Reply to reviewer 3

The manuscript by Zinke et al. investigates the concentration distribution (and fluxes) of the greenhouse gases CH_4, CO_2, and N_2O in surface water in various bays of the Stockholm Archipelago in two seasonal campaigns (spring and fall). The aim was to characterize the spatial and temporal variability of greenhouse gas concentrations (and fluxes) in the surface water of these underrepresented systems and to identify important environmental drivers. The authors recognize, above all, a pronounced variability in the concentration distribution of greenhouse gases.

We sincerely thank the reviewer for their valuable and in-depth feedback, which has been instrumental in improving the clarity, rigor, and overall quality of the manuscript. Below, we provide a detailed point-by-point response to each comment, with our responses highlighted in blue.

General comment

The manuscript deals with an important topic, as the significance of shallow water areas as sources or sinks of greenhouse gases is not yet sufficiently understood. This is mainly due to the insufficient data collection in these areas to date. The manuscript makes a contribution in this regard, which is also its strength. However, the manuscript provides little insight into the environmental drivers that influence the source/sink strength of these areas (in this case, bays). This may be due, among other things, to the fact that the results are not clearly discussed and correlations have not been clearly identified. The authors refer too often to the appendix instead of addressing this central point in the main text. In the main text, the reader is confronted with overloaded tables and is expected to work out the connections for themselves. This was not always possible for me, and often the connections suggested in the text contradict the data shown in the tables or Figure 2.

My opinion is that the manuscript needs extensive revision before it can be proposed for publication. The discussion should be more detailed and, above all, the correlations between greenhouse gas concentrations and environmental factors/drivers should be better elaborated (the main objective of the manuscript, as stated by the authors). The presentation of the results should also be revised to make them more accessible to the reader.

We thank the reviewer for their detailed and constructive assessment. We agree that the original version did not sufficiently elaborate the environmental drivers controlling GHG source/sink behavior in shallow bays, and that key results were too often relegated to the appendix, making the manuscript difficult to follow. In response, we have substantially revised both the *Results and Discussion* sections. Specifically, we now include a dedicated Spearman correlation analysis between GHG concentrations and key environmental parameters in the main text, explicitly addressing the study's central objective of identifying potential drivers. Because environmental parameters were sampled at one central location per bay, we pooled data from April and September/October to ensure sufficient data points. This limitation is now clearly stated and discussed.

We have revised the discussion to better align interpretations with the data shown, clearly distinguishing statistically supported correlations from more speculative explanations, and explicitly acknowledging remaining limitations related to sample size and spatial coverage. We believe these changes substantially improve clarity, coherence, and alignment between the stated objectives, the analyses performed, and the conclusions drawn.

We appreciate the reviewer's concern regarding the visibility and accessibility of the spatial patterns and statistical results. We agree that these findings are central to the manuscript and should be clearly communicated to the reader. At the same time, we aimed to balance clarity with manuscript length and readability. Including all spatial maps (Figs. A1–A6) and detailed statistical tables in the main text

would substantially increase the length of the manuscript and introduce considerable redundancy, as these figures primarily provide spatial detail rather than additional qualitative trends. We therefore chose to retain these materials in the appendix, which is an integral and openly accessible part of the manuscript, while revising the main text to more explicitly describe the key spatial patterns and differences they reveal. In the revised version, we have strengthened the references to the appendix figures, clarified how they support the interpretations, and ensured that all major findings are explicitly stated and discussed in the main text rather than left for the reader to infer from the appendix alone. We believe this approach provides a transparent and reader-friendly presentation while maintaining an appropriate manuscript length.

Specific comments

Abstract

Line 5   Why was the highly productive summer omitted? What was the strategy here in selecting the study periods?

The study periods were selected to cover the pre-spring bloom period and the post-summer bloom period. From previous measurements in the wider Stockholm archipelago we expected significantly elevated CH_4 concentrations in late summer due to organic matter degradation after the summer bloom compared to per-spring bloom conditions. That being said, due to limitations in the availability of equipment and personnel, measurements were only conducted during those two periods.

L6   "relate to bay characteristics and seawater properties", What does that mean? Express it better and define it more precisely.

We have rephrased the sentence as follows: "We explored how GHG concentrations relate to bay physical characteristics (i.e. topographic openness, sediment properties, vegetation cover) and seawater properties (temperature, salinity, dissolved-oxygen saturation, chlorophyll-a, organic carbon, and nutrient concentrations), identifying strong seasonal variation in these relationships."

L9   "indicating a shift in biogeochemical processes", It is very unclear what is meant here. This needs to be explained better here and also addressed more thoroughly in the discussion later.

We have rephrased the sentence in the abstract as follows: "CH_4 concentrations below 250 nmol L^{-1} were negatively correlated with N_2O, whereas higher CH_4 levels showed a positive correlation, suggesting differences in the dominant sedimentary microbial pathways."

In the discussion we added the following text: "Although our measured dissolved oxygen levels measurements in the central bay locations indicate generally oxic conditions in both Högklykeviken ($O_{2,dissolved}$ = 8.3 mg L^{-1} ≈ 91%) and Östra Myttingeviken ($O_{2,dissolved}$ = 5.6 mg L^{-1} ≈ 59%), we cannot resolve small-scale oxygen heterogeneity and therefore can only speculate that oxygen-reduced microenvironments may existed in areas of high CH_4 concentrations. Beyond oxygen availability, several additional mechanisms could explain the shift from a negative to a positive CH_4-N_2O correlation. As mentioned earlier, increased inputs of labile organic matter can stimulate methanogenesis further inside the bays, while changes in the availability of alternative electron acceptors (e.g., nitrate, sulfate, iron) alter competition among metabolic pathways, which can suppress or enhance methanogenesis and modulate N_2O production or consumption. Coupled processes such as nitrate-dependent anaerobic methane oxidation can also link CH_4 and N cycling in non-linear ways. Ebullition would provide a pathway for CH_4 accumulation by bypassing water-column oxidation and decoupling CH_4 from dissolved N_2O dynamics. However, as mentioned previously, our

measurement set-up does not allow us to discern between bubble-mediated and diffusive CH_4. Changes in rooted vegetation and bioturbation may further modify sediment oxygen penetration and bubble release, influencing the relative dominance of CH_4 and N_2O-producing pathways. Finally, sediment disturbance from the research vessel in very shallow areas could explain these anomalous patterns. In order to resolve which of these factors operates in our bays would require targeted process data, limiting our discussion to speculations."

L11   What is meant by "anthropogenic degradation"? This needs to be expressed more clearly.

We have clarified what we mean by "anthropogenic degradation" by explicitly describing the relevant human impacts. The text now reads: *"...with one bay subject to substantial human impacts (e.g. dredging, high nutrient loading, reduced vegetation cover) showing CO_2-equivalent CH_4 emissions that surpassed CO_2 uptake in this particular bay."*

L13ff   At the end of the abstract, the uniqueness/novelty of the manuscript should be emphasized more clearly.

We added the following sentence to the abstract: "This study is distinctive in simultaneously measuring all three major GHGs across multiple bays in relation to diverse environmental controls, offering a uniquely integrated understanding of coastal GHG dynamics."

Introduction

Overall, the introduction is well written. However, it is noticeable that in many places the references supporting the statements have not been cited.

We thank the reviewer for this comment and have added references that support the statements made in the introduction (see response to comments below).

L28ff   References are missing

We added the following references:

Lainela, S., Jacobs, E., Luik, S. T., Rehder, G., & Lips, U. (2024). Seasonal dynamics and regional distribution patterns of CO 2 and CH 4 in the north-eastern Baltic Sea. *Biogeosciences*, *21*(20), 4495-4519.

Bange, H.W., Mongwe, P., Shutler, J.D., Arévalo-Martínez, D.L., Bianchi, D., Lauvset, S.K., Liu, C., Löscher, C.R., Martins, H., Rosentreter, J.A. and Schmale, O. (2024). Advances in understanding of air–sea exchange and cycling of greenhouse gases in the upper ocean. *Elem Sci Anth*, *12*(1), p.00044.

L31ff   References are missing

We added the following reference:

Bauer, J.E., Cai, W.J., Raymond, P.A., Bianchi, T.S., Hopkinson, C.S. and Regnier, P.A., 2013. The changing carbon cycle of the coastal ocean. *Nature*, *504*(7478), pp.61-70.

L36   Not only refer to your own publication from the group here, but also name the original publications (at least one by another author).

We added the following references:

McGinnis, D.F., Greinert, J., Artemov, Y., Beaubien, S.E. and Wüest, A.N.D.A., 2006. Fate of rising methane bubbles in stratified waters: How much methane reaches the atmosphere?. *Journal of Geophysical Research: Oceans*, *111*(C9).

Bisander, T., Prytherch, J. and Brüchert, V., 2025. Methane ebullition as the dominant pathway for carbon sea-air exchange in coastal, shallow water habitats of the Baltic Sea. *Biogeosciences*, *22*(18), pp.4779-4796.

L38	References are missing

We added the following references:

Hanson, R. S., & Hanson, T. E. (1996). Methanotrophic bacteria. *Microbiological reviews*, *60*(2), 439-471.

Venetz, J., Żygadłowska, O.M., Dotsios, N., Wallenius, A.J., van Helmond, N.A., Lenstra, W.K., Klomp, R., Slomp, C.P., Jetten, M.S. and Veraart, A.J., 2024. Seasonal dynamics of the microbial methane filter in the water column of a eutrophic coastal basin. *FEMS microbiology ecology*, *100*(3), p.fiae007.

and the following reference on anaerobic oxidation of methane in response to reviewer 2:

Knittel, K., & Boetius, A. (2009). Anaerobic oxidation of methane: progress with an unknown process. *Annual review of microbiology*, *63*(1), 311-334.

L40	"Methane" or "CH4", This must be consistent throughout the text.

We replaced "methane" with CH_4 throughout the text.

What does the author mean by "**delicate** equilibrium between methane production and oxidation"? This needs to be explained better.

We adjusted the text as follows: "Nevertheless, coastal eutrophication from increased nutrient input via river run-off and anthropogenic sources can alter the balance between CH_4 production by methanogens and CH_4 oxidation by methanotrophs, such that net CH_4 emissions may increase or decrease depending on environmental conditions."

L42	References are missing

We added the following references:

Broman, E., Sjöstedt, J., Pinhassi, J. and Dopson, M., 2017. Shifts in coastal sediment oxygenation cause pronounced changes in microbial community composition and associated metabolism. *Microbiome*, *5*(1), p.96.

Egger, M., Lenstra, W., Jong, D., Meysman, F.J., Sapart, C.J., Van der Veen, C., Röckmann, T., Gonzalez, S. and Slomp, C.P., 2016. Rapid sediment accumulation results in high methane effluxes from coastal sediments. *PloS one*, *11*(8), p.e0161609.

L44	Here, the sentence needs to explain more clearly how "promotes organic matter accumulation" affects the O2 content in the water.

We added the following sentence: "As this material decomposes, microbial respiration consumes oxygen faster than it can be replenished, leading to hypoxic or anoxic conditions (Heip et al., 1995)."

Heip, C., Goosen, N., Herman, P., Kromkamp, J., Middelburg, J., and Soetaert, K.: Production and consumption of biological particles in temperate tidal estuaries, Oceanography and Marine Biology: an annual review, 1995

L50   Reference to eutrophication in coastal ecosystems is missing

We added the following reference:

Żygadłowska, O.M., Roth, F., van Helmond, N.A., Lenstra, W.K., Venetz, J., Dotsios, N., Rockmann, T., Veraart, A.J., Stranne, C., Humborg, C. and Jetten, M.S., 2024. Eutrophication and deoxygenation drive high methane emissions from a Brackish Coastal system. *Environmental Science & Technology*, *58*(24), pp.10582-10590.

L52   CRDS technology can no longer be promoted as a novelty

We removed the word "Recent"

L55   Discuss here why these periods were selected > Sampling strategy

See also response to an earlier comment. We added the following text to the manuscript: "These sampling periods were selected to cover the pre-spring bloom period and the post-summer bloom period."

L55ff   This is where the manuscript's main weakness lies: how do which drivers affect GHG concentrations? This seems to be one of the main objectives of the manuscript.

A more in depth discussion of the environmental drivers based on a correlation analysis was added to the manuscript (see also response to next comment and response to reviewer 2).

L57ff   Present more clearly in the discussion how the environmental factors mentioned affect GHG concentration/emissions: eutrophication gradient, geomorphology, physical factors (water retention time), sediment composition. Can the manuscript provide a clear answer to this complex question.

We have now added a discussion of Spearman correlations between GHGs and environmental parameters (for a more detailed answer we refer to the response to reviewer 2).

L59ff   "show distinct spatial patterns, with hotspots emerging in different niches within a bay", where in the following discussion are spatial patterns and hotspots addressed? Should this be evident from the spatial surface data in the appendix? If so, it needs to be given more weight and better presented in the main text.

We thank the reviewer for this comment. The sentence cited refers to one of the central hypotheses of the study: that the three GHGs would exhibit distinct spatial distributions between bays, within each bay and between inside- and outside-bay areas. These spatial differences are addressed in Sections 3.1.1–3.1.3, where we compare between bays and inside versus outside bay areas. The within-bay variability and the location of local hotspots illustrated in detail in Figures A1–A6. CH_4 and N_2O spatial variability and hotspots are further discussed in more detail in section 3.1.5 (where we

describe the correlation between N_2O and CH_4) and linked to environmental characteristics of these bays (in sections 3.1.4-3.1.5).

While incorporating all maps into the main manuscript would substantially increase its length, we note that the appendix is an integral part of the manuscript (as opposed to supplementary material), and therefore remains fully accessible to the reader. We believe this approach provides the best balance between readability and comprehensive presentation of the spatial data.

Methods

L66   What does "continuous daytime measurements" mean? During what period (times of day) were the tests carried out? It should also be mentioned here at the beginning when exactly the investigations in the bays took place.

Measurements were always conducted around midday as stated in L81 of the original manuscript.

L73ff   Here, the reader is confronted with a multitude of references intended to show that *Ea* is a measure that affects GHG dynamics in bays: "all of which can influence GHG cycling." Ultimately, the authors must ask themselves whether their data really demonstrate a solid correlation between *Ea* and GHG dynamics (concentrations and emissions). This was not always clear to me in the discussion.

Our intention in introducing the topographic openness index (Ea) was not to claim a direct or mechanistic relationship between Ea and GHG concentrations/fluxes based on our dataset alone, but rather to justify why bay openness is a relevant contextual variable in coastal systems as Ea impacts parameters such as water exchange, sediment type and community structure which are known to modulate GHG cycling in many coastal environments.

We agree that our dataset does not demonstrate a consistently strong or uniform correlation between Ea and GHG dynamics. We have revised the paragraph (see response to next comment). We have further removed the separation between enclosed and semi-open bays, as the different bays clearly represent a gradient in openness.

        Furthermore, it is not clear to the reader, for example, how sediment characteristics (what is meant by this?) and biological communities (which are referenced here) affect the GHG cycle. What information is contained in the references and how does it relate to GHG dynamics? Since, in my opinion, this is central to the strategy of the study, it needs to be introduced here and addressed in more detail in the discussion.

We have now rephrased the text and added more details as follows: "Bay openness strongly influences water retention time (Persson et al., 1994), sediment characteristics such as grain size, organic-matter content, and redox conditions (Wikström et al., 2025), as well as the composition of benthic and macrophyte communities (Munsterhjelm, 1997; Hansen et al., 2008; Snickars et al., 2009; Scheinin and Mattila, 2010). In enclosed bays, reduced water exchange promotes the accumulation of fine sediments and organic matter, creating conditions favourable for anaerobic decomposition and methane production in the sediment. Conversely, open bays often are characterized by coarser, more oxygenated sediments that enhance aerobic respiration and CH_4 oxidation. Likewise, differences in macrophyte cover influence sediment oxygenation through root oxygen release and alter organic-matter deposition."

In the discussion we believe this is already discussed in L200-204 in the original version of the manuscript.

L77ff   The postulated correlation between chl/nutrients (TP) and open/closed bay is not clearly evident in the table.

We have visualized the data from table 1 in the figure below. When treating Högklykeviken as an outlier, as it clearly had significantly higher TP and CHL-a values, the remaining bays show significant correlations between openness and TP or CHL-a.

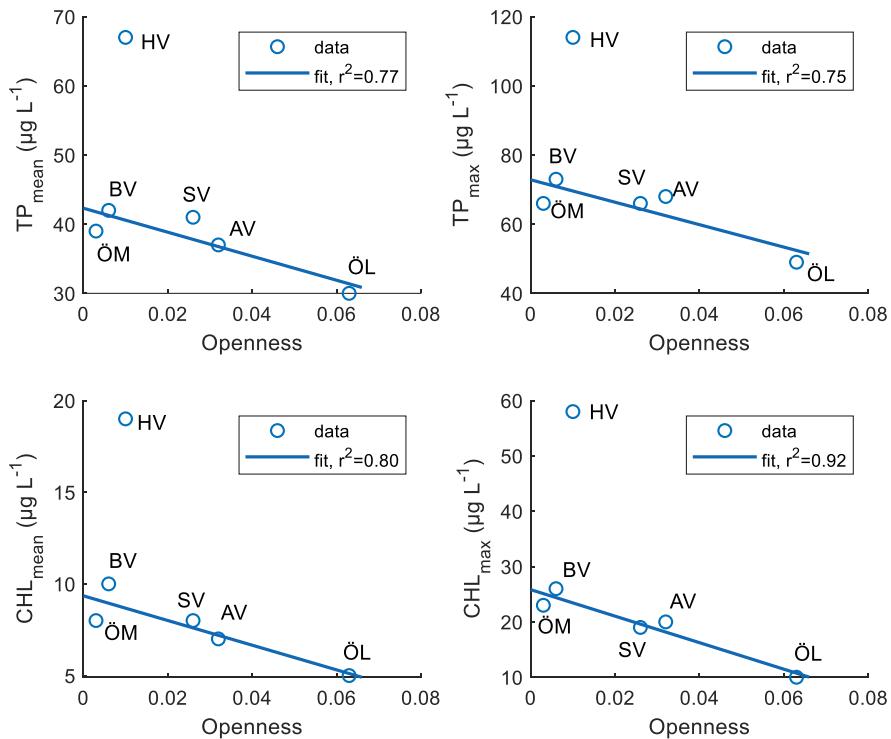
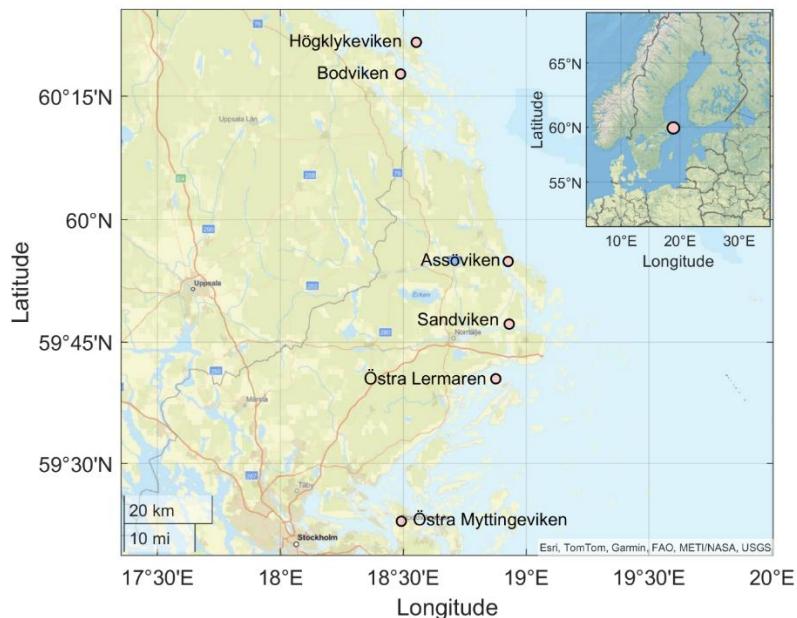



Figure: Correlations between openness and total phosphorus (TP) and chlorophyll a (CHL).

Fig. 1   This is the first time I have seen a TomTom map in a publication. If you want to keep it, it needs to be revised: the cities are not readable and some of them can be deleted. The same applies to the roads. The land color is very weak and could be better distinguished from the water.

The TomTom basemap is generated using MATLAB's *geobasemap* function, which provides standardized, geographically accurate, and properly licensed cartographic layers suitable for scientific figures. We use this basemap to ensure reproducibility and compliance with licensing requirements. In response to the reviewer's comments, we revised the inset map to a different basemap style (not TomTom based) with stronger land–water contrast and without any city labels. We also updated the basemap style of the zoomed-in map to increase the land–water contrast but retained key geographical reference points (major cities and main roads), as these aid orientation. Although the underlying basemap is still derived from TomTom data, we selected this style because it offers the best balance between resolution, contrast, and geographic context. MATLAB does not allow manual

adjustment of basemap label font sizes; however, we believe the locations of our sampling bays remain clear and easy to interpret in the revised figure.

L86   At a depth of 30 cm, it is really possible to avoid air bubbles being sucked into the WEGAS system. What were the ship's speed and wind conditions (waves)?

The ship moved at a speed of 3 kn and given the sheltered nature of the bays, we observed no wave breaking/ white capping inside the bays as stated in L117 of the initial manuscript. Under these conditions a sampling depth of 30 cm was sufficient to ensure that no air was sucked into the system. Furthermore, given the small diameter of the sampling inlet (1/2 inch≈12.7 mm), it is highly unlikely that bubbles originating from the sediments were sampled into the system.

L90   How was the system calibrated?

The Picarro G2508 used in this study was factory-calibrated by the manufacturer in 2022, and the field campaign presented here represents its first deployment after calibration. The G2508 operates using cavity-ring-down spectroscopy (CRDS), which exhibits negligible long-term drift according to the manufacturer's documentation. Thus, the probability of significant drift between factory calibration and our field deployment is low. Furthermore, because our comparisons focus primarily on relative differences among bays measured with the same instrument under identical conditions, any potential systematic bias would not affect the observed spatial patterns.

To further assess potential drift, we conducted a post-study calibration against certified standard gases (only for CO_2 and CH_4). We found that since the last calibration, the slope only changed by 0.2% for CO_2 and -0.5% for CH_4 suggesting that spatial and temporal differences are not affected by potential sensor drift. Assuming monotonic drift, the offset applicable to the April and September measurements was estimated by linear interpolation and ranged from -0.4 ppm for CO_2 and 0.03 ppm for CH_4 for the April measurements to -1.2 ppm CO_2 and 0.04 ppm CH_4 for the September measurements, which is much smaller than the measured values in this study. This information was added to the manuscript section 2.2.

L95   How were the ppm determined? Why are CO_2 concentrations not converted?

In the revised version we now present CO_2 concentrations in µatm (which is a common way to present pCO_2). The partial pressure in µatm was derived from the mole fraction (in ppm) measured in the equilibrated air using the Seacarb function (see also response to next comment).

L98   How were the partial pressures determined? How was the total pressure in the Equi headspace determined?

Partial pressures were derived from mole fractions measured in the equilibrated air using the Seacarb function, accounting for temperature and density effects. The total pressure in the equilibrator was assumed to be 1 atm. We have added this information to the manuscript: "Mole fractions (ppm) of CO2 were converted to partial pressures (µatm) using the Seacarb (v.3.3) x2pCO2 function (Gattuso et al., 2021)"

L104   Eq 3. In which calculation is the Bunsen coefficient used? Please complete the equations.

In line 107 of the initial manuscript we stated "For N_2O, $K_0 = \beta$. For CH_4 (ideal gas behavior),
$K_0 = \beta(R \times 273.15$ K).""

L106   How and where was the temperature measured?

Seawater temperature was measured using a thermosalinograph, located in the water handling system directly directly prior to the shower-head equilibrator. This is stated in L86-87 in the intial manuscript.

L106ff   "For N2O, K0 = β. For CH4 (ideal gas behavior), K0 = β(R×273.15 K)", Please write complete and understandable sentences.

We have now rephrased the sentence as follows: "For N_2O, the solubility constant is given by $K_0=\beta$, whereas for CH_4—assuming ideal gas behavior—the solubility constant is calculated as $K_0=\beta(R \times 273.15$ K).""

L109   Please write "air-sea fluxes of GHG ..."

We added "of GHGs".

L111   "K_0 is solubilty", the connection to Eq. 3 is not clear here. Complete the equations.

We believe that the connection between equation 3 and K_0 is clear from the sentence that follows the equation and that no additional equation is needed to make this more clear. Many published studies present this connection in a similar manner (e.g. Roth et al., 2022; 2023).

L12   The usability of this *k*-model must be explained. Why is it considered suitable? And does it allow for comparison with published fluxes (Table 5)?

We thank the reviewer for this important comment regarding the choice of gas-transfer velocity parameterization. Although our measurements were conducted in brackish waters (S ≈ 5 PSU), we selected the Cole & Caraco (1998) formulation because it was developed for shallow, sheltered, fetch-limited systems where gas exchange is not solely driven by wind speed. These physical conditions closely resemble those of our study bays, which are characterized by low currents, short fetch, and weak wind forcing. Supersaturation of surface waters implies a positive air–water

concentration gradient and thus outgassing even under low wind conditions; the Cole & Caraco parameterization accounts for this by allowing non-zero gas exchange at low wind speeds.

In contrast, parameterizations such as Wanninkhof (2014) are optimized for open-ocean conditions with large fetch and fully developed wave fields and are therefore likely to underestimate fluxes in small, sheltered bays. Other parameterization developed for macrotidal estuaries (e.g. Borges et al., 2004) likely represent an upper bound for our system. A comparison of flux estimates using these three parameterizations shows that Cole & Caraco (1998) yields intermediate values, supporting its suitability for the physical setting of our study.

We have now added a discussion on the sensitivity of flux estimates to different gas-transfer parameterizations and highlight that methodological differences can substantially affect absolute flux values, thereby limiting direct comparability between studies:"Furthermore, estimates of air–water GHG fluxes are highly sensitive to the choice of gas-transfer velocity parameterization. In this study, we applied the formulation by Cole and Caraco (1998), which was developed for shallow, sheltered, fetch-limited systems and allows for non-zero gas exchange under low wind speeds. This is particularly relevant for the studied bays, which are characterized by weak currents and limited wind-driven turbulence. Alternative parameterizations such as the open-ocean parameterization of Wanninkhof (2014) or the estuarine parameterization of Borges et al. (2004) produce significantly lower or higher estimates, respectively. These differences highlight that absolute flux values are strongly dependent on the assumed turbulence regime and caution against direct inter-study comparisons without careful consideration of the underlying gas transfer assumptions"

L116 $Sc_{this\ study}$, "this study" can be deleted as it is not mentioned in Eq 5.

We removed "this study" based on the reviewers suggestion.

L117ff   Is it really appropriate to use an average value here? Wind events prior to the measurements could have caused increased GHG emissions from the water. This may make it difficult to compare the bays and time periods. The wind data for the measurement days should be shown here. How does the value of 2 m s-1 fit in with the measured data? Please discuss.

We agree that wind speed is an important driver of GHG exchange across air–water interfaces. However, as mentioned in the manuscript, we did not measure wind speed during our deployments. Furthermore, for several of the bays, no nearby meteorological stations exist that would represent local wind conditions. The closest stations were often >20–40 km away and located in very different exposure settings. Using these stations would likely introduce bias rather than reduce it.

For this reason, we decided to use wind estimates from the ICON EU model (DWD), obtained at 10 m height and ~7 km horizontal resolution. While the model cannot resolve small-scale variability, it provides a consistent and physically based estimate of the regional background wind forcing at the standard 10-m reference height. Because gas-transfer parameterizations are conventionally defined at 10 m wind speed, model-derived 10-m wind is actually more representative for applying standard flux equations than the near-surface wind we experienced during sampling. As such, we consider the ICON-EU 10-m wind to be more appropriate than assuming a constant 2 m s^{-1} wind across all sites.

We have added the following text to the manuscript: "Wind speed at 10 m height was obtained from the ICON-EU numerical weather prediction model (Deutscher Wetterdienst, Germany). Model output at ~7 km horizontal resolution was accessed through the Ventusky online visualization platform (https://www.ventusky.com). We extracted 10-m wind values corresponding to the sampling dates

and coordinates of each site. The derived wind speeds were 1.67 m s⁻¹ (Sandviken), 7.0 m s⁻¹ (Assöviken), 6.67 m s⁻¹ (Högklykeviken), and 4.4 m s⁻¹ (Bodviken) in April; 3.3 m s⁻¹ (Sandviken), 3.9 m s⁻¹ (Assöviken), 7.2 m s⁻¹ (Högklykeviken), and 3.6 m s⁻¹ (Bodviken) in September; and 2.5 m s⁻¹ in both Östra Lermaren and Östra Myttingeviken in October."

L125ff   This section on methodology lacks any references.

We have added the following references to the respective methods:

TP and PO_4-P: Swedish Standards Institute (SIS). Water quality - Determination of phosphorus - Ammonium molybdate spectrometric method (ISO 6878:2004). http://www.sis.se

NO_2-N + NO_3-N, TN: Swedish Standards Institute (SIS). Water quality – Determination of nitrite nitrogen and nitrate nitrogen and the sum of both by flow analysis (CFA and FIA) and spectrometric detection (ISO 13395:1996), 1996;

NH_4-N: Swedish Standards Institute (SIS). Water quality – Determination of ammonium nitrogen - Method by flow analysis (CFA and FIA) and spectrometric detection (ISO 11732:2005), 2005; http://www.sis.se

Dissolved oxygen: Swedish Standards Institute (SIS). Water quality – Determination of dissolved oxygen - Electrochemical probe method (ISO 5814:2012), 2012; http://www.sis.se.

Temperature and salinity: Swedish Standards Institute (SIS). Water quality – Determination of electrical conductivity (ISO 7888:1985), 1985; http://www.sis.se.

Chlorophyll-a: Swedish Standards Institute (SIS). Determination of chlorophyll in water – Extraction with acetone; Spectrophotometric method (SS28146), 1980; http://www.sis.se.

TOC: Swedish Standards Institute (SIS): Water analysis - Guidelines for the determination of total organic carbon (TOC) and dissolved organic carbon (DOC) (SS-EN 1484), http://www.sis.se, 2024

Loss on ignition (LOI): Swedish Standards Institute (SIS). Determination of suspended solids in waste water and their residue on ignition. http://www.sis.se

Turbidity: Swedish Standards Institute (SIS). Water quality - Determination of turbidity - Part 1: Quantitative methods (ISO 7027-1:2016). http://www.sis.se

L139   Were all basins inspected by divers a few weeks before September? Please write more clearly in the text. How were these estimates of vegetation cover (in %) made? There is no reference to the method used here.

The vegetation assessment method follows a standardized national protocol that has been widely applied in this region (e.g., Hansen et al., 2019). We have now added this reference and clarified in the manuscript that all bays were surveyed visually by divers prior to the September sampling campaign, during which percentage cover was estimated in situ following the established protocol.

Hansen, J.P., Sundblad, G., Bergström, U. *et al.* Recreational boating degrades vegetation important for fish recruitment. *Ambio* 48, 539–551 (2019). https://doi.org/10.1007/s13280-018-1088-x

L142   "The interannual variation in total vegetation cover is rather small in these bays", How do you know that? Please provide a reference.

We conducted an analysis on vegetation data collected yearly in the bays from 2020 to 2024. These analyses have not been published yet. As we don't have a published reference for the sentence, we have decided to remove it.

L144ff   The word "plot" seems a little unusual to me. Maybe replace it.

We replaced the word "plot" with "survey site".

L146ff   Why were these two vegetation indicators chosen? Please explain.

We selected two vegetation indicators—(1) total vegetation cover and (2) cumulative cover of all rooted vegetation—because they capture distinct functional aspects of benthic vegetation that are relevant for greenhouse-gas dynamics. Total vegetation cover provides an integrated measure of overall primary producer abundance, which can influence water-column oxygen dynamics and carbon cycling through photosynthesis and respiration. Rooted vegetation cover specifically reflects the presence of macrophytes capable of affecting sediment–water exchange processes through below-ground gas transport in addition to photosynthesis and respiration. These indicators therefore represent the most ecologically meaningful metrics for assessing vegetation-related controls on GHG concentrations in these shallow bays.

We have added this explanation to the manuscript.

L150ff   This section on methodology lacks any references.

The procedures described in this section follow widely used and well-established field and laboratory methods. In previous work (e.g. Hermans et al., 2024), we have not typically cited references for standard techniques such as core slicing, as these are routine in marine sediment studies. We have now added a reference for the loss-on-ignition (LOI) procedure at 550 °C. This temperature is consistent with EPA Method 160.4, which provides a widely used protocol for measuring organic matter content.

U.S. Environmental Protection Agency. (1971). *Method 160.4: Residue, Volatile (Gravimetric, Ignition at 550°C) by Muffle Furnace.* U.S. Environmental Protection Agency.

L154   Is the sediment dry density of 2.65 g cm-3 (Burdige et al.) applicable to all sediment types in the different bays (inner and outer part of the bay)?

A dry grain density of 2.65 g cm^{-3} is widely used as a standard value in marine sediment studies because the lithogenic fraction is typically dominated by light silicate minerals—primarily quartz—which have grain densities close to this value (Burdige, 2006). That said, the actual density of bay sediments may be lower and can vary between sites and depth intervals due to differing mineralogical and organic matter compositions. However, for porosity calculations, using a fixed grain density is necessary to avoid introducing additional, poorly constrained variables. Additionally, we would like to clarify that sediment cores were only collected in the center of each bay and not in the outer-bay area.

L157ff   Is it really the case that the top 1 cm of sediment is responsible for GHG dynamics in surface water? Methane production usually occurs in deeper sediments. And what about possible gas releases from deeper sediment strata? How does the top 1 cm of sediment relate to this?

Thank you for noting this—on revisiting the text, we agree that the original sentence was misleading. While the surface centimetre is indeed biogeochemically active and directly interacts with the overlying water column, it is more appropriate to state that this layer regulates **benthic diffusive fluxes** of greenhouse gases rather than overall sediment GHG production. As the reviewer correctly points out, CH_4 production predominantly occurs deeper in the sediment column, and ebullitive fluxes can transport gas from these deeper layers directly to the water column. We have rephrased the text as follows: "For this study, we used only data from the uppermost sediment layer (0–1 cm) which represents the sediment-water interface where redox-sensitive processes and exchanges directly influence surface-water GHG concentrations. We note that deeper sediment layers may be important for methane production and ebullition dynamics, but were beyond the scope of the present study."

Results and Discussion

L161   This entire paragraph refers to important findings that are only indicated here. Instead, reference is made to the statistics in the appendix. Since this is a central part of the manuscript, the results must be included in the main text and explained in more detail (including in a graphical representation, not just in tables). What does it mean that statistical tests "confirmed significant differences in GHG concentrations between bays"? Wouldn't one expect that? What are the differences? How pronounced are the differences and do they allow for classification in the *Ea* category? Can environmental drivers that influence GHG concentrations be derived from this?

We thank the reviewer for this comment. The paragraph in question is intended to introduce the broader structure of the results section by summarizing the statistical framework used to assess spatial and seasonal variability in GHG concentrations. The detailed interpretation of these differences—both between bays and between inside versus outside bay areas—is provided in the subsequent subsections (Sections 3.1.1–3.1.3), where we describe patterns for each gas individually. These patterns are also visually represented in the boxplots in Fig. 2, which clearly illustrate the magnitude and direction of differences across bays, seasons, and inside/outside regions. In the revised manuscript, we have added a Spearman correlation analysis to investigate the relationships between GHG concentrations and key environmental variables.

Regarding the location of the statistical tables, in an other comment the reviewer suggested reducing the number of tables in the main manuscript. To align with that guidance, we have decided to keep the detailed statistical output in the appendix, which is an integral part of the manuscript and fully accessible to readers.

Fig. 2   This graphic needs to be revised: (a), (b)... need to be aligned with each other, made larger and bold; lines in the illustrations are too narrow and unclear and could possibly be removed entirely. The scaling of the y-axis also varies, which makes comparison difficult. I suggest sorting the bays into "closed" and "open" so that the reader does not always have to refer to Table 1. Why are CO_2 concentrations given in ppm (in headspace gas) and not converted to CO_2 dissolved in water?

We have adapted the figure based on the reviewers suggestion: We have aligned the panel labels, removed the grids, sorted the bays by openness and now present pCO_2 values in µatm. However, we have deliberately decided to present the data on varying y-axis in order to have a better resolution of the between-bay variability. Presenting the data on the same scale would result in a loss of information gained from this plot.

L167   I would remove the word "pattern" from the headings.

We have removed "and patterns" from the subheadings.

L168   What are the saturation values referred to here? Perhaps you could mark them in Fig. 2.

The saturation values are provided in Table 2, and we have now added an explicit reference to this table in the text for clarity. In addition, we double-checked and corrected the saturation calculations in response to Reviewer 1's comments. We chose not to add the saturation values directly to Figure 2, as we believe they are more clearly presented in the table, and including them in the figure would overcrowd the graphic without adding additional information.

Why is reference made to Table 2 and A1 and not to the central Figure 2?

We have added a reference to Figure 2.

L170ff   "These bays showed significantly higher CO2 concentrations inside compared to outside areas" Given the overall inconsistency in Fig. 2 between all bays, I'm not sure if this really needs to be pointed out. The discussion here should focus more on why such an inconsistent pattern occurs (in contrast to other GHGs).

To clarify, the statement regarding higher CO_2 inside the bays refers specifically to those bays where overall CO_2 concentrations were elevated. In these cases, inside-bay measurements were significantly higher than outside-bay values, as shown in the boxplots in Fig. 2. In contrast, in bays where CO_2 concentrations were close to saturation, no consistent inside–outside differences were observed, and statistical tests did not indicate significant contrasts. Thus, while the pattern is not universal across all bays, it is robust for the subset of bays with the highest CO_2 levels.

L175   How can the high CO2 concentrations and high degree of eutrophication at BV be explained? This contradicts the statement made in the text.

We thank the reviewer for pointing out this inconsistency. In the revised manuscript, we have clarified that BV displays *high* eutrophication status. We now explain that high CO_2 concentrations in BV are consistent with enhanced remineralisation of organic matter under eutrophic conditions.

Revised text: "The bays with elevated CO_2 concentrations shared several characteristics: extensive vegetation cover across all three bays, as well as high sediment organic carbon content and lower eutrophication status in Östra Lermaren and Östra Myttingeviken (see Tables 3 and 4). While it might seem counter-intuitive that these two bays, which are the least eutrophied and most vegetated bays, act as hotspots for CO_2 emissions, this might be due to high autochthonous input from decaying plant matter. This pattern aligns with other coastal studies documenting seasonal CO2 hotspots in areas with elevated organic matter input due to remineralization processes (Amaral et al., 2021; Asmala and Scheinin, 2024). In contrast, Bodviken exhibited both high CO_2 and comparatively higher eutrophication status, suggesting that enhanced internal mineralization under nutrient-rich conditions may dominate CO_2 production in this system."

L176   "counter-intuitive that the least eutrophied … bays", BV indicates a comparatively high degree of eutrophication in TP and Chl. So how does the statement in the text match up with this?

We agree with the reviewer that the phrasing was inaccurate. In the revised text we acknowledge that BV is in fact among the more eutrophic bays and explain why eutrophic and vegetated systems can still act as CO_2 hotspots due to elevated respiration and sediment mineralisation (see revised text in the comment above).

L177   "this might be due to high allochthonous, terrestrial input and autochthonous input from decaying plant matter", What is this assumption about these bays based on (references?), and why should allochthonous input into these bays be different from the other bays studied?

We appreciate the reviewer's comment and have revised the text to avoid unsupported statements. As such we have now removed the reference to allochthonous, terrestrial input (see revised text in the comment above).

L188   How does the author conclude that this coastal CO_2 source is overlooked? It is understudied. What do other studies say about this topic (references)?

Previous studies have reported CO_2 hot spots in similar coastal areas in the Baltic Sea (e.g. Humborg et al., 2019; Asmala & Scheinin, 2024; Woszczyk & Schubert, 2025). Other studies in more open areas of the coastal Baltic Sea (e.g. Lainela et al., 2024; Honkanen et al., 2024) have reported CO_2 draw-down associated with the spring/summer blooms. The differences between those studies are likely caused by differences in depth, openness, seasons and parameterizations used.

We rephrased the text as follows: "These findings suggest that although shallow bays accumulate substantial organic matter and are significant reservoirs of carbon and nutrients accumulated from surrounding areas (Gubri et al., 2025; Wikström et al., 2025), their role in atmospheric CO_2 exchange is not uniform. Instead, they may function either as CO_2 sources or sinks depending on seasonal conditions and bay-specific properties such as openness, vegetation cover, and eutrophication status."

Asmala, E. and Scheinin, M., 2024. Persistent hot spots of CO2 and CH4 in coastal nearshore environments. *Limnology and Oceanography Letters*, *9*(2), pp.119-127.

Honkanen, M., Aurela, M., Hatakka, J., Haraguchi, L., Kielosto, S., Mäkelä, T., Seppälä, J., Siiriä, S.M., Stenbäck, K., Tuovinen, J.P. and Ylöstalo, P., 2024. Interannual and seasonal variability of the air–sea CO 2 exchange at Utö in the coastal region of the Baltic Sea. *Biogeosciences*, *21*(19), pp.4341-4359.

Humborg, C., Geibel, M.C., Sun, X., McCrackin, M., Mörth, C.M., Stranne, C., Jakobsson, M., Gustafsson, B., Sokolov, A., Norkko, A. and Norkko, J., 2019. High emissions of carbon dioxide and methane from the coastal Baltic Sea at the end of a summer heat wave. *Frontiers in Marine Science*, *6*, p.493.

Lainela, S., Jacobs, E., Luik, S.T., Rehder, G. and Lips, U., 2024. Seasonal dynamics and regional distribution patterns of CO 2 and CH 4 in the north-eastern Baltic Sea. *Biogeosciences*, *21*(20), pp.4495-4519.

Woszczyk, M. and Schubert, C.J., 2021. Greenhouse gas emissions from Baltic coastal lakes. *Science of the Total Environment*, *755*, p.143500.

L193   What are the saturation values referred to here? Perhaps you could mark them in Fig. 2.

The saturation values are presented in Table 2. Similar to the chapter on CO_2, we have decided not to include the saturations in Figure 2.

        Why is reference made to Table 2 and A1 and not to the central Figure 2?

Here we do not refer to Table 2 or A1. Instead we refer to Table A2 to address the statistical test performed to underline the significant differences between inside and outside bays. We have now also added references to Fig. 2 and Table 2 as well.

L196   What is the context of the reference to Conrad (2009)? Is this about high methanogenic activity due to the conversion of organic material?

Yes, the reference to Conrad (2009) is meant to provide mechanistic context by explaining that methanogenic archaea rely on substrates generated during the anaerobic degradation of organic matter

L198   What is meant by "degraded system"?

This particular bay is highly impacted by anthropogenic influences such as dredging, input of nutrients and high boat traffic, as reflected in the elevated TN concentrations measured in this bay. We have now replaced "degraded" with "disturbed".

L201ff   Reference missing to studies that have already shown this.

We have added the following references:

Gubri, B., Hansen, J.P., Wikström, S.A., Snickars, M., Dahl, M., Gullström, M., Rydin, E., Masqué, P., Garbaras, A., Björk, M. and Boström, C., 2025. Shallow Coastal Bays as Sediment Carbon and Nutrient Reservoirs in the Baltic Sea. *Estuaries and Coasts*, *48*(5), p.136.

Egger, M., Lenstra, W., Jong, D., Meysman, F.J., Sapart, C.J., Van der Veen, C., Röckmann, T., Gonzalez, S. and Slomp, C.P., 2016. Rapid sediment accumulation results in high methane effluxes from coastal sediments. *PloS one*, *11*(8), p.e0161609.

L202ff   Figure 2 does not show that closed and open bays can be clearly grouped according to their CH4 concentration. Rather, the picture is inconsistent. I thought that the author's intention was to be able to clearly assign GHG concentrations to open and closed bays, but this is difficult to see. Please reconsider your statement.

Our initial hypothesis was that higher CH_4 concentrations will be observed in more enclosed bays. While it is true that this does not become evident from our measurements, this is still a surprising finding of our study.

L205ff   How does this statement fit with your own data: In Table 4, OCsed (positive CH4 conc.) correlates with vegetation cover (negative CH4 conc.). Please discuss using your own data! Reference to the statement at the end of the sentence is missing.

We have now removed this sentence as the pooled data from April and September no longer shows a significant correlation.

L211   Please replace the word "boost."

We replaced "boosting" with ", thereby increasing"

L211ff   Warming of the water column leads to stratification and high CH_4 concentrations below the thermocline and low concentrations above the thermocline (exchange with the atmosphere).

We adapted the text as follows: "Warmer seawater temperatures also decrease CH4 solubility and enhance stratification, resulting in elevated CH_4 concentrations below the thermocline and lower concentrations above it."

L213ff   An illustration showing the correlation between conc. CH4 and salinity is missing here. Where does the fresh water come from? Do the bays differ in terms of their river inflows?

We have added a heatmap plot and scatterplots showing the relationships between GHGs and environmental parameters (including the relationship between CH_4 and salinity). The salinity in most bays was close to the salinity of more open areas of this area of the Baltic Sea during the September measurements. Only Bodviken was characterized by a lower salinity ~3.2 g/kg in spring. This decreased salinity could be attributed melting ice, which occurred only a few days prior to the sampling campaign. Similarly, slightly lower salinities were measured in April in Sandviken, Assöviken and Högklykeviken.

L216ff   There is no apparent connection between the sentences. Why does this sentence have to be here? It has no connection to the explanations in the previous sentences.

We agree that the sentence appeared disconnected from the surrounding text. To improve the logical flow, we have rephrased it as follows: "Another factor that can contribute substantially to CH_4 emissions in shallow, organic rich sediments is ebullition (McGinnis et al., 2006; Hermans et al., 2024; Bisander et al., 2025). Recently, Bisander et al. (2025) showed that ebullition from sandy sediments can be substantial. The WEGAS system measures CH_4 from both benthic diffusion and bubble dissolution. Consequently, the observed CH_4 concentrations represent the combined effect of these pathways, and without isotopic information we cannot distinguish between diffusive transport and ebullition. Although no visible bubbling was observed during sampling, we cannot exclude the possibility that episodic ebullition events might have impacted our measurements. This measurement limitation should be considered when interpreting the relationships between CH_4 and the environmental parameters described above"

L217   Reference is missing.

This sentence was now rephrased.

L218ff   I would move this paragraph up to where CH4 concentrations were discussed already (L193ff).

We decided to keep this paragraph here such that the sections on the three GHGs follow the same structure: (1) describe seasonal and inter-bay variability, (2) relate variations in GHG concentrations to the environmental parameters and (3) comparison to previous studies.

L218   How do these values compare with those for the open Baltic Sea?

In response to the comment by reviewer 1, we have added a comparison to CH_4 in the open Baltic Sea, measured from the Finnmaid ferry: "The CH_4 concentrations measured in our study are significantly higher than values reported from long-term measurements in the open Baltic Sea, ranging between 3.5-6 nmol L^{-1} (Schneider et al., 2014), 2.8-18.6 nmol L^{-1} (Jacobs et al., 2020) and 3.2-22 nmol L^{-1} (Gülzow et al., 2013)."

Gülzow, W., Rehder, G., Schneider v Deimling, J., Seifert, T. and Tóth, Z., 2013. One year of continuous measurements constraining methane emissions from the Baltic Sea to the atmosphere using a ship of opportunity. *Biogeosciences*, *10*(1), pp.81-99.

Jacobs, E., Bittig, H.C., Gräwe, U., Graves, C.A., Glockzin, M., Müller, J.D., Schneider, B. and Rehder, G., 2020. Upwelling-induced trace gas dynamics in the Baltic Sea inferred from 8 years of autonomous measurements on a ship of opportunity. *Biogeosciences Discussions*, *2020*, pp.1-38.

Schneider, B., Gülzow, W., Sadkowiak, B. and Rehder, G., 2014. Detecting sinks and sources of CO2 and CH4 by ferrybox-based measurements in the Baltic Sea: Three case studies. *Journal of Marine Systems*, *140*, pp.13-25.

L220   Where were the studies conducted that are cited (Humborg and the other studies)? All studies in Tvärminne?

Yes, the studies cited in L220-221 were all conducted in the wider Tvärminne archipelago. Measurements by Myllykangas et al (2020) and Asmala & Scheinin (2023) were however conducted over a more extensive area, also including Pojo Bay and other bays. We have now replaced "Tvärminne Archipelago" with "southwestern coast of Finland".

L226   What are the saturation values referred to here? Perhaps you could mark them in Fig. 2.

The saturation values are presented in table 2 to which we now refer in the text. Similar to the chapter on CO_2 and CH_4, we have decided not to include the saturations in Figure 2.

Why no reference to Table 2 and A1 like above and not to the central Figure 2?

Here we do actually refer to Figure 2e in the original version of the manuscript and not to the Tables mentioned by the reviewer.

L228   Only one enclosed bay (HV) is shown in Figure 2e. Can we really then speak of a "clear pattern"?

We agree that, because only one enclosed bay (HV) appears in Figure 2e, the phrase "clear pattern" may imply a level of generality that our data cannot fully support. We have revised the text to avoid overstating the conclusion. Our intention was to highlight the consistent gradient observed *within the set of bays shown in the figure,* where N_2O concentrations decrease systematically from the two semi-open bays (SV, AV) toward the closed bay (HV). We agree that this pattern should not be generalized to all enclosed versus open bays.

L233   A reference to temperature-dependent enzyme activity is missing.

We rephrased the text to clarify that lower temperatures decrease the rate of N_2O reduction by denitrifying bacteria, but do not directly inhibit the nosZ enzyme or its gene expression. Instead, the reduction in activity is due to general metabolic slowdown at low temperatures, not specific inhibition of the enzyme itself. We have further added a reference to Wang et al., (2014) : "Under low-temperature conditions, enzymatic activity of N_2O reductase may be reduced, potentially slowing conversion of N_2O to N_2 and thereby increasing net N_2O emissions (Wang et al., 2014)."

L234ff   Where is the correlation between NO2+N03 and N2O shown? Why is it different in the bays? Formulate hypotheses and discuss them.

The correlation between NO_2+NO_3 and N_2O is now shown in the Spearman correlation analysis. To help interpret this outcome, we also discuss several plausible mechanisms that can decouple dissolved NO_2+NO_3 and N_2O in shallow coastal bays. These include: (i) spatial decoupling between water-column NOx and benthic N_2O production, (ii) dominance of complete denitrification to N_2 in organic-rich bay sediments, (iii) rapid biological uptake of inorganic N in the water column, (iv) advection and ebullition pathways that bypass dissolved NOx controls, and (v) methodological and scale differences relative to the Murray et al. (2015) synthesis.

L236ff   What is meant by "flow rates"? Please clarify!

We have replaced "flow rates" with "water currents" (here and elsewhere).

Why does this have an impact on denitrification? Please explain and provide references.

We have now rephrased the text as follows: "The consistently higher N_2O concentrations outside the bays may be explained by hydrodynamic and sedimentological conditions that favor coupled nitrification–denitrification (Marchant et al., 2016). Higher water currents enhance oxygen penetration into coarse sediments (sand, gravel, stones), which promotes nitrification in the oxic surface layer and denitrification in underlying anoxic microzones"

Marchant, H., Holtappels, M., Lavik, G., Ahmerkamp, S., Winter, C., & Kuypers, M. (2016). Coupled nitrification–denitrification leads to extensive N loss in subtidal permeable sediments. *Limnology and Oceanography*, 61. https://doi.org/10.1002/lno.10271.

Murray, R.H., Erler, D.V. and Eyre, B.D., 2015. Nitrous oxide fluxes in estuarine environments: response to global change. *Global change biology*, *21*(9), pp.3219-3245.

L238   Again, "flow rates." Does the author mean "currents"? Please clarify!

We have replaced "flow rates" with "water currents" (here and elsewhere).

"creating different biogeochemical conditions", creating different biogeochemical conditions, to what end? Please discuss why this has an impact and what kind of impact.

We have added the following sentence: "These conditions promote weaker ventilation, stronger sediment–water coupling, and lower oxygen availability, which tend to suppress nitrification and favour complete denitrification to N_2 rather than N_2O, ultimately reducing dissolved N_2O concentrations."

L238ff   "N2O production…", That's a little out of context again. I would suggest incorporating that into the introduction, where the processes have already been described.

We have now incorporated this paragraph into the introduction as per the reviewer's suggestion.

L242ff   Please establish a clearer link to the studies listed and discuss the differences/similarities (e.g. correlation with O2 concentrations). Can the study by Brunberg (2025) already be cited, as it is still under review? Why does the summer bloom not lead to a comparable N2O signal?

To establish a clearer link to the cited studies we have revised the text as follows: "Seasonal patterns in dissolved N_2O observed in our shallow Baltic Sea bays, with relatively high concentrations in spring (April) and lower concentrations in autumn (September/October), are consistent with patterns reported from other Baltic coastal settings. Long-term observations at the Boknis Eck time-series station in Eckernförde Bay likewise show elevated N_2O in winter and early spring followed by reduced concentrations in autumn, particularly under hypoxic or anoxic conditions (Ma et al., 2019). At that site, seasonal declines in dissolved oxygen and nutrient dynamics were closely coupled to N_2O variability, with lower autumn N_2O attributed to increased denitrification to N_2 under suboxic conditions that consume N_2O (Ma et al., 2019). Likewise, Cheung et al. (2025) identified pronounced seasonal N_2O variation in coastal Baltic waters and linked it to shifts in redox conditions and stratification that modulate microbial nitrification and denitrification pathways—processes that are

both oxygen sensitive and seasonally dynamic. In shallow bays, spring mixing and higher oxygen availability may enhance nitrification and partial denitrification, leading to relatively elevated N_2O, whereas prolonged summer stratification and oxygen depletion in late summer and early autumn favor complete denitrification and N_2O consumption, resulting in lower observed N_2O concentrations. These seasonally varying oxygen and nitrogen transformation dynamics offer a plausible mechanistic framework for the spring–autumn N_2O trend observed in our study."

We had hoped that the study by Brunberg et al. would be published by the time of our manuscript's review. As this is not the case, we have removed it throughout the manuscript and have added a reference to Geilfus et al. (preprint under open review at BG) from the same study area. However, contrary to Brunberg et al., the study by Geilfus et al. does not present seasonal data.

The absence of elevated N_2O concentrations during the summer bloom can be explained by several factors. First, summer blooms in the Baltic Sea are dominated by N_2-fixing cyanobacteria, which bypass the nitrification and denitrification pathways that normally generate N_2O. As a result, N_2O production during this period is minimal. In addition, summer surface waters are typically nitrate-depleted because phytoplankton rapidly consume available DIN, creating strong N limitation that further suppresses nitrification and denitrification. Ongoing photosynthesis keeps surface waters well-oxygenated, inhibiting denitrification and preventing N_2O accumulation. Finally, the reduced solubility of N_2O at higher temperatures enhances its outgassing, further dampening any potential signal.

Tab. 2-4 The results need to be presented differently. I recommend a graphical representation of the key results. Otherwise, the reader will be overwhelmed by the overloaded tables.

We would like to keep the tables in their current format as they provide the values, directly available for the reader. We agree with the reviewer that additional visualizations are needed to depict relationships between GHGs and environmental drivers which cannot be easily inferred from these tables. We have therefore added a Spearman correlation matrix to the main manuscript as well as scatterplots showing the spread in environmental and GHG data across bays and seasons in the appendix. That being said, we believe that the differences in GHG concentrations between bays are already visualized in Figure 2 and the spatial variations of GHGs within each bay are accessible in Figures A1-A6.

L249   Correlation between N2O and CH4. The entire chapter is written in a very unstructured manner. I recommend reconsidering this and introducing a logical, sequential structure.

The chapter is structured in a way that we first describe the negative correlation observed in all bays, then mention the shift to a positive correlation in the two bays with the highest CH_4 concentrations and in the following discuss potential drivers of this correlation shift. We believe this structure is logical. We have further extended the discussion of potential environmental drivers (see also reply to reviewer 2 and response to comment further below).

L253   The word "flow" is unclear.

We replaced "flow" with "water current".

L254   "high-energy environments", this needs to be expressed more clearly. What does the author mean? This similarity to earlier studies should be discussed in more detail.

By "high-energy environments," we refer to areas with strong hydrodynamic forcing—higher current velocities, turbulent flow, and coarse-grained substrates such as sand and gravel. These conditions

enhance oxygen penetration into sediments and promote efficient nitrification–denitrification coupling, processes known to stimulate N_2O production and release (Murray et al., 2015). We now clarify this in the text and expand the link to previous studies, explaining why such environments tend to act as N_2O hotspots.

"This pattern is consistent with previous observations that N_2O hotspots often occur in **hydrodynamically energetic settings**, where strong currents, turbulent mixing, and coarse substrates (sand and gravel) enhance oxygen penetration into sediments and stimulate nitrification (Murray et al., 2015). Such conditions also promote rapid porewater–water column exchange, facilitating the release of N_2O produced during coupled nitrification–denitrification. Our elevated N_2O concentrations in channels and outer-bay areas therefore align well with the mechanistic understanding established by earlier studies."

L256   The last sentence is a repetition of the first sentence in the paragraph. It can therefore be deleted (again, the word "flow"?).

We now removed the sentence as per the reviewer's suggestion.

L260   "indicative of the complex redox dynamics of these systems", what does the author mean by this? Please discuss the assumed processes/relationships that could be important in this context.

In the sentence, the reviewer is referring to, we replaced "redox dynamics" with "carbon-nitrogen-cycling dynamics". Furthermore, we have added a more in-depth discussion of potential processes driving changes in CH_4 and N_2O: "The different spatial distributions of CH_4 and N_2O may partly reflect their different optimal oxygen conditions: CH_4 production occurs mainly in anoxic regions, while N_2O production is maximal at suboxic levels near oxygen minimum zones where denitrification dominates (Naqvi et al., 2010; Ji et al., 2018; Barnes and Upstill-Goddard, 2018). Although our dissolved oxygen measurements in the central bay locations indicate generally oxic conditions in both Högklykeviken (O2,dissolved = 8.3mg L^{-1} ≈ 91% saturation) and Östra Myttingeviken (O2,dissolved = 5.6mg L^{-1} ≈ 59% saturation) we cannot resolve small-scale oxygen heterogeneity and therefore can only speculate that oxygen-reduced microenvironments may existed in areas of high CH_4 concentrations. Beyond oxygen availability, several additional mechanisms could explain the shift from a negative to a positive CH_4-N_2O correlation. As mentioned earlier, increased inputs of labile organic matter can stimulate methanogenesis further inside the bays, while changes in the availability of alternative electron acceptors (e.g., nitrate, sulfate, iron) alter competition among metabolic pathways, which can suppress or enhance methanogenesis and modulate N_2O production or consumption. Coupled processes such as nitrate-dependent anaerobic methane oxidation can also link CH_4 and N-cycling in non-linear ways. Ebullition would provide a pathway for CH_4 accumulation by bypassing water-column oxidation and decoupling CH_4 from dissolved N_2O dynamics. However, as mentioned previously, our measurement set-up does not allow us to discern between bubble-mediated and diffusive CH_4. Changes in rooted vegetation and bioturbation may further modify sediment oxygen penetration and bubble release, influencing the relative dominance of CH_4 and N_2O-producing pathways. Finally, sediment disturbance from the research vessel in very shallow areas could explain these anomalous patterns. In order to resolve which of these factors operates in our bays would require targeted process data, limiting our discussion to speculations."

L261ff   "Importantly,…" I don't understand the meaning of this sentence. Where can I find data on this (e.g., on the sediment properties in the bays studied)?

In the revised version, we removed the respective sentence.

L263   I would recommend merging the paragraph with the paragraph in L252.

Following the revisions, we believe that the current structure provides a natural and logical flow.

L264   The term "oxygen minimum zones" may be unsuitable for shallow water studies. Please reduce the number of references.

We have removed "oxygen minimum zones" as well as the references to Foster & Fulweiler (2016), Ji et al. (2015) and Tang et al. (2022) to reduce the number of references to three.

L267   "91%", please explain in more detail what these values are.

We now specify in the text that these are saturation values.

L268ff   O2 was measured after all. Where is there a figure showing whether O2 and CH4 correlate with each other?

Dissolved oxygen was only measured in one central location of the bay together with other seawater properties (see also table 3). As such, it is unfortunately not possible to correlate O_2 and CH_4 at the same resolution as CH_4. However, a correlation plot of the mean CH_4 with O_2 per bay is included in the Spearman correlation analysis and scatterplots in the appendix which we added to the manuscript as mentioned in an earlier comment.

L270ff   Where are the local N2O peaks shown (figure)? Again in the appendix?

Yes, these peaks are visible in Figure A6 c and f in the appendix, although the sentence referring to these local peaks has now been replaced in the updated manuscript.

          Wind induced mixing can also stir up water that is low in O2 and rich in N2O from the water body near the seabed?

As we already mentioned in a response to a previous comment, the bays were very sheltered and wind speeds were generally very low inside the bays. Therefore, it is unlikely that wind induced mixing contributed to local hotspots inside the bays. It might however have contributed to the overall higher concentrations outside the bay areas. We have added this to the discussion in sections 3.1.3 and 3.1.5: "Higher N_2O concentrations outside the bays may partly reflect wind-induced mixing in the more exposed areas, where longer fetch and higher wind speeds enhance vertical exchange and stimulate nitrification–denitrification dynamics. In contrast, the sheltered bay interiors experience reduced wind forcing, limiting mixing and potentially suppressing N_2O production and release."

L278   What is the significance of N2 fixation in relation to your own data? Does this need to be pointed out here?

We thank the reviewer for this comment. We agree that nitrogen fixation is not necessarily linked to the CH_4–N_2O relationship discussed here and is not relevant for interpreting our data. We therefore removed the reference to aluminum-induced nitrogen fixation to avoid confusion.

Conclusion

L315   "This study provides a comprehensive assessment of GHG emissions…", "comprehensive" is perhaps the wrong word, as this is only a very limited study in selected basins.

We replaced "comprehensive" with "spatially resolved".

L321   What is meant by "generally dominated fluxes"? What kind of flux? Please rephrase.

We have rephrased the text as follows: "generally dominated **CO2-equivalent** fluxes"

L322   "N2O showed opposite seasonal trends with higher concentrations outside the bays"…and lower concentrations in fall. Please complete the statement.

We added "and lower concentrations in fall than in spring" as suggested by the reviewer.

L323   This statement is still very weak and needs to be further extended in the discussion (in 3.1.4). How can this be explained? What does it indicate? Why is it only noticeable in some basins? Ideas should be developed here (or better in the discussion section).

As addressed in a response to an earlier comment, we have now extended the discussion of potential drivers of the CH_4-N_2O correlation. The text was rephrased as follows: "We observed a change in the relationship between CH_4 and N_2O, with negative correlations at CH_4 concentrations below 250 nmol L^{-1} and positive correlations at higher concentrations. To our knowledge, such a pattern has rarely been reported for shallow coastal bay environments and highlights the complexity of coupled nitrogen and carbon cycling under variable redox and hydrodynamic conditions. This shift likely reflects a transition from conditions where nitrification and coupled nitrification–denitrification dominate to more reduced, microbially active regimes in which methanogenesis become more prevalent"

L326ff   "insights into the potential environmental controls on coastal GHG dynamics", This is where the manuscript's weakness lies. The "controlling drivers" need to be better identified and presented. A clear classification is not apparent in the text.

We have added a Spearman correlation analysis to the revised manuscript and have added the following text to the conclusions: "By placing GHG concentrations and fluxes in the context of measured environmental parameters, this study identifies observational relationships between bay characteristics and seawater properties with variability in coastal GHG dynamics. CO_2 was negatively correlated with chlorophyll a and pH and positively correlated with LOI and rooted vegetation, while CH_4 was negatively correlated with dissolved oxygen and positively correlated with LOI. N_2O was negatively correlated with seawater temperature, TN, total vegetation and rooted vegetation. At the same time, the pronounced spatial and temporal heterogeneity across bays and seasons, together with the limited number of study sites, constrained our ability to quantitatively attribute individual drivers, underscoring the need for targeted process-based studies to resolve the mechanisms underlying these patterns."

L328   "high spatial … variability", does this refer to spatial variability in individual basins (e.g., inside/outside bays)? To this end, figures from the appendix (e.g. Fig. A1) should be transferred to the main text and discussed in greater detail (perhaps only for selected bays).

This sentence refers to spatial variability at multiple scales—between bays, within individual bays, and between inside and outside bay areas. We agree that these patterns are important, which is why they are shown in detail in Figures A1–A6. However, transferring all spatial maps to the main text would substantially increase the length of the manuscript and disrupt its flow. We therefore chose to keep these figures in the appendix, which is part of the manuscript (unlike a separate supplement) and immediately accessible to the reader. We believe that the spatial variability on between bay and

between inside and outside bays is already addressed in section 3.1.1-3.1.3, while drivers for the variability between and within these bays are discussed in sections 3.1.4 and 3.1.5, respectively.

L332ff "overlooked but important component", "overlooked" is probably not the right word. We know that they are important. If so, they are undersampled (spatial heterogeneity) and the processes that control greenhouse gas dynamics are poorly understood.

We thank the reviewer for their comment and have now replaced "overlooked" with "understudied".

L336ff How can "EC flux measurements and monitoring of seawater" help to better identify the biological drivers (methanogenic and methanotrophic; both in seawater?)? Please rephrase.

We thank the reviewer for this comment. We have revised the sentence to clarify that it is the combination of EC flux measurements with detailed biogeochemical and microbial monitoring—rather than EC alone—that would help link flux variability to underlying methanogenic and methanotrophic processes. "[…] long-term monitoring that combines eddy-covariance flux measurements with high-resolution monitoring of seawater chemistry, oxygen levels, sediments, and microbial community composition. This combined approach would enable attribution of CH_4 flux variability to specific biogeochemical drivers, such as methanogenic production in sediments and methanotrophic consumption in the water column."

L338ff Please include the problem of not capturing gas bubble fluxes in the discussion and discuss it based on existing literature (e.g., Bisander et al. 2025).

In the revised manuscript, we added a discussion of not capturing bubbles using our measurement system. We have further added references to Bisander et al., 2025 as well as McGinnis et al., 2006 and Hermans et al., 2024: "Another factor that can contribute substantially to CH_4 emissions in shallow, organic rich sediments is ebullition (McGinnis et al., 2006; Hermans et al., 2024; Bisander et al., 2025). Recently, Bisander et al. (2025) showed that ebullition from sandy sediments can be substantial. The WEGAS system measures CH_4 from both benthic diffusion and bubble dissolution. Consequently, the observed CH_4 concentrations represent the combined effect of these pathways, and without isotopic information we cannot distinguish between diffusive transport and ebullition. Although no visible bubbling was observed during sampling, we cannot exclude the possibility that episodic ebullition events might have impacted our measurements. This measurement limitation should be considered when interpreting the relationships between CH_4 and the environmental parameters described above"