

Referee #1

Overview

The authors have satisfactorily addressed the major concerns raised in the first round of review. The revised manuscript now provides a clearer description of the methodological constraints and has tempered the language regarding benthic processes to reflect the lack of direct measurements. I appreciate the detailed responses regarding the equilibration time of the flow-through system and the justification for using a literature-based gas transfer velocity.

Below are my specific comments:

1. Benthic processes and AOU

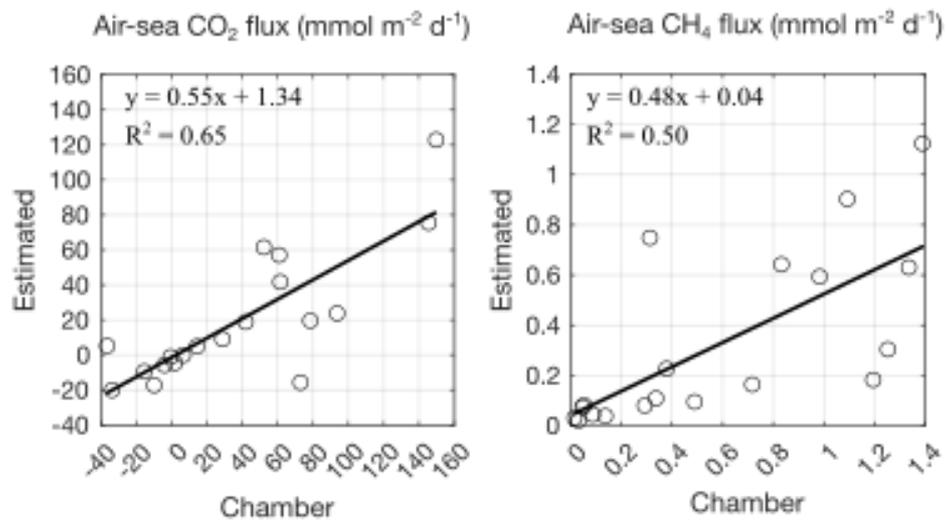
The authors have acknowledged the lack of direct benthic measurements and have modified the text to frame the benthic contribution as a hypothesis supported by AOU and literature. I agree with this revised approach. In shallow coastal ecosystems, it is well-established that benthic rather than pelagic signals often dominate ecosystem metabolism. Therefore, the lack of correlation between pelagic Chl-a and AOU is scientifically consistent with a benthic-driven system. The use of AOU as a proxy for net ecosystem metabolism in this context is acceptable, provided the limitations now stated in the text are maintained.

2. Gas transfer velocity

I accept the authors' justification for retaining the Borges et al. (2004) parameterization derived from Randers Fjord. Given that the Baltic Sea is a non-tidal/microtidal environment similar to Randers Fjord, this is a more appropriate choice than open-ocean parameterizations (e.g., Wanninkhof) or those derived for macrotidal estuaries (e.g., Abril). Furthermore, I agree that deriving a site-specific k wind relationship from only two or three chamber measurements per site would lack statistical robustness and could introduce significant error.

Optional comment: While I understand the logic of using a consistent method for all gases, the availability of direct chamber measurements for CO_2 and CH_4 offers a rare opportunity for validation. If the authors have this comparison available, adding a simple scatter plot of Measured vs. calculated flux to Supplementary Information could be a valuable addition for the community to see how well the standard microtidal parameterization performs in this specific archipelago environment. This is, however, not a requirement for acceptance.

→ We could add the following figure in the supplementary as suggested. It shows the relation between in-situ measurements (chambers) and the estimated air-sea flux of CO_2 and CH_4 .



Recommendation

The authors have effectively responded to the reviewer's comments. I have no further objections and recommend the manuscript for publication.

Thank you for the kind words.

Referee #2

I reviewed a previous version of this manuscript, and provided an overall positive assessment. Yet, I expressed several concerns which were mostly focused on the spatial analysis of the observations, as well as the discussion on greenhouse gas cycling and the role of pelagic vs. benthic environments. Additionally, I provided several specific comments in which I recommended revisions throughout the manuscript.

After reading the revised version of this manuscript, it is my opinion that the authors have adequately addressed the points raised during the first round of review. Below, I provide a list of specific, minor comments which I would advise revising before final acceptance:

l. 71 – 76: While the first two sentences seem to be logically connected, this does not seem to be the case between the second and the third. Perhaps just lies in the formulation.

→ We changed the sentences as follows:

“However, coastal estimates remain uncertain due to sparse measurements and high spatial heterogeneity (Wan et al., 2023). Given the strong warming potential of N₂O, understanding its fluxes from estuaries is essential for a complete picture of coastal greenhouse gas dynamics and for determining how these emissions, together with those of CH₄ (with a warming potential of 27; IPCC 2023), offset the coastal uptake of CO₂ (Resplandy et al., 2024; Rosentreter et al., 2018; Roth et al., 2023). Such offsets directly influence the net climate mitigation potential of coastal ecosystems, thereby questioning their ability to mitigate rising atmospheric GHG concentrations and associated radiative effects (Kristensen et al., 2025; William and Gattuso, 2022).”

l. 81 (“In doing so, we aim to (...)”): I fully agree with the great value of this contribution. Yet the statement “(...) the contribution of coastal ecosystems to GHG emissions (...)” in this context appears quite broad and gives the impression that the manuscript provides a global rather than regional/local perspective.

→ We changed the sentences as follows:

“This approach enabled us to capture spatial variability and examine the roles of physical drivers and biological processes in the observed changes in GHG concentrations. In doing so, we aim to quantify the contribution of the studied coastal ecosystems to GHG emissions and to provide additional data to improve both regional and global estimates of GHG emissions from the coastal ocean.”

l. 101 – 111: There is reference to the types of faunal biomass in the different environmental settings addressed in this study. I would assume this is used as an indication of potential bioturbation activities and their role in benthic GHG cycling. However, this topic (at least from this perspective) is not picked up later in the results or discussion.

→ We thank the reviewer for this comment. While faunal biomass may indeed provide indications of potential bioturbation activity, assessing its role in benthic GHG cycling would require dedicated measurements that are beyond the scope of this study. Given the absence of data directly addressing these processes, a detailed discussion would be speculative.

l. 149: When expressing atmospheric GHG values in ppm or similar, I would not speak about concentrations, but rather mixing ratios, as these are not real units.

→ We corrected the text accordingly.

l. 163: “k” should be italicized.

→ Thank you for the correction.

l. 172: I think “were measured” is more appropriate than “are measured” in this context.

→ We corrected the text accordingly.

l. 215 – 216: The formulation “concentrations were supersaturated” reads odd, as both are quantities referring to the same compound. I would advise revising.

→ The sentence now reads:

“Surface water was consistently supersaturated with respect to CH₄”

l. 216: Perhaps the authors could consider showing the calculated atmospheric concentration of methane (i.e. with the same units as the water concentrations) instead of the mixing ratio. This helps the reader to quickly relate both quantities.

→ We think readers are more likely to be familiar with atmospheric CH₄ concentration (or mixing ratio) expressed in ppm rather than nmol/L. Therefore, we decided to remove the reference to the atmospheric concentration, as it is already mentioned earlier in the text, at line 149.

l. 297 – 298 (“The sampling sites (...)”): This sentence appears to be out of context when compared to the previous and following sentences. I would advise revision.

→ Lines 297-298 read:

“The sampling sites are compared to the seawater endmember (S = 6.42, TA = 1635 μmol kg⁻¹, DIC = 1586 μmol kg⁻¹, black symbol in Fig. 3).”

The preceding sentences explain the theory we will use; this sentence introduces our work, and the following begins the discussion. Therefore, we don’t think it is out of context, but it provides a needed link before presenting/discussing our results.

l. 312 – 315: Similar as above; here the statement about chlorophyll seems only loosely connected with the rest.

→ Line 311 begins a new chapter that discusses the potential role of primary production in the observed GHG concentration. It also initiates the discussion of the role of AOU. It is therefore not loosely connected to the rest but strongly needed for the overall discussion.

l. 343 – 344 (“community production”): This is rather unspecific; short clarification would help the reader.

→ We changed the sentence as follows:

“Benthic processes such as benthic community metabolism (i.e., the balance between production and respiration), organic matter degradation, and...”

Figure 1: There is a “ ’ ” symbol that should be removed. Also, as in the text, it should read “coloured dissolved organic matter”.

→ Thank you for the correction.

Kind regards,
Damian L. Arévalo-Martínez

Referee #3

The authors have accepted the reviewers' comments and criticism and have incorporated most of them very well into the manuscript. The revised manuscript is easy to follow and the main results are presented in a focused manner. The authors have done a good job.

Thank you for the kind words.