Anonymous Referee #1

The manuscript by Dobashi et al. reports on gas exchange in the Baltic Sea. The authors report that wind-based parameterizations for the open ocean are also applicable to the inland sea that is the Baltic Sea. Since this is not always clear in scientific literature, and the parameterizations themselves contain uncertainties due to the complexity of gas exchange, these types of studies are certainly relevant. To capture the complexity of gas exchange, the authors measured additional important parameters: the concentration of surface-active substances at the sea surface, trubulence with ADCPs and the properties of air bubbles in the upper water column; the former influences gas exchange in relatively calm seas, and air bubbles rising in the water column during rough seas with breaking waves.

- Thank you for your comments. We addressed your feedback as follows.

Main comments:

Line 15: Eddy covariance was also used, correct?

Since two techniques were used here to measure k—dual tracer and eddy covariance—the results from both should be compared directly, rather than just the coefficient of variation of the root mean square error.

- Eddy covariance-derived *k* has been presented in Dong et al. (2026b). We have added a brief comparison with the dual tracer-derived *k* in section 3.2 and introduced the paper as follows.

  The measured *k*(600) by $^3He/SF_6$ tracer technique was 10.8 ± 2.9 cm h^{-1} for the whole period (Fig. 1 and Table 3). The observed *k*(600) was similar to offshore regions at the same wind speed. <u>During the experiment, concurrent measurements of *k*(600) was made using closed-path eddy covariance. *k*(600) from closed-path eddy covariance was 11.0 ± 4.4 cm h^{-1} for the corresponding period, which is similar to $^3He/SF_6$ *k*(600). A detailed explanation and comparison between eddy covariance and $^3He/SF_6$ *k*(600) are presented in Dong et al. (2026b).</u>

The locations of the ADCP measurements are shown graphically in the figure 2, but the ADCP data itself is not presented or discussed. What effect did currents and TKE have on k, as well as on other parameters such as air bubbles and surfactants? These relationships should be illustrated, as they could provide a very good representation of the complexity of gas exchange.

- We have horizontal ADCP data, but not vertical velocity. Therefore, it is difficult for us to calculate the TKE. In this study, ADCP was used to estimate tracer movement, as explained in section 2.1.

- Since the experiment site is deep (>200 m), bottom-generated turbulence will not be a factor. We added the information on depth and discussed the effect of bottom-generated turbulence as follows.

  section 3.1:
  <u>The depth of the experiment site was 201 ± 42 m.</u>

  section 4.1:
  <u>The depth is as deep as ~200 m, so the effect of the bottom-generated turbulence should be negligible.</u>

Why was the camera used to detect air bubbles on only one day for a limited time? Technical issues? It should be briefly described for clarity, as reader may And wouldn't one expect breaking waves—and thus air bubbles—at a wind speed of 6-7 m/s?

- Yes, it was used only for a limited period due to technical issues, as explained in 5th paragraph in section 2.3:

  The buoy equipped with a bubble camera at a nominal depth of 80 cm was deployed on YD 185 from 09:20 to 13:55. The bubble camera is a custom-built instrument that captures high-resolution images illuminated by a strobing light sheet, and can detect bubbles with radii ranging from 0.02 to 4 mm (Al-Lashi et al., 2017). The instrument malfunctioned after this deployment, unfortunately, preventing further deployments.

- Even when the bubble camera was not in the water, whitecaps were not observed on the ocean surface visually. However, we would not expect to see whitecaps even at higher wind speeds, because unless there is significant surfactant activity, the bubbles will coalesce very quickly into larger bubbles, rise rapidly to the surface, and then burst almost immediately under lower salinity conditions. Although we did not observe whitecaps, there may have been some bubble formation, but those bubbles would have formed and been destroyed very quickly. We added the following sentences in sections 3.1 and 4.1:

  section 3.1:

No whitecaps at the sea surface were observed visually during the experiment.

section 4.1:
Salinity was constantly 7.3 throughout the measurement period. At this salinity, the number of bubble coalescence events is expected to be much higher than in the open ocean where salinity is about 35 (Firouzi et al., 2015). The consequence is that in fresher water, bubbles will coalesce and rapidly rise to the surface, reducing both their contact time with the water and their surface area. This suggests that salinity during our observation period may have suppressed bubble-mediated gas exchange relative to the open ocean, even if the same number of bubbles were created by breaking waves. The bubble camera deployed at a wind speed of ~6 m s^{-1} did not detect bubbles, and whitecaps were not observed even at high wind speeds. This suggests the number of bubbles was small and that air-sea gas exchange via bubbles was not significant. Limited fetch may also have inhibited wave development, leading to a less developed wave field than in the open ocean and resulting in reduced breaking waves and bubble production. Indeed, Dong et al. (2026b) concluded that bubble-mediated air-sea gas exchange was not significant (~20% of total gas exchange) based on their analysis of closed-path eddy covariance data during CenBASE.

Line 112: Sampling surface films is problematic using various techniques due to the varying but critical film thicknesses. Sampling from large ships, even from the bow, is also problematic because of the ship's pitching in rough seas; and I suspect that sampling was carried out from the ship precisely under those conditions.

- There were no simultaneous measurements of glass plate and Garrett screen, so methodological effects cannot be fully disentangled from environmental variability. Nevertheless, all sampling methods followed established community standards, and it is therefore unlikely that methodological differences alone can account for the observed variability. We have added the following sentence in section 2.3.

  These two sampling methods were not conducted simultaneously; therefore, methodological differences cannot be separated from environmental variability. However, since both methods are based on community standards, it is unlikely that the observed variability is entirely due to methodological differences (Karnatz et al. 2026).

Line 160: When calculated according to TEOS-10, the salinity must be expressed in units (i.e., g/kg). Even when calculated according to the older 1978 standard, salinity values should be expressed in PSU.

- UNESCO specifies that salinity has no units, since it is a ratio of conductivities. The reference is below.
  UNESCO. The Practical Salinity Scale 1978 and the International Equation of State of Seawater 1980: Tenth Report of the Joint Panel on Oceanographic Tables and Standards. UNESCO, 1981. UNESCO Technical Papers in Marine Science No. 36.

Line 163: Were surfactants concentration measured during this study or taken from Karnatz et al (2025). The authors need to explain why concentration are in a rather narrow range despite wide wind speed range

- Surfactant was measured during this experiment and published in Karnatz et al (2026). We are showing the observations, and there's no reason to expect a relationship between the wind speed and surfactant concentration.

In Section 3.2, the results are already being compared with those of other studies and should therefore be moved to the discussion section. The authors should rather present the results in detail, e.g., a comparison of dual tracer and eddy covariance methods. I find a direct comparison to be very relevant for this and future studies.

- OK. We have deleted the phrases comparing different parameterizations, since it is already discussed in the discussion section. We added a brief comparison of $^3He/SF_6$ *k* and eddy covariance *k* in section 3.2 as described above.

Line 236: What is number of observation?

- It is 15. We added the value.

Line 237/278: The authors describe the concentration of surfactants as elevated. What is the basis for this assessment? Gas exchange is reduced at significantly higher concentrations of surfactants, e.g., in the presence of surface films (slicks). There are references in the literature supporting this. For example, Mustaffa et al. present high concentrations, and line 250 is somewhat misleading ignoring extreme - but not rare - cases

- The basis of this assessment is that the surfactant activity during this experiment was higher than the typical offshore value. We explicitly reported the typical surfactant value when citing Mustaffa et al. (2020) as follows.

Surfactant activity during CenBASE was higher than typical open ocean (up to 0.25 mg-eq L^{-1}; Fig. 5 in Mustaffa et al., 2020), potentially suppressing *k*.

Line 244: That didn't surprise me, since the study was based on a single water mass (due to the fact that it was following the gas plume). The authors should address this in their discussion, as gradients in biological activity—or upwelling zones, rainfall, and other factors—would play a role in transects between coastal waters and the open ocean. So it should read "in this study" and not "in this region".

- It shouldn't have anything to do with the water mass being Lagrangian. We think "in this region" is correct.

Line 264: The authors should briefly discuss the fundamental differences between the parameterizations from both Wanninkhof (1992) and Kuss et al. (2004), compared to Wanninkhof et al. (2009) and Takahashi et al. (2009). That may revela a more mechanistic understanding of biases.

- In Table 1, we added information on how they were derived. The concern in Kuss et al. (2004) is already discussed in section 4.2 as follows.

  One of the parameterizations derived for the Baltic Sea was from waterside CO_2 mass balances (Kuss et al., 2004). To determine air-sea gas flux using this method, other variables, such as advection and biological activity, must be accurately accounted for. Errors in these calculations propagate to air-sea gas flux, *k*(600), and its parameterizations. Kuss et al. (2004) studied three periods, with estimated errors in CO_2 flux calculations of 14%, 25%, and 83%, respectively. In particular, the large error in the third period is expected to have a significant impact on the calculation of *k*(600).

Overall, the manuscript is relevant but requires major revisions.