

Biogeosciences Discuss., referee comment RC4
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Comment on bg-2022-201

Anonymous Referee #3

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-RC4>, 2022

The following is a review of the manuscript "Carbon monoxide (CO) cycling in the Fram Strait, Arctic Ocean " (Manuscript bg-2022-201) submitted to Biogeosciences by Hanna Campen and coauthors. The manuscript reports results from extensive incubation experiments in the Fram Strait, Arctic Ocean. A great deal of data was obtained and the authors attempt to use it to "better understand the changes of CO in the ocean." I support this manuscript for publication in Biogeosciences if the authors could carefully address the following comments.

Specific issues:

- (1) Part 15 "Both production and consumption of CO will likely increase in the future," This is your result? I did not find this conclusion in the text.
- (2) Part 20 Please delete "see" "e.g."
- (3) Part 65 Please delete "see"
- (4) Part 145 "thermal CO production" Maybe you can provide the site temperature.
- (5) Part 150 I am very skeptical about this result "Since there was no obvious relationship between the timing of the sampling, CO concentrations and preceding light intensities (Fig. 3), this indicates that photochemical CO production did not exceed CO consumption." Unless I am mistaken, you observed that CO concentration increased in incubations in natural sunlight in Fig. 3

(6) Part 155 "The kCO computed from our experiments (Table 1) are comparable to previously published findings from Arctic waters."

Higher or lower? And what is the main reason? Please describe in detail how, where, when to collect samples. Some readers are concerned of the final results and discussion.

Please delete "(Please note that kCO are given as positive values in Xie et al. (2005))."

Please delete "(see Introduction)", and add a reference.

(7) Part 200 "The fact that we still measured oxidation rates in waters with very low CO concentrations might indicate that the dominant community is rather heterotrophic, which in turn could help explaining the poor correlation with Chl a."

Maybe you can provide the bacterioplankton data?

(8) Part 205 please delete "see above"

This last paragraph does not look like a conclusion, maybe you need to simplify this paragraph.

(9) Part 210 please delete "(representing future scenarios of ocean acidification)"

"lower pH" replaced by "ocean acidification"

This sentence looks like illogical " We observed a tight coupling of CO production and consumption. Hence, the produced CO is not necessarily emitted to the atmosphere as the dissolved CO seems to be rapidly consumed before its atmospheric release."

(10) Part 220 (e.g. Lannuzel et al., 2020) (Tuerena et al., 2022) (Cherkasheva et al., 2014)

(10) The conclusion should not contain too many references.

Please delete (as observed in our study)