Comments on **egusphere-2025-5068**

Summary

The manuscript by Geilfus and colleagues presents the results of an extensive campaign devoted to investigate the source/sink dynamics of the major greenhouse gases (GHG) CO2, CH4 and N2O along a temperate estuarine system. The authors aimed to quantify GHG fluxes across the sea-air interface under a salinity gradient, as well as to compare those fluxes between sites with contrasting circulation/atmospheric forcing features. Despite inherent heterogeneity in GHG distribution and sea-air fluxes, the main finding is that while CH4 and N2O mostly offset CO2 uptake in the estuary, N2O might also counteract CH4 emissions.

General assessment

Detailed studies of GHG dynamics across the land-ocean continuum are, despite the admittedly increasing attention of the topic over the last decade, still scarce. From that perspective, there is a great value in having such a comprehensive survey of distribution and air-sea fluxes of all three gases simultaneously. An important aspect shown by these measurements/analyses, is that establishing the overall radiative balance of coastal ecosystems is challenging, as departures from the classical view (CO2 uptake coupled to offset from non-CO2 gases) is not necessarily a given, when local-scale environmental variability is considered. While the extent at which, for instance, N2O might offset CH4 emissions in this and other systems, as well as its temporal variability over longer time scales remains to be seen, I do think that it is important to show the complex interactions as it has been done here. Overall, the manuscript is well written, the structure is mostly clear and the figures are of good quality.

That being said, I spotted a few issues which I would invite the authors to address. The perhaps major issue I see, is the difficulty of combining two scales of variability, namely, the spatial gradient due to salinity, and the variability associated to exposed and sheltered areas. While I appreciate that it is challenging to put these different aspects together, I think combining them makes the manuscript less clear than it could be based on the observations. On the one hand, the data presented in this manuscript shows a very clear salinity-dominated gradient which is reflected in other chemical parameters measured along with the gases. On the other, separating areas in exposed and sheltered (which partially overlap with the salinity gradient), adds a complexity that is not necessarily relatable to the fresh-seawater continuum. Another issue I see is that the definitions of exposed and protected are not fully explained in terms of how consistent they are spatially. For instance, while stations 1-4 and 14-21 are clustered together under the exposed category, the GHGs observations do show that the patterns can be radically different (see e.g. Figs. 2 and 7). All in all, my feeling is that splitting the spatial variability in two “chapters” (e.g. i) salinity gradient, ii) sheltered water ways), would help the authors to convey their arguments more clearly.

Another aspect is the discussion on the drivers explaining the observed GHG variability. Due to the nature of the data sets gathered as part of the study, there is an imbalance between the possible depth of interpretation of pelagic vs benthic processes. I appreciate that the authors remain careful in that they suggest benthic sources/sinks are “likely” drivers. However, given the weight of this argument throughout the manuscript, I have to agree with the other reviewer (Truong An Nguyen) in that this part could be improved. Here I wonder whether the authors could make usage of physical information they collected during their survey to compute e.g. a stratification index that could be used to see at what extent the different areas could have been stratified or well mixed. I noticed that mixing was referred to based on literature, but perhaps it would help the interpretation if we have an understanding of water column oxygen and stratification as potential conditions affecting gas

production/consumption/fluxes to the atmosphere. Lastly, I noticed that the authors gave a relatively high weight to AOU as a tracer for explaining GHGs distribution. The problem I see with that is that AOU is not a compelling predictor of e.g. N2O in shallow systems, where air sea gas exchange is dominant. Furthermore, since the connection between primary production (using chlorophyll as a proxy) and N2O is not direct, so I would not expect this to necessarily provide a mechanistic explanation.

In the following I list other comments, some of which substantiate my assessment above, and some of which correspond to specific issues I spotted while reading the manuscript, and that I hope are useful for the authors when preparing their revision.

Specific comments:

l. 14 – 21: Given that the crux of the manuscript is illustrating the variability in GHG sources and sinks, it would make sense then to express N2O and CH4 in saturations or Delta values (allowing also the reader to directly draw comparisons with global ocean estimates).

l. 28 – 30: The connection between benthic processes and the statement above (as indicated by "therefore") is not clear.

l. 88 – 100: I would suggest revisiting the categories of exposed and sheltered, in particular because areas grouped within the former seems to behave differently (see also comment above).

l. 102 and ff: Information on calibration and drift of the Li-COR analyzer is missing.

l. 111: Here and in other instance the authors refer to 45 min as the time needed to reach equilibrium. However, from the formulation it is not fully clear whether this is the response time of their equilibrator and how it was quantified. Here a more detailed description of the measurement procedure would be useful for the reader.

l. 130 – 131: The agreement between chamber-derived and calculated flux densities seems to be remarkably good. Since such chambers are known to suffer from artefacts under turbulent conditions, it would be good for the readers (potential users) to learn about the sampling conditions.

l. 155: Here it is not clear whether the standardised wind speeds were computed from the mean of wind speeds at 2 and 3.2 m height.

l. 157 and ff.: I think it would be important, for the sake of clarity, to mention how well the in-situ atmospheric measurements fit/are comparable to the atmospheric measurements at this site. For instance, I noticed that the mean value reported for CO2 (406 ppm; l. 195), is considerably lower than values reported at PAL station at the time of sampling: https://gml.noaa.gov/data/data.php?site=PAL&parameter_name=Carbon%2BDioxide&frequency=Discrete. The values publicly available for PAL oscillate between ca. 409 and 416 ppm for the same time period in 2023. Admittedly, these values are flagged as "preliminary", but even after taking the last year of calibrated (final) measurements and applying the global annual rate of increase as an index (see: https://gml.noaa.gov/ccgg/trends/), the mean atmospheric xCO2 value would be ca. 414 ppm. I did not check for the other gases (and certainly would not expect to change much the overall distribution), but it still would be good to clearly state which values were used and why.

l. 188: This should read "coloured dissolved organic matter".

l. 208 and ff.: Some parts of Fig. 3 are addressed during the results section, while others are mentioned only later during the discussion. Since some of the results are referred to in Fig. 4, it is confusing for the reader. I kindly suggest considering to split Fig. 3 in separate figures that appear then in sections 3 and 4.

l. 210 – 211: I think “air-sea flux densities” would be a more adequate term here (instead of “exchanges”).

l. 232 – 241: My impression is that this analysis would be better suited for the discussion. Also, it would perhaps help the line of argumentation if variables which are mechanistically expected to be related are analysed. An example of this would be N2O and CO2.

l. 344 – 365: I think comparing the radiative budget calculated for this system with global estimates (e.g. works by Rosentreter et al and Resplandy et al), would strengthen the arguments laid in this manuscript and emphasize its relevance.

l. 392: It is not clear what is meant by “amplify” the global ocean carbon budget.

Kind regards,
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