger the difference in the CO concentrations between the underway observation and discrete measurement. In addition to the vertical gradient, horizontal variability likely plays a role in the difference of the CO concentrations between the two methods, as evidenced at Stations 8 and 9 (Figure 2b).*

*Another factor to consider when comparing surface CO concentrations using the two methods is that continuous observation in underway system can mitigate the presence of high or low spikes in dissolved CO concentrations due to the long equilibration time associated with sparingly soluble gases, such as CO (Johnson, 1999). Taking these spatial variabilities in CO concentrations and the dynamic equilibration occurring in the underway system into account, it is possible to obtain different CO concentrations using the two sampling methods for measuring dissolved CO...”*

* lines 78-89 *: “...The analytical system was calibrated with commercially available calibration gases (49.09±1.16 ppb, 102.0±0.7 ppb, and 912.8±4.7 ppb) during the SHIPPO campaign. The dry mole fractions assigned to these calibration gases were adjusted based on traceable standard gases from NOAA/ESRL/GMD (NOAA-GMD/WMO 2004 scale). For measuring high CO concentrations (>1 ppm), the highest concentration of calibration gas was adjusted using Swiss Empa standard gases (personal communications, 2012). To cover a wide range of CO concentrations between the air and surface seawater, two different size of sample loops (0.5 mL and 2 mL) were installed on the 10-port VICI valve. This setup allows us to confidently measure CO concentrations of up to ~2 ppm in unknown samples in confidence, as the concentrations of the standard gases range from ~20 ppb to ~1800 ppb. Beyond this range of unknown samples, we anticipate an increase in analytical uncertainty. The uncertainties (1*σ*) associated with the standard gases are estimated to be between 0.5 ppb and 1.1 ppb, following the NOAA-GMD/WMO 2004 scale (see Figure S1). The detection limit of the system was determined to be 6 ppb (= 3*σ *of blank signals) based on the blank runs applied during discrete sample analysis. To correct for detector signal drift, calibration runs were performed every 40 minutes during sample analyses. ...”*

**5) Statistical analysis:** In the Materials and Methods section, please specify the type of software and methodology employed for statistical analysis in the paper.

: Following the comments, we added the type of software and methodology employed for statistical analysis by adding a new subsection 2.8 (as below).

## *“....2.8 Statistical analysis*

*In calculating all statistical values, we utilized Microsoft Excel, Python, or IDL (Interactive Data Language, NV5 Geospatial) depending on the specific case. For instance, when estimating linear and exponential curve fitting to explore the relationships between parameters, we employed Python programming. The determination of kCO values and error ranges, as well as the CO budget calculations, were carried out using the IDL program.....”*

**6)** Please list the units for each parameter in all formulas.

: We have added the units for each parameter in all formulas in the revised manuscript.

**Specific comments:**

7) Line 29: Kitidis et al. (2006) reported variability of CDOM in surface waters of the Atlantic Ocean. This is not related to the photoproduction of CO. Please delete citation of Kitidis et al.’s (2006) paper from this line.

: Removed (line 30 as below).

*“...The annual global CO photoproduction in the ocean spans an estimated range of 10 to 400 Tg CO (Mopper and Kieber, 2000; Zafiriou et al., 2003; Erickson III, 1989; Conrad et al., 1982; Fichot and Miller, 2010; Stubbins et al., 2006b).....”*

8) Line 84: What is “Schuetze reagent”? Please complete the information about "Schuetze reagent" (e.g., manufacturer, specifications, etc.).

: Schuetze reagent has been used to oxidize CO to CO2 in stable isotope analyses. We refer to the references added in the text. One of co-authors benefited sharing the material from Dr. Carl Brenninkmeijer (citation in Line 102).

9) Line 90-92: Give a specific value for the gas constant. Explain the Benson solubility coefficient and cite Wiesenburg and Guinasso, 1979.

: We add the gas constant in line 110. The definition of Bunsen solubility is added lines 111-114 and we have cited Wiesenburg and Guinasso (1979) in Line 113 as below.

*“....., where xCO represents the dry mole fraction of dissolved CO (nmol mol-1), P the ambient pressure (atm), Pw saturated water vapour pressure (atm), ρw the density of seawater at a given temperature (kg L-1), R gas constant (0.08205601 L·atm·mol-1K-1), T absolute temperature at the sampling time of dissolved gas (K), Vh volume of the headspace (mL), and Vw volume of the seawater (mL). denotes the Bunsen coefficient of CO solubility which is defined as the volume of CO gas, reduced to STP (0°C 1 atm) contained in a unit volume of water at the temperature of the measurement when the partial pressure of the CO is 1 atm (Wiesenburg and Guinasso, 1979). The normalization of at the absolute temperature of 0°C in Eq. (1) is called the Ostwald coefficient solubility, L. .....”*

10) Line 170: List the formula for Ostwald coefficient.

: The Ostwald coefficient is simply a conversion of Bunsen coefficient to the temperature when the gas concentration was measured. Please refer to the Lines 114 and Eq. (1).

11) Line 192-193: “…its physical properties…” to “… the physical properties of WC…”.

: Revised (line 226 as below).

*“.....the physical properties of TWC undergo slight modifications in the East China Sea.....”*

12) Line 284: Give the web address of NOAA/ESRL global network.

: We added the web address (See line 317).

13) Line 285-287: “…varied by about 30% with respect to mean value of 118 nmol mol-1…” what do you mean? Is COair 30% higher or lower than the average? 118 nmol mol-1 is the mean value of what? Why does this statement reveal that the large variability of CO in the Northern hemisphere is related to anthropogenic emissions?

: We apologize for any confusion in our previous statement. The phrase "varied by about 30%" refers to the standard deviation of atmospheric CO concentrations with respect to the mean value of 118 nmol mol-1, which represents the average CO concentration across our entire cruise track. In other words, the standard deviation of CO concentrations around this mean value is approximately 30%, indicating some degree of variability in atmospheric CO levels along our study area (lines 318-320 as below).

*“....Atmospheric CO mole fractions exhibited approximately a 30% variation with respect to the mean value of 118 nmol mol−1, revealing significant variability associated with anthropogenic emissions in the Northern Hemisphere, in particular in Chinese mainland and Korean Peninsula (Park and Rhee, 2015).....”*

Regarding our mention of anthropogenic emissions, it's important to note that atmospheric CO concentrations tend to remain relatively constant in areas with minimal anthropogenic influences. This is because the majority of atmospheric CO comes from anthropogenic sources, such as the burning of fossil fuels in vehicles, industrial processes, and power plants. Anthropogenic emissions significantly contribute to atmospheric CO levels, especially in proximity to continents, leading to higher CO concentrations. As a result, the atmospheric CO levels in the Northern Hemisphere are generally about three times higher than those in the Southern Hemisphere due to the greater presence of anthropogenic sources in the Northern Hemisphere.

14) Line 296-297: “… lower than the values observed in other areas due probably to lower productivity evidenced by low Chl-a concentration … ” List the average concentrations of CO and Chl-a in other papers.

: We apologize for any previous confusion. Our intention was to convey the following:

In Case 1 waters, such as our study area, Chl-a can indeed serve as a proxy for CDOM levels. Considering that our study area is characterized as a low-productivity region, it is reasonable to expect relatively low CDOM content compared to other marine regions. This observation and the low insolation by general high cloudiness can help partially explain the lower CO levels observed in our study area. We have made the necessary modifications to the sentence to ensure that it accurately conveys the intended information (lines 331-334 as below).

*“...It is important to note that our study area, along with the mentioned regions, all falls under the category of Case 1 waters, where the Chl-a concentration can serve as a proxy for CDOM production, as discussed in Steinberg et al. (2004). Therefore, the combination of low productivity and overcast conditions can partially explain our lower mean CO concentration (Figures 2c&g)....”*

15) Line 304-306: “This inverse relationship is consistent with the observations in the Atlantic Ocean…” This inverse relationship is universal and does not indicate that the biogeochemical properties of CDOM in the two sea areas are similar.

: We appreciate the comments and removed the corresponding phrase “, implying similarities in the biogeochemical properties of CDOM between the Atlantic and Pacific open oceans” in the revised text. We have incorporated improved expressions regarding the CDOM data in lines 342-344, as follows.

*“Nonetheless, the values of and S obtained in this study are relatively high and low, respectively, compared to those in the Atlantic, possibly due to the influence of CDOM sources in the marginal seas adjacent to the Pacific Ocean and to relatively short photo-bleaching processes occurred in the open ocean (Vodacek et al., 1997).”*

16) Line 324: The paper reported by Li et al. (2015) is not related to the dark production of CO. Please delete citation of Li et al.’s (2015) paper from this line..

: We apologize once again for our oversight regarding the literature citation in the text. As previously mentioned, our reference management program, EndNote, did not function correctly, and we thoroughly reviewed the literature citation during the manuscript revision. The reference “Li et al. (2015)” should be replaced with “Zhang et al., 2008” in line 358 of the revised version.

17) Line 328: It is stated here that the mean kco value in the NP is 0.17 ± 0.35 hr−1. It is not consistent with the Table 1. Please check.

: We apologized for any confusion caused. The mean *k*co value in the NP shown in Table 1 is correct, and we have made the necessary corrections in the text accordingly (line 366 as below).

*“Mean values in ES, NP, and BS were determined at 0.27(±0.05) hr−1, 0.13(±0.15) hr−1, and 0.36(±0.39) hr−1, respectively.”*

18) Line 330: “…high Chl-a or active primary productivity can serve as an indicator of the activity of CO-oxidizing microbes.” Xie et al. (2005) reported that the kco in the Beaufort Sea was positively correlated with the concentration of Chl-a. However, it can be seen from the data in Table 1 that the manuscript is inconsistent with the findings of Xie et al. (2005). Please give a figure for the relationship between kco and Chl-a.

: We apologize large confusion in conveying our discussion with the phrase. As pointed out, the *k*CO values and the in situ Chl-*a* measurements did not show any significant relationship. However, we wanted to suggest the consistencies with Xie et al. (2005) in terms of the fact that *k*CO values tends to be reduced from the bay through the coastal area to the offshore area. Our *k*CO values also showed larger values in the two marginal seas near the continent compared to the province NP. We revised the overall phrases the reviewer pointed out and please see lines 367-375 (as follows).

*“**...The decrease in the mean values from the marginal seas to the open oceans aligns with previous findings compiled by Xie et al. (2005), indicating a decreasing trend in from bay to offshore areas. Xie et al. (2005) speculated that the high observed in the Beaufort Sea in their study might be due to the Arctic Ocean receiving substantial inputs of terrestrial organic carbon, which promote the growth of microbial communities. Indeed, considerable fluvial input of organic carbon has been observed over the Bering Sea near the Arctic Ocean (Walvoord and Striegl, 2007; Mathis et al., 2005). This could partially explain the higher in the Bering Sea compared to the East Sea, despite both being marginal seas. Nonetheless, as Figure 4d shows, the mean kCO values in the marginal seas (ES and BS) and at Station 4 in NP are larger than the values measured in NP, alluding a division of microbial activities between terrestrial and open ocean influences. As indicated by the absorption spectrum of the CDOM, the MLD water masses at Station 4 should be affected by the terrestrial input....”*

19) Line 339-340: “…while there was little difference between NP and BS due to the high CDOM content in BS.” What do you mean?

: We apologize for any confusion in our previous statement. Our intention was to convey that the high CDOM observed in the BS can result in a photoproduction rate in the BS that is comparable to that in the NP, despite the lower irradiance levels in the BS. We have revised the sentence as indicated in lines 380-383 (as follows) for clarity.

*“**...There was little difference between NP and BS, despite the lower insolation in BS. This anomaly can be attributed to the high content of CDOM in BS (Table 1 and Figure 3a). The elevated J value in ES can be attributed to the combined effect of both high insolation and a significant CDOM presence in the surface seawater. At Stations 3 and 4, the reduced insolation was somewhat compensated for by the high CDOM content (Figure 3a), resulting in a J value similar to that at the other stations in NP....”*

20) Line 353-354: “highest” to “higher”. In addition, the mean dissolved [CO] in the BS was approximately 3 times higher than that in the ES (Table 1), and they were not similar.

: The reviewer's observation is correct, and we apologize for any confusion in our previous statement. The point we intended to convey is that the higher microbial oxidation rate (*M*) in the two provinces is a result of the high oxidation rate constant (*k*CO) in those provinces, despite the relatively low CO concentration in the ES. We have revised the sentence to accurately reflect this explanation (lines 398-399 as below).

*“...The microbial oxidation rates were higher in the marginal seas of ES and BS, which is attributed to the high kCO in those two provinces, despite the relatively low mean dissolved [CO] in the ES (Tables 1 and 2)....”*

21) Line 393: “although” to “therefore” Please check the mean value of CB200 in the ES.

: We have revised the mean value of CB200 in ES (line 475 in the revised manuscript).

22) Line 408: “Figure 6b” to “Figure 6a-c”

: Revised (see line 453 in the revised manuscript).

23) Line 433-434: “…the CBMLD values for the three provinces do not show any clear differences…” Please use statistical analysis to show whether there are significant differences among them.

: As shown in Figure 7a, when considering the averages and error ranges, the relationship between the CO budget and CBMLD does not appear to be as pronounced as the relationship between the CO budget and CO200 shown in Figure 7b (attached). We added the correlation coefficients of Figure 7a and b in line 494 and 495 (as below), respectively.

*“...Secondly, despite the fact that the CO budget values in NP and BS are both close to or below zero, there are no distinct differences in CBMLD between the provinces, and no apparent the relationship between CBMLD and the CO budget in the mixed layer, as shown in Figure 7a (R = 0.24). Thirdly, when considering the integrated CO down to 200 meters depth, the CB200, a clear increasing trend is observed in the order of BS, NP, and ES (R = 0.50)....”*

24) Line 477: “Conte et al (2021)” to “Conte et al. (2019)”

: Revised (see line 543 in the revised manuscript).

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