Biogeosciences Discussions

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Comment on bg-2022-201

Anonymous Referee #1

Referee comment on "Carbon monoxide (CO) cycling in the Fram Strait, Arctic Ocean" by Hanna I. Campen et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-201-RC1, 2022

This manuscript by Hanna Campen and coauthors considers the production/loss of carbon monoxide in Arctic seawater under different pH and environmental conditions. Overall, the manuscript was well-presented with strong text and clear figures. The Introduction section was very well-researched and referenced, and the authors thoroughly informed the reader about published relevant work.

However, there were a few things about the manuscript that were not clear to me. These are listed below

1, At one site, the background CO was 50 +/- 9 pmol/L. During the dark incubations, CO concentrations decreased from 50 pmol/L to approx 10 pmol/L. These CO values are incredibly low and must have been scraping detection limit. From memory, a working detection limit is approx 0.1 nmol/L so the authors somehow must have improved upon this.

2, I didnt see any mention of a blank which could have consisted of deionised water (or MilliQ or sterile freshwater). This would have informed the authors about any production of CO from the bottle. I realize the authors tried to avoid the use of plastic which leaches CO but at low picomolar concentrations even trace contamination could have influenced the field data. For your info, even Niskin bottles made of PVC produce CO which is why previous researchers measuring dissolved CO made Niskin bottles made out of titanium (I think it was Craig Taylor and Oliver Zafirou that did this work).

3, It is hard to interpret the data relating to CO production because the light experiments were conducted for 48 h period and therefore cover both light and dark periods. Its not clear to me why the CO production experiments did not run for a 12 h light period.

4, Can you make box and whisker plots (Figure 3) with triplicate values?

5, Did the authors know or test whether the pH values they established for their experiments influence cDOM bioavailability? This is something that could have been done prior to the expedition. If changing the pH does not cause any impact on cDOM bioavailability, then why would any change in production/consumption occur?

6, Do the authors think an acclimation period should have been included for the pH experiments? If the impact of pH on cDOM chemistry and/or microbiology was not immediate, then changes throughout the following 24-48 hrs would be interpretated as biological signal.

7. The small paragraph beginning on Line 205 which compares carbon monoxide with carbonyl sulfide is too speculative and should be removed.

Summary

I support scientific publications where the results do not show significant results, such as this manuscript where two pH manipulations did not impact CO production or oxidation, however, the questions raised above must be fully addressed. I also think the authors should consider whether the Results+Discussion section presents any novel data or perspectives, as its current format seems to be just a comparison withâ□ª previous work published by Huixiang Xie.