Title: Interannual and seasonal variability of the air-sea CO2 exchange at Utö in the coastal region of the Baltic Sea
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MS No.: egusphere-2024-628
MS type: Research article "Interannual and seasonal variability of the air-sea CO2 exchange at Utö in the coastal region of the Baltic Sea"

Hokanen et al. present an interesting article that seeks to quantify CO2 flux between the coastal Baltic Sea waters and the overlying atmosphere across 5 years of observation. Using direct Eddy Covariance measurements across the sea surface from a land-based tower, interspersed with pCO2 gradient measurements, that were incorporated into a wind-driven CO2 flux parameterized model, the investigators were able to estimate annual CO2 flux estimates and report both seasonal flux patterns and interannual variability. Ultimately, they are able to use 5 years of data to estimate CO2 budget for the Archipelago Sea where Utö is located. Importantly, by taking advantage of the long-term environmental monitoring programs and assets at Utö (ICOS Atmospheric station, Marine Research Station, etc.), the authors were able to record from a suite of co-located instruments to 1) refine their pCO2 gradient and flux measurements, 2) suggest plausible forcing factors to explain seasonal and inter-annual variation in pCO2 in water and CO2 flux direction and magnitude. I believe this work should make an important contribution to quantifying CO2 flux in this region of the Baltic Sea and increase overall knowledge, dynamics, and forcing that take place in nearshore ocean ecosystems. The authors address the possible actions of biological, chemical, and physical factors to explain patterns of nutrient input and uptake, suggesting land-sea interaction and lateral transport of nutrients, combined with temperature and solar input as driving seasonal phytoplankton dynamics as a key driver of seasonal CO2 drawdown in the water and wind as especially important in winter months for enhancing gas exchange.

<u>Substantive Concerns</u>
1) Although details of CO2 flux measurements and modeling were detailed in Appendix A (Lines 430-478), I was surprised that some of this information was not included in the main manuscript, e.g., Methods and Results. Perhaps I am missing or misinterpreting (forgive me if that is the case), but it seems that the opportunity to use the direct comparison of EC measurements and the parameterized air-sea CO2 flux results to help validate the flux model is being lost. Although parameterized air-sea CO2 flux data are used for gap filling when accurate EC was disrupted by ships or otherwise not possible, is there a reason not to compare time periods when both EC and pCO2 gradients were conducted to ground-truth model results? This seems especially important since EC, the direct measurement was only used 26% of the time (Line 161). In some sense, it seems that the EC was actually filling gaps for flux model results. Can you comment on this?

2) Figure 9 contains especially important data describing 5 year's of CO2 fluxes observed at Utö, as measured by EC and estimated by parameterized models according to EQ 1 (line

44). However, I think it would be helpful to identify and differentiate EC values from the modeled value in Figure 9.

3) 2.2 Flow-through system (Lines 104-111). Is it possible to report the diameter of the 250m pipe that connects the submerged borehole pump (with inlet at 4.5m depth) to the manifold that feeds the instruments in the station? Although a flow rate of 55 l min-1 is reported, it is not possible to determine the residence time of the water inside the transfer pipe. Additionally, although there was frequent and automatic cleaning of the sensors and instrument hoses (Lines 110-111) there is no information on how frequently 250m pipe is and whether biofouling inside this pipe is likely to affect pCO2 value measured at equilibrator inside the marine station. Lines 131-134: The authors do cite information about potential magnitude of possible pipe effect "The effect of the long inlet tube on the pCO2 measurement at Utö has also been verified to be small (Honkanen et al. 2021), but a brief summary statement about this potential measurement error would be helpful.

4) Figure A1 (page 31). Given the centrality of the gas transfer velocity estimates to CO2 flux, which appear to have been relied on for 74% of observations, I am surprised that this key parameterization result is buried in the Appendix. Given the CO2 flux is the basis for generation of annual CO2 budgets, I wonder if the authors might consider incorporating this key finding and a discussion of it in the main text? If manuscript space/length is an issue, some of the environmental measurement figures (Figs 3-10) could be moved to Supporting Materials, but discussed and referenced in the main body of the text.

Minor suggested edits/comments

Abstract (line 40), please define FCO2 as CO2 flux to avoid confusion with fugacity.

It would be helpful to include the salinity range of the Archipelago Sea in description of study site. General comments about oceanographic and possibly land-sea interactions of the Gulf of Finland and Gulf of Bothnia with Archipelago Sea would be helpful to readers less familiar with region.

Figures

Some figures are positioned in the main text (Figs. 1 and 2) but the other figs appear after the text and Appendix section on pages 23-28, these should be inserted at appropriate locations within the main text of the manuscript.

Figure 2 indicates that pH was one of the measurements made with the flow-through systems, but investigators did not seem to mention whether pH tracked pCO2 and would be expected.

Figure 4 is missing units on the left side vertical axis.

Figure 8 is missing vertical axis label.

Line 151 and elsewhere, (m.a.s.l.), please define acronym (meters above surface level?)

Line 155, “The measured EC fluxes were corrected for the flux loss occurring in the system.” Please elaborate.

Lines 207-209: “for the most part of the year” - edit to “for most of the year”.
“The year of 2018 was the warmest amongst the studied ones” – edit to “. . . warmest of those studied.”

Discussion
Line 323: “as a source” – edit to “as a net source”

Lines 325-339: Authors might consider referring to allocthonous vs. autocthonous carbon. This is simply an observation, but in shallow well mixed estuaries, the benthos can be less disconnected from surface waters, with respect to its ability to mineralize organic carbon than it is in this system. In the current study location, strong thermoclines prevent mixing such that benthic respiration appears to have little or no effect compared with the photosynthetic drawdown of CO2 due to summer phytoplankton blooms.

Lines 358-359: “The seawater pCO2 in the summer 2017 remained extraordinarily high, compared to other years.” Think about rephrasing as “In comparison with other years, the seawater pCO2 in the summer 2017 remained unusually high and values nearly exclusively above the multi-year mean.”

Line 360: “. . . the sink fluxes were accordingly lower than other years.” Consider rephrasing to “. . . the sink fluxes were accordingly weaker than other years.”

Lines 363- 364: “The mixing of CO2 rich to the surface may have diluted the drawdown surface, thus decreasing the negative fluxes in summer.” Consider changing “decreasing” to “lessening”.

Line 366: “Due to this, the sea was able to release more carbon dioxide . . . .” Consider rephrasing to “For this reason, we believe the sea was able to release more carbon dioxide . . . .”