The manuscript entitled "Mechanisms of air-sea CO_2 exchange in the central Baltic Sea" by Yuanxu Dong et al. describes the results of a field campaign to measure CO_2 fluxes in the Baltic Sea, with a specific focus on additional physical processes (beyond wind speed) influencing gas exchange. The EC measurements appear to have been collected with the proper technical methods. The manuscript is well written and the figures clearly present the results. While there are certain built-in limitations from the relatively short period of data collection (e.g., limited range of SA and wind speeds), the study provides a worthwhile contribution to the field by testing key advancements to CO_2 flux estimation. The multivariate (SA, Hs, ustar) parameterization for k660 provides a straightforward path for incorporating additional physics and provides a useful starting point for future studies. I recommend the paper for publication after some minor comments are addressed.

Comments:

Line 26: What does "40% stronger seasonal cycle of CO_2 flux" mean precisely? Larger difference between annual high and low values? Larger magnitude fluxes overall?

Line 54: "affected by many factors near-surface processes" is redundant.

Line 72-73: awkward comma splice

Line 108: change "mole density" to "molar density"

Line 125: It's not necessary, but it could help future groups, if you mention that this configuration (i.e., 3/8" ID, 33.2 lpm flow rate) results in turbulent flow within the tube.

Line 129: The Edson et al. (1998) paper did not have a procedure to correct for misalignment between the anemometer and the motion sensors. Miller et al. (2008; https://doi.org/10.1175/2008jtecho547.1) added an alignment transformation matrix. Did you use this?

Line 152: This is confusing: "according to the open ocean EC cruise tracks (see Yang et al., 2022)". Do you simply mean you extracted ERA5 wave parameters according to the same method as Yang et al. (2022)? Based on a couple sentences down it appears you perform an analysis using previous EC cruises? If so, it hasn't been introduced yet. That's why the wording on Line 152 is confusing.

Line 186: Add "coming" or "being obtained" (or similar) before "from" in "leading to most valid EC measurements from outside this period"

Line 197: The sentence that starts with "This supports…" needs work. As it's written, it needs and object after "supports" (e.g., "idea"). But the sentence is wordy. Here's a suggested modification: "This suggests that the COARE model remains applicable in fetch-limited marine environments when wave information is included, despite being developed primarily from open-ocean observations."

Line 218: DT Experiment is summarized in Appendix 1 (not 2).

Line 231/233: "parameterization" and "parameterisation" are used in the same caption (and throughout the manuscript). Choose one for consistency.

Line 247: As this is currently written it sounds only theoretical. Might be worth citing Yang et al. (2022) here, as this was empirically found (see the last sentence of the article).

Line 266: It's not clear to me what you did here. "Following this separation framework" sounds like you ran the same machine-learning analysis as Yang et al. (2024) to get the coefficients in Eq. 3. Did you do that? If not, where do they come from? I don't see that exact equation in Yang et al. (2024).

Line 273: I suggest adding a little more explanation of what these modeled values represent. It was not clear to me initially whether the modeled values in this paragraph were the model applied to the Yang 2024 open ocean data or whether they were the model applied to Baltic cruise data that had similar wind speeds to the open ocean data, but with Hs for the Baltic Sea (which you do show later). It became clear later, but would help to clarify it here. How did you select for similar wind speeds? Was it simply all open ocean data points with wind speeds below 12 m/s? Or did you match wind speed distributions?

Line 274: "yields on average values" to "yields average values"

Line 290: Appears to be missing "and the CenBASE cruise track"

Figure 5B: If I understood correctly, the solid black and blue are total k660 from the Yang et al. (2024) equation and the dashed black and red isolate only the kb component. If so, the notation in the legend does not clearly convey that. Specifically, it's unclear what "Hs_" means. It's not immediately intuitive that it means total k660. Also, since the solid blue and the dashed red lines are related in the same way as the solid and dashed black lines, I'd suggest using the same color for them.

Table 1: It would also help to be more explicit in the table what is modeled and what is measured. For example, adding "EC" somewhere on the "Baltic Sea (CenBASE)" line and "model" somewhere on the "open ocean" line. Separately, "Gas Transfer velocity (cm hr-1)" heading is over the wrong column. Maybe it could be placed over the k columns?

Figure 6: Yang21 isn't cited in the caption, but the rest of the studies are.

Line 356: Because the SA suppression is inferred, there is no way to test this. But do you think that the two *sf* corrections are independent? Would you expect an additional interaction term (e.g., coeff * SA * ustar) to modulate the resulting *sf*? Could that explain the different slopes from different studies in Fig 6b (i.e., they have different SA concentrations and therefore have different *sf* vs ustar relationships)?

Line 374: It's not clear how the 20% uncertainty was calculated. The word "assign" sounds like it was a rough estimate (which would be fine). But if it was more quantitative than that, it would be worth elaborating.

Line 377: "ERA" to "ERA5"

Line 383: 4.5 cm/hr here, 4.6 cm/hr in the table. Surely just a rounding error, but worth making them consistent.

Line 395: Abbreviation "Chl-a" is defined twice in this paragraph. Also, the abbreviation is not used consistently after defining.

Line 418: "especially stronger" to "especially strong"

Line 422: "compare" to "compared"

Reviewer: Brian Butterworth