We sincerely thank Dr. Brent Else for his positive feedback and thoughtful, constructive comments, which have greatly helped us improve the quality and clarity of the manuscript. All coauthors have carefully considered the reviewer's suggestions and worked together to revise the manuscript accordingly. In the responses below, the reviewer's comments are shown in **black**, our replies in **blue**, and the corresponding revisions in the manuscript in **orange**.

I am pleased to present this review of "Mechanisms of air-sea CO2 exchange in the central Baltic Sea" by Yuanxu Dong and co-authors. The paper presents a well-designed and well-executed field study of gas exchange in an interesting coastal region. Very few similar studies have been conducted, and most of those have occurred (for good reasons) in the open ocean. By focusing this study in the Baltic Sea, important new insights and clarifications about how we should be determining gas exchange in coastal waters are revealed. I thought the manuscript was technically solid. The field methods were well described, and utilized the current state-of-the-art. The choices of data analysis techniques were good, and reasonings used to interpret key factors like the impact of fetch-restricted wave fields and surfactant effects were well justified and acknowledged relevant uncertainties. I really don't have much negative to say about this paper... I think it will be an important contribution to the field, and I support its publication.

Thank you very much for the encouraging feedback and for recognizing the value of our study. We hope that our work will help draw attention within the community to the importance of conducting more gas exchange experiments in coastal environments.

I have a few minor comments that I think the authors should consider addressing before publication:

Line 95: Wave parameters extracted from the ERA5 reanalysis product are an important component of this paper. I think the authors should explain a bit more the uncertainties in these products, and how that may affect their analyses.

This is a good point. In the later stage of the original text, we have included the uncertainty analysis of ERA5 and how it impacts our quantification of the suppression fraction in the "Uncertainty analysis" section. Below is the relevant text, and hope this solves the reviewer's concern.

The suppression analysis uses H_s data derived from ERA5 reanalysis, which likely carry an uncertainty of about 30% in the Baltic Sea (Giudici et al., 2023). Consequently, the uncertainty in the H_s-related suppression estimate is ~1.6 cm hr^{-1} (i.e., $\sqrt{(4.0 \times 25\%)^2 + (4.0 \times 30\%)^2}$ cm hr^{-1}).

Line 111: The authors choose to use a 10-minute flux averaging interval, but do not discuss the potential ramifications of this. I would assume it would result in some loss of low-frequency flux. Can this choice be better justified?

Thank you for raising this important point.

For flux calculations, we generally used 20-30 min intervals for the open ocean and 10 min for coastal waters, as coastal environments are more dynamically variable due to their complexity. A shorter time interval may better fit the stationarity requirement, but without losing a significant low-frequency signal. A classical coastal EC study by Vickers & Mahrt (1997) suggests that the wind stress calculated with 5 min averaged over all stationary records captures 95% of the stress values calculated using a 60 min averaging timescale. The stress is not sensitive to the local averaging timescale until the interval is less than 3 min. The 10 min averaging interval we used is also consistent with a previous coastal study (Yang et al., 2016)

Below is a normalized Ogive curve for CO_2 flux from CenBASE, calculated with a 20-minute averaging interval. Low-frequency contributions are negligible, with the majority of flux variance explained by frequencies above 10^{-2} Hz (~2-minute timescale). This characteristic was confirmed across all analyzed time periods.

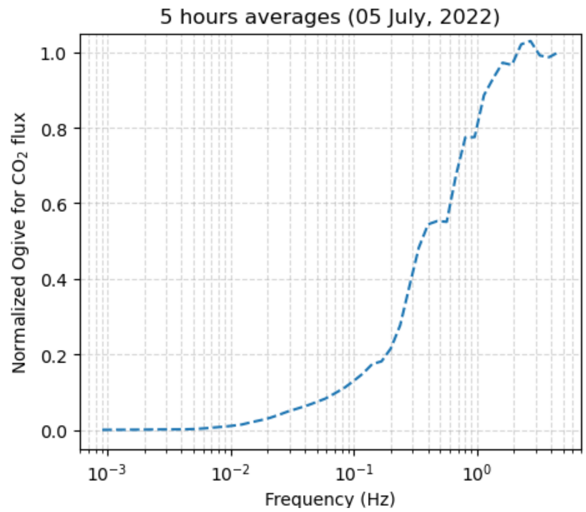

Figure 1 Normalized Ogive curve for EC air-sea CO_2 flux. The results shown here represent an 5-hour average of the 20-min fluxes on 5 July, 2022 during CenBASE.

Vickers, D., & Mahrt, L. (1997). Quality Control and Flux Sampling Problems for Tower and Aircraft Data. *J. Atmos. Oceanic Technol.,* 14, 512–526, https://doi.org/10.1175/1520-0426(1997)014<0512:QCAFSP>2.0.CO;2.

Yang, M., Bell, T. G., Hopkins, F. E., Kitidis, V., Cazenave, P. W., Nightingale, P. D., Yelland, M. J., Pascal, R. W., Prytherch, J., Brooks, I. M., & Smyth, T. J. (2016). Air-sea fluxes of CO2 and CH4 from the penlee point atmospheric observatory on the south-west coast of the UK. *Atmospheric Chemistry and Physics*, 16(9), 5745–5761. https://doi.org/10.5194/acp-16-5745-2016

Line 190: is this u* value actually observed? Just previously, and after this, u* from COARE models are discussed. Can you clarify in the text whether this u* is coming from eddy covariance, or the COARE model?

Sorry for the confusion. Yes, here is the u_* from EC observation. We clarify this by rephrasing this sentence as:

Observed values of u_* from EC momentum fluxes during CenBASE were 10% higher than modelled open ocean u_* at the same wind speeds (Fig. 3A), likely reflecting fetch-related differences.

Overall, I found it a bit strange that the dual-tracer results were included in this paper. They are not discussed at great length, and apparently there is another manuscript in review (or perhaps being drafted) that will deal with this in greater detail. I think it is worth keeping the measurements in, but maybe the authors could find a place to more explicitly explain why it is important to have this data in this study.

This is a good point. Yes, the dual-tracer dataset alone has been analyzed in a separate, submitted manuscript, with a preprint forthcoming. There is ongoing debate regarding the suitability of EC for quantifying air-sea gas exchange. Simultaneous application of EC and the dual-tracer (DT) method enables a direct intercomparison of these two approaches. Our results demonstrate that the two methods yield comparable gas transfer velocities under low-moderate wind speeds. However, the EC technique operates on a shorter timescale and provides a wider wind range constraints on the gas transfer velocity. To emphasize this advantage, we have added the following statement to the first paragraph of Section 3.3 ("Gas transfer velocities from EC and DT"):

Simultaneous gas transfer velocity observations by the EC technique and the dual-tracer method provide a direct comparison of the results from these two different approaches.

In the discussion section, the original manuscript also includes a statement:

The K_{660} derived from both techniques agrees well, confirming the reliability of both methods for gas transfer velocity observations.

Line 218: Appendix A2 is about surfactants, not dual-tracers.

Good spot. Thank you. Revised.

K_{660} from the DT experiment is summarized in Appendix A1.

Line 224 and Line 236: qualitative statements about agreement between DT and EC fluxes are made... I find this a bit weak; there are clearly some DT points that fall below or above the EC fluxes. Can these statements be made a bit more quantitatively?

Good suggestion. From Fig. 4, looks like the several DT points at low wind speed are slightly lower than the EC results, but well within the uncertainty range. For the point at U_{10} ~7 m s^{-1}, DT is higher than the EC and the one at U_{10} ~9 m s^{-1} is lower than the EC, but considering the uncertainty, they still agree. In general, the DT data is concentrated in the wind speed range of 5-

9 m s^{-1} and on average gives 10.3 cm hr^{-1} and the CenBASE EC-based parameterisation for the equivalent DT wind speed gives K_{660} of 11.1 cm hr^{-1}. Therefore, the EC result only slightly (7.8%) differs from the DT observations. We took the reviewer's suggestion and gave a quantitative comparison.

Within this wind range, DT-and EC-derived K_{660} values are in good agreement, with the former being only slightly (~8%) lower than the latter (Fig. 4).

Line 236 is about the comparison of the observed DT results in the Baltic Sea and the modelled DT results based on the open ocean data. Similarly, we added a quantitative comparison.

DT-derived K_{660} values during CenBASE also generally agree with the open-ocean DT-based parameterisation of Ho et al. (2006) under equivalent wind speeds (orange dashed line in Fig. 4A), with the former on average being only ~7% lower than the latter.

Line 273: I was confused by the statement "applying equation 3 to the equivalent wind speeds"... Equation 3 only includes u* and Hs as variables.

Sorry for the confusion. As also noted by the first reviewer, here we rephrased the sentences:

The observed EC K_{660} during CenBASE was on average 14.9 cm hr^{-1}. To compare this value with open-ocean conditions at equivalent wind speeds, we apply the wind-speed observations from the CenBASE cruise (i.e., wind speed values shown in Fig. 4A) to Equation 3 to estimate open-ocean K_{660}, yielding average values of K_{i660} = 15.1 cm hr^{-1}, K_{b660} = 7.0 cm hr^{-1}, and K_{660} = 22.1 cm hr^{-1}. This means that the observed K_{660} during CenBASE was 33% (7.2 cm hr^{-1}) lower than the open-ocean K_{660} estimate.

Section 3.6: The origin of some of the data is a bit unclear... in particular the source of the fCO2 data is not well explained.

Good point. We now added the source of the fCO_{2w} data in the first paragraph of this section:

To upscale these results, we adopt a fCO_{2w} product from Bittig et al. (2024) to examine how fetch and surfactants shape the climatological CO_2 flux of the Baltic Sea. This climatological fCO_{2w} product is derived by combing observations and model patterns.

Figure 7: Why not show us the computed flux? A lot of the text (eg Line 430-434) describes patterns in the computed flux, but this is not presented.

Thank you for this question. The atmospheric fCO_2 (fCO_{2a}) over the Baltic Sea is not provided by the dataset used for seawater fCO_{2w} (Bittig et al., 2024). Flux calculation also requires additional inputs such as sea surface temperature, salinity, and sea ice concentration. Our study does not aim to provide a quantitative flux analysis, but rather to examine how the new parameterization could affect the spatiotemporal patterns of air-sea CO_2 flux. We therefore believe the current analysis and Figure 7 sufficiently support our conclusions. We hope this clarification addresses the reviewer's concern.

Line 490: There is a reference to a "bubble buoy" which I don't think is described anywhere else in the manuscript? What is this? Is it important to keep in?

A bubble (spar) buoy was deployed, but unfortunately the bubble measurements were not working, but the bubble buoy also integrated temperature, salinity and dissolved oxygen sensors (shown in Fig. A7). Here we just want to highlight the bubble-mediated transfer study requires real bubble dynamic measurements and the bubble buoy is useful to do that. Since the data from the bubble buoy was used, we include a short description in the section 2.3 (**Auxiliary observations**) to raise the reader's awareness that a buoy was deployed during the cruise:

A spar buoy equipped with cameras, temperature, salinity, and dissolved oxygen sensors was deployed at several stations to characterize upper-ocean bubble and water column dynamics.