The interactions among air, water, and ice have long been recognized as critical for accurately estimating gas fluxes in polar oceans. However, measuring CO2 fluxes in natural sea-ice-covered regions remains extremely challenging, particularly due to the logistical difficulties of conducting long-term eddy covariance (EC) observations in such environments. This study presents 17-month EC measurements of air-sea CO2 fluxes in a coastal, ice-covered setting, which is a significant contribution to the field. The dataset clearly captures the temporal variability of CO2 fluxes across multiple timescales. Notably, the identification of CO2 outgassing associated with ice formation is a novel and important finding that could have substantial implications for refining estimates of the polar ocean carbon sink.

The manuscript is well written, the results are clearly presented, and the conclusions are scientifically sound. I believe the paper is suitable for publication after addressing the following minor comments.

PS, the comments from the other reviewer is referred.

Minor comments,

Lines 45–50: I suggest including the recent study by Prytherch and Yelland (2021), which is a dedicated investigation of the influence of sea ice on CO2 exchange: https://doi.org/10.1029/2020GB006633 . While it is cited in line 89, it appears to be missing from the bibliography.

Line 125: It appears that a LI-COR 7500 sensor is installed on the tower, but LICOR7200 does not appear. I see in you 2018 paper, the LI-COR 7500 was used to measure water vapor and CO2 was measured by LICOR7200? Could you clarify here?

Line 180: It would be helpful to provide more explanation of the Kice term, specifically, how it was derived or constrained.

Figure 3: Could you include information about wind direction? It seems that some flux data may be missing due to winds coming from the direction of the island?

Figure 4: The high-frequency time series is 6-hour averaged. Readers may be interested in the extent to which the observed variability is influenced by EC uncertainty. Could you provide at least a simple estimate of the uncertainty magnitude, include that value in the figure caption, and briefly discuss it in the main text?

Figure 5: I agree with the other reviewer that the possibility of pCO_2ice being negative should be explained. I suspect the derived values may be sensitive to the estimation of Kice. While some discussion is included later in the manuscript, it would be helpful to provide an earlier explanation, perhaps around line 185.

Line 275: You mention that camera images were collected, but none are shown in the paper, which is a shame. Would it be possible to include several representative images from different stages of the observation period? These could be placed alongside Table 1 or included in the supplementary material.

Line 333: For your reference, we have conducted a related study using eddy covariance and pCO2w measurements in an ice melt region, which indicates substantial CO_2 uptake: Dong et al. (2021), Geophysical Research Letters, https://doi.org/10.1029/2021GL095266 .

Line 370: The first sentence in this paragraph reads awkwardly to me. Please consider rephrasing for improved clarity and flow.

Line 533: The square brackets around the reference should be removed to maintain consistency with the formatting style.

Line 569: The abbreviation “EC” appears here without prior definition.

Conclusions: The comparison with Sims et al. (2023) is valuable, but might be more impactful if introduced earlier in the discussion section. As currently presented, it reads more like a discussion point than a concluding remark.

Final suggestion: It may strengthen the conclusions if you emphasize that concurrent measurements of pCO_2w would provide more robust support for some of the interpretations presented in this study.

-Yuanxu Dong