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Comment on bg-2022-82

Anonymous Referee #2

Referee comment on "On physical mechanisms enhancing air–sea CO_2 exchange" by Lucía Gutiérrez-Loza et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-82-RC2, 2022

This manuscript by Gutiérrez-Loza et al. presents a new dataset of eddy correlation air-sea CO_2 flux measurements that is in itself a valuable contribution to the field. Analysis of the dataset has the potential to improve our understanding of gas exchange processes and the analysis done here does reveal some new insights particularly regarding the importance of water-side controls on the gas exchange rate and the influence of processes that cannot be parameterised as a function of wind speed. It is generally well written, interesting and easy to follow. I encourage the authors to work further on the dataset to see it through to publication.

However, there are a few major issues that would be essential to address first:

- How representative are the results here in a global context? The title leads one to expect that these will be universally applicable insights, but this does not seem to be the case. Indeed the most confidence the authors were able to express in the wider applicability of their results was 'the results presented here are *most probably relevant* for other marginal seas and coastal areas' (line 375, emphasis mine). Line 314 also suggests this work may be only relevant to the Baltic. The title should be further qualified '… in the Baltic Sea' or similar unless the authors can be sure that their results are more widely applicable.
- One of the key motivations for this study is reducing uncertainties in gas exchange calculations (e.g. lines 22 – 24) yet there is no meaningful uncertainty analysis of the results of this study. I could not even see uncertainty estimates for the raw measurements that underly the new dataset being presented and there was no propagation of uncertainty through to the final results. This is essential especially if the results are to be compared with previous work or other approaches, else you cannot be sure if the results are actually consistent or not.
- Many of the relationships described in section 3.2 and its subsections were not convincing based on the figures. For example on line 225 'kr showed a clear relationship with significant wave height … (Fig 6a)' but if we look at Fig 6a, then I see more clearly the colours getting 'higher' in more vertical bands towards the right (ie correlating with U10) rather than vertical bands towards the top. Same applies for line 239 comment about mixed layer depth. But in general I think that the format of Figs

6-10 makes it very hard to see the correlations described anyway: you are trying to eyeball the angle at which changes in colours occur and there are so many datapoints that they all block each other (e.g. fig 8b has a section in the middle that looks all blue i.e. low values, but with hints of higher pink points that can just be seen around the edges – very hard to interpret). My suggestion would be to replot these figures with kr on the x-axis and the variable of interest (currently the colours) on the y-axis. The points could then be coloured by U10 or something else. This would be a much more clear and convincing way to see correlations. Furthermore, related to point (2) above, there needs to be statistics for the correlations that you report.

- Many parts of the dataset are excluded and the impact of this on the results and their wider applicability is not much discussed. We have low wind speeds on line 139, low fluxes on 154, high humidity on line 157, stratified conditions on line 169, and unexplained low k660 values on line 174. Maybe it's valid to not include these in the analysis, but we really need an accompanying robust discussion of how often those conditions occur in the real world and what that means for the gas exchange rate.
- Despite high variability and physical/biogeochemical heterogeneity being an important motivator of the study, some key properties were assumed to be uniform (salinity on line 116, somewhat cryptic 'biogeochemical water properties' on line 101). This may be fine but this assumption is not critically assessed. One should be able to quantify a maximum effect size for how important ignoring this variability could be.

Other minor comments:

46 how deep is 'the upper layer of the ocean'

Fig 1 caption 'see text for details' of open sea sector – please give section number and repeat values here

Fig 2 Very unclear to me what this figure shows. What are X and Y on the axes? What does 'footprint' mean?

161 'in these region' => 'in this region'

175 'more detail analysis' => 'more detailed analysis'

183 mixed up > and < symbols for intermediate conditions

Section 3.1 worth pointing out that the seasonal cycle of pCO_2w looks to be biologically controlled rather than temperature controlled in this part of the world – any impact on wider applicability? See e.g. analysis of Takahashi et al (2009, DSR2)

Fig 3 State which method of calculating air-sea CO_2 fluxes is used here

228 please explain briefly why the wave age suggests local generation of waves (for the non expert)

Figs 4 & 5 suggest to make points smaller and semitransparent so that structure within the big grey overlapping blob can be seen

300 there are studies that compare these other parameterisations, have you looked at those to put your comparison in more context?

305-306 not convinced that this long-term average being correct but short term was really 'shown' here. Needs statistics and more rigorous definitions (what is long term? How much uncertainty is there by ignoring the water side effects?)

394 define 'adequately' (adequate for what purpose?)