Reply to reviewer 1

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 comments

The study investigates an important topic, GHG emissions and dynamics on shallow coastal areas. Every single study is important in gathering more experimental data and it is not necessary to have all aspects addresses in single study. I recommend publishing this study after addressing a few minor issues in the final version of the manuscript.

We thank the reviewer for their positive feedback and input.

Introduction

The introduction is fluently written and covers the relevant literature.

Observations

The observations are done using suitable instrumentation. The study does not describe the calibration methods for the gas analysers or whether standard gases were measured during the experiments as a reference. Could be beneficial to add few words about the potential field calibrations.

Thank you for this comment. We have now added detailed information on sensor calibration and instrument performance to the Methods section. 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. According to the manufacturer's specifications, the 1-min precision is <300 ppb + 0.05 % of reading (typical 74 ppb) for CO_2, <7 ppb + 0.05 % (typical 0.1 ppb) for CH_4, and <10 ppb + 0.05 % (typical 1.1 ppb) for N_2O. These values are several orders of magnitude smaller than the concentration ranges measured in our study (CO_2: 300–1000 ppm; CH_4: up to 250 ppm; N_2O: 0.34–0.36 ppm), ensuring that instrumental precision does not constrain the interpretation of our results.

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.

Results

The study provides a short glance on GHG dynamics on a few coastal bays in Sweden, with high variability observed. The observations were done always around the noon. While this ensures possibility to compare the results between the locations, the study cannot provide any information on diurnal variation and impact of meteorological conditions (e.g. solar radiation) influencing significantly the biogeochemistry and sea-atmosphere fluxes, especially during the biologically active period. Please address these issues when generalizing the results. The study covers only two seasons, spring and autumn – what is the expected seasonal behavior of concentrations and fluxes?

We thank the reviewer for the opportunity to clarify the scope and limitations of our study. We agree that GHG fluxes can vary considerably over the diel cycle, particularly during periods of high biological activity, and that conducting measurements only around noon constrains our ability to assess this diurnal variability and the influence of short-term meteorological. Regarding seasonality, we agree that covering only spring and autumn limits our ability to describe full annual dynamics. As such, our measurements represent snapshots of two transitional periods, which we already highlighted in the discussion. Based on previous annual studies from similar environments one can expect CO_2 undersaturation/ uptake during spring that can be related to phytoplankton and algae growth, and elevated CO_2 and CH_4 concentrations/ eflux in late summer/autumn due to decaying biomass, while N_2O peaks in spring can be associated with decaying spring phytoplankton bloom. This agrees well with our observations. In winter, ecosystems are less biologically active, leading to decreasing concentrations in the seawater.

We have now clarified these limitations in the revised manuscript (section 3.1.1, added text in bold): "However, our measurements represent only snapshots from two seasons and capture transitional states rather than peak or minimum seasonal conditions. In temperate coastal environments, growth of phytoplankton and algae in spring reduces pCO_2 in the water column, while biomass decay in fall results in elevated pCO_2. Recent studies by (Honkanen et al., 2021) and Pönisch et al. (2025) reported diurnal variability in surface-water pCO2 and CH4 in the Baltic Sea that could be linked to biological and physical drivers such as solar radiation, temperature or biological activity. We acknowledge that our measurements, which were always conducted around noon, do not capture these diurnal fluctuations and thus likely introduce a small but systematic bias relative to true daily mean conditions. While such measurements remain valuable**,** more extensive, long-term monitoring is required to identify the environmental parameters that drive these systems to function as CO_2 sources or sinks across different temporal scales.**"**

pCO2, pCH4 and pN2O has been measured for long period throughout the Baltic Sea on ICOS VOS line between Travemünde and Helsinki. Please compare the coastal concentrations with concentrations observed over the open sea and give a rough estimate how many percent of the shallow and bay areas cover the Baltic Sea. This will give an indication of the impact of shallow areas on total GHG budget of the Baltic Sea.

We have added a comparison to the CO_2 and CH_4 data from the open Baltic Sea measured on the Finnmaid ferry. We could not find any long-term, processed N_2O data. The following text was added to the respective sections:

"Long-term measurements across the open Baltic Sea, that were conducted on the Finnmaid ferry between Travemünde and Helsinki (Bittig et al., 2023), reported values ranging between 18–1238 µatm (mean 293±60 µatm) in April and 14–1198 µatm (mean 375±50 µatm) in September. Similar measurements by Schneider et al. (2014) yielded values of < 200 µatm in summer and ~400 µatm in September."

"The CH4 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)"

According to Roth et al. (2022) the area of shallow bays with depths under 5 m amounts up to 30,000 km^2 which equals around 7% of the total area of the Baltic Sea. This estimate is based on a digital bathymetric model (EMODnet) presented in Jakobsson et al. (2019). As this estimate also includes shallow areas that are not bays, the percentage coverage of sheltered, shallow bays such as those studied in this study, is likely even smaller. We have added the following sentence to the introduction: "Shallow areas with water depths < 5 m comprise up to about 30,000 km^2, or roughly 7% of the Baltic Sea (Roth et al., 2022; Jakobsson et al., 2019), though the coverage of sheltered shallow bays, such as those investigated in this study, is likely smaller. Focusing only on the Stockholm and Uppsala archipelagos, Åland islands, and southwestern Finnish archipelago, these shallow, enclosed bays cover approximately 142 km^2 (Gubri et al., 2025)."

Furthermore, we now also use the area estimate of 30,000 km^2 in section 3.3 to derive an upper estimate of the total CO_2-equivalent fluxes from shallow areas in the Baltic Sea. The adjusted text now reads: "[…] to constrain potential regional contributions, we scaled our total CO_2-equivalent fluxes using two area estimates: (1) the total area of shallow, enclosed bays in the archipelagos around Stockholm, Uppsala, Åland and southwestern Finland (142 km^2, Gubri et al., 2025) as a lower estimate and (2) the total area shallower than 5 m in the Baltic Sea (∼ 30,000 km^2, Jakobsson et al., 2019; Roth et al., 2022) as an upper estimate. The resulting total carbon fluxes ranged from -7.5 t C d^{-1} to 30.7 t C d^{-1} (median 5.1 t C d^{-1}) for the lower limit and - 1596 t C d^{-1} to 6492 t C d^{-1} (median 1076 t C d^{-1}) for the upper limit".

Specific comments

Table 2

Östra Lermaren, please check the values for pCO2 and correct.

We have checked and corrected the values for pCO2.

Please re-check the pCO2 saturation calculations

We have re-checked the pCO2 saturation calculations and corrected the values in the table.

There are a couple of typos (comma vs dot), please correct.

We have corrected the typos pointed out by the reviewer.

How sensitive are the results for the choice of selected wind speed (2 m/s) used in the study? Please calculate the fluxes using wind speeds of 1 m/s and 5 m/s and describe the impact of choosing 2 m/s on the results.

Based on the reviewer's suggestion we estimated the fluxes at different wind speeds (see Figure 1 below). The differences in the mean fluxes for wind speeds between 1-5 m s^{-1} are relatively small (ΔF_{CO2}=22 mg m^{-2} d^{-1}, ΔF_{CH4}=0.84 mg m^{-2} d^{-1}, ΔF_{N2O}=0.03 mg m^{-2} d^{-1}), while the range between minimum and maximum values increased significantly at higher wind speeds. However, in response to comments from reviewer 2 and 3, we now use ICON-EU model wind speed data from the respective sampling days and locations for a revised version of the flux estimations.

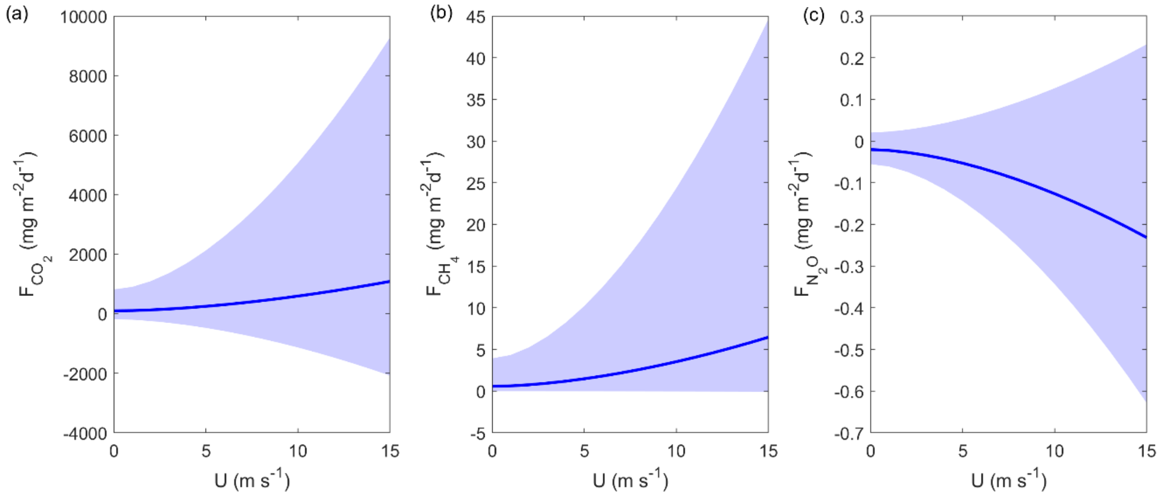


Figure 1 Wind dependent fluxes of (a) CO_2, (b) CH_4 and (c) N_2O (averaged over all bays in September). The line represents the mean value at different wind speeds, while the shaded area represents the range between minimum and maximum value.

In shallow waters, ebullition may be important for CH4 fluxes – please shortly address its potential importance on calculated methane fluxes in the paper.

We agree that ebullition can be an important pathway for CH_4 emissions in shallow coastal systems, particularly in sheltered or organic-rich environments where sediment methane production is high. Due to the lack of isotope measurements, we cannot differentiate between contributions from diffusive exchange and ebullitive fluxes. While we did not observe visual signs of bubbling during sampling, we acknowledge that episodic ebullition events may have contributed to total CH_4 emissions. We have added the following text to section 3.1.2: “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.”

Data availability: EGU journals require open data policy. As the most important research contribution of this study is the dataset collected, please submit the data to MEMENTO and SOCAT databases after the publication of the paper so that they will be available for the Baltic Sea GHG research community in the future.

We are planning to make the data openly available through the MEMENTO and SOCAT databases once the manuscript revision has been finalized.

Reply to reviewer 2

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.

The manuscript by Zinke et al. presents data of the concentrations (and fluxes) of the three major greenhouse gases CO2, CH4, and N2O in a variety of shallow bays in the wider Stockholm Archipelago, Baltic Sea, using continuous recording of a system with an air-sea equilibration system coupled to a Picarro G 2508 CEAS. Measurements were done during two seasons, around midday, and a variety of potentially controlling environmental parameters was recorded as well. The authors use these data to calculate fluxes using the ASE parameterization developed by Cole and Caracoa (1998) for lake environments.

While the data set is interesting and the question of GHG emissions from shallow coastal waters and its potential changes is timely, the paper unfortunately does not explain parts of the methods and approach, and despite the fact that the authors claim the need to find ways to address the large spatiotemporal variability of GHG fluxes from these environments, they do by far not fully exploit their data set to answer these questions by e.g. stringent correlation analysis. The only correlation they address in more detail is the one between N2O and CH4, while the physical drivers including the - nicely introduced – topographic openness index – are not addressed in a systematic way.

We thank the reviewer for their thorough review. In the revised version we included a Spearman correlation analysis. Since the environmental parameters were only measured in one location of each bay, correlating these parameters across all bays only yields four data points in April and six data points in September/October. Given the limited number of data points we have therefore decided to pool the data from both April and September/October. A more detailed discussion of this matter will be addressed in the respective comment below.

Therefore, I can recommend publication of the paper only after major revisions.

I will start with some general comments and then get more detailed .

Scope: in the last paragraph of the introduction, the authors indicate the scope of their study, in particular naming the spatio-temporal variability and the examination of potential control parameters. However, they do neither discuss the limits of their approach towards these goals nor fully exploit them.

We thank the reviewer for this important comment. In the revised manuscript, we have explicitly clarified the scope and limitations of the study in the final paragraph of the Introduction and expanded on these points in the discussion. Specifically, we now acknowledge that the temporal resolution is restricted to two seasonal snapshots and midday measurements, precluding a full assessment of diurnal variability. At the same time, we have strengthened the exploitation of the dataset by adding correlation analyses between GHG concentrations and key environmental variables, and by more explicitly linking observed spatial patterns to potential physical and biogeochemical drivers. These additions better align the analyses with the stated objectives while maintaining a clear distinction between supported findings and more speculative interpretations.

Variability: it is not really clear how and when the sampling was done exactly. The authors state that the measurements were done during midday (line 81), but also describe a 45min measurement cycle which was repeated several times. From the details of the measurements mapped in the Figs Appendix

A1-A6, it appears that for the inner bays, only the long-term measurement at the positions indicated by red triangles were used. For "outside Bay area", there is no stationary point indicated so apparently the data outside a line indicating the boundary between inner- and outside bay were pooled. The approach of these outside Bay area measurements is not explained at all in the text, nor the selection of the separation or the question how "out" these areas actually are (in terms of connection to the open Baltic, residence time, depth etc.). These approaches should be explained in more detail.

Continuous flow-through measurements of surface water GHG concentrations were conducted around noon (typically between 11 AM and 2 PM) over a time scale of 60-90 minutes (typically ~75 minutes) depending on the size of the bay. The long-term measurements marked with the red triangle in figures A1-A6 are only for seawater properties (including nutrient concentrations, dissolved oxygen, chlorophyll-a, LOI, turbidity, pH, temperature and salinity). Temperature and salinity were also measured in the WEGAS system using a thermosalinograph – this data is available for every GHG data point. No long-term monitoring was conducted outside the bay area. We have now expanded the description of how inner versus outer bay areas were defined by adding the following sentences to section 2.2.1: "To distinguish between "inner bay" and "outer bay" sampling points, we delineated the bay boundary at the narrowest part of the inlet connecting each bay to the open Baltic Sea. This location represents the transition in water exchange, residence time, and mixing characteristics."

Temporal variability: while it is correct that the instrumentation used by the authors can be used to tackle temporal variability, it is not used that way in this study. Basically, the authors claim to have done experiments over midday, and it is not clear what time frame is represented in their individual data sets (as shown in the Figures A1-A6 of the appendix; a few hours is my guess). There is a complete lack of discussion on diurnal cycling and potential bias, in particular on the fluxes, while very relevant studies on the topic exist, e.g. the study by Honkonen et al. 2021 on diurnal variability of pCO2 fluxes at Utö (really nearby) or the recent study of Pönisch et al. (2025) on summerly GHG fluxes from a rewetted peatland (shallow coastal water). The latter has a detailed analysis on diurnal variation and the effect and biases this might have on GHG flux calculations, which the authors should address.

In the context of our study "temporal variability" refers more to seasonal differences between April and September. However, since our study provides only snapshots from two months and does not cover a full annual cycle, we would like to refrain from using the wording "seasonal". As already addressed in the previous comment, measurements lasted between 60-90 minutes (typically ~75 min). As such, the reviewer is right to point out that our sampling approach of measuring around midday does not allow to resolve diurnal cycles, leading to a systematic bias. We have added a discussion of this limitation along with references to Honkanen et al. (2021) and Pönisch et al. (2025) to the discussion (see corresponding comment further below). Furthermore, to avoid any confusion we removed "temporal" from the title and from the research aims stated at the end of the introduction.

Flux calculations: The authors should consider whether the flux calculations should be part of the paper. The strength of the manuscript is the measurement of concentrations and potential relation to controlling parameters. The flux calculations are based on the concentrations measured, a chosen ASE model (without discussion of the choice), and with a lack of wind data. If the flux part should be incorporated, it would be essential to

- discuss the potential daily bias in these shallow systems (see above).

We added a discussion of the daily potential bias in the context of under-/overestimation of our fluxes. A discussion is added to the corresponding comment below.

- to at least get access of wind data from an adjacent wind station of a reliable wind product. Just assuming an average wind speed of 2m per second is not state of the art.

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.

Importantly, field observations indicated that actual wind speeds at the water surface inside sheltered bays were low, often much lower than the 10-m model wind. This is expected due to sheltered nature of the bays, with limited fetch. 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.”

- To discuss the chosen wind model. This is of particular importance, as the wind model used in this study has a considerable wind-independent additive term, which dominates at windspeeds < 3.5 m/s. For systems like the ones investigated, there is no right or wrong choice, but some justification would need to be given. Way more important is that it would be important to look into the wind parameterizations used in the flux estimates worldwide the authors compare their results with (Table 5), which likely mostly used parameterizations without such a strong low-wind component.

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:

"Estimates of air–water GHG fluxes are sensitive to the choice of gas-transfer velocity parameterization. In this study, we applied the formulation by Cole & 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."

Assessment of drivers and potential controls:

The authors nicely introduced potential drivers of GHG production in the intro and beginning of the method, and took measures to investigate those (T,S, topographical openness, organic matter content, nutrient concentrations, …). However they report correlations only sporadically and without a sound estimate of significance etc. This is already documented in the separation of the GHG data and the "auxiliary" data in tables 2-4. The authors should try to give statistical underpinned information on correlations and potential driver analysis. A very good example for this is the use of Spearman correlation coefficients and effect size attribution as e.g. in Pönisch 2025.

We had initially considered to include a Spearman correlation analysis. However, since the environmental parameters were only measured in one location of each bay, correlating these parameters across all bays only yields four data points in April and six data points in September/October. Given the limited number of data points, most correlations were not significant if presented separately for April or September/October and we therefore initially decided not to include this analysis in the manuscript. In the revised version we have decided to combine the data from both periods and moved the discussion of environmental drivers from chapters 3.1.1-3.1.3 to a separate chapter. In cases were correlations were at or slightly above the significance level of 0.05, we refer to these correlations as "correlation trends".

"**Correlation of surface water GHG concentrations with environmental parameters across bays**

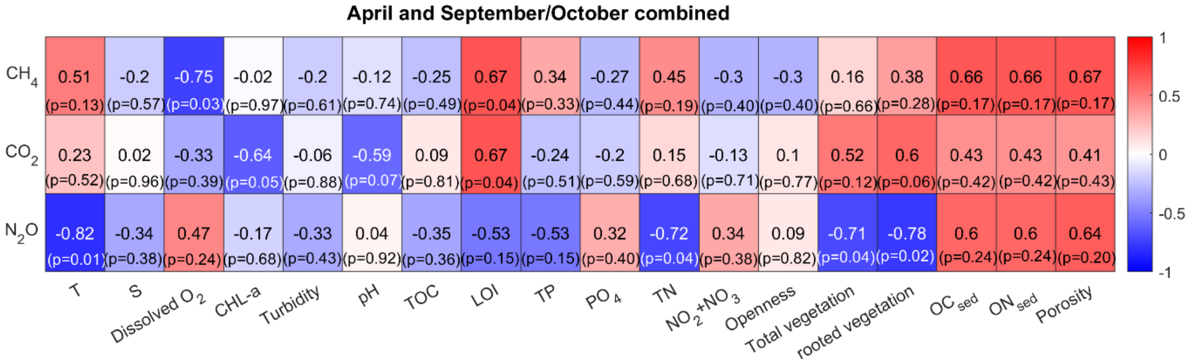
To identify environmental factors associated with variability in surface-water GHG concentrations, we conducted a Spearman's rank correlation analysis using bay-averaged GHG concentrations and environmental parameters measured in the center of each bay (Fig. 3). To increase statistical power and assess general trends, data from April and September/October were pooled.

CO_2 concentrations were positively correlated with LOI ($r = 0.67$, $p = 0.04$) and showed negative correlation trends with chlorophyll-a ($r = -0.64$, $p = 0.05$) and pH ($r = -0.59$, $p = 0.07$) as well as a positive correlation trend with rooted vegetation cover ($r = 0.60$, $p = 0.06$). The negative relationship with chlorophyll-a and pH suggests that periods or locations of enhanced primary production are associated with CO_2 drawdown and elevated pH, whereas positive correlations with LOI and

vegetation indicate that respiration and mineralization of organic matter—particularly from macrophyte-derived inputs—can offset photosynthetic uptake and elevate CO_2 concentrations in surface waters. This interpretation is supported by the observation that the bays with the highest CO_2 concentrations (Östra Lermaren, Östra Myttingeviken, and Bodviken) shared extensive rooted vegetation cover and elevated sediment organic carbon content. In Östra Lermaren and Östra Myttingeviken, which also exhibited the lowest eutrophication status as measured by TP and chlorophyll-a, high CO_2 concentrations may appear counter-intuitive but are likely driven by substantial autochthonous organic matter inputs from decaying vegetation, consistent with coastal studies documenting seasonal CO_2 hotspots linked to remineralization of organic-rich material (Amaral et al., 2021; Asmala and Scheinin, 2024). In contrast, Bodviken combined high CO_2 concentrations with comparatively higher eutrophication, suggesting that enhanced internal mineralization under nutrient-rich conditions may dominate CO_2 production in this system.
Although the correlations with pH and rooted vegetation were slightly above the conventional 5% significance threshold, they are consistent with the expected coupling between primary production, organic matter mineralization, and CO_2 dynamics in shallow coastal systems. Given the limited number of bays, these near-significant relationships should be interpreted as exploratory and warrant confirmation through studies with higher spatial and temporal resolution.

CH_4 concentrations showed a significant negative correlation with dissolved oxygen ($r = -0.75$, $p = 0.03$) and a positive correlation with loss on ignition (LOI; $r = 0.67$, $p = 0.04$). These relationships are consistent with enhanced methanogenesis under low-oxygen conditions and increased availability of degradable organic substrates in the water column, which together promote CH_4 production and accumulation.

In contrast, N_2O concentrations exhibited significant negative correlations with temperature ($r = -0.82$, $p = 0.01$), total nitrogen (TN; $r = -0.72$, $p = 0.04$), total vegetation cover ($r = -0.71$, $p = 0.04$), and rooted vegetation ($r = -0.78$, $p = 0.02$). These patterns suggest that warmer, more vegetated bays with higher nitrogen availability may favor complete denitrification to N_2 or enhanced biological nitrogen uptake, thereby reducing N_2O accumulation in the water column. In addition, vegetation-associated oxygenation of surface sediments may suppress N_2O-producing pathways while promoting N_2O reduction, contributing to lower observed N_2O concentrations.


April and September/October combined

	T	S	Dissolved O_2	CHL-a	Turbidity	pH	TOC	LOI	TP	PO_4	TN	NO_2+NO_3	Openness	Total vegetation	rooted vegetation	OC_{sed}	ON_{sed}	Porosity
CH_4	0.51 (p=0.13)	-0.2 (p=0.57)	-0.75 (p=0.03)	-0.02 (p=0.97)	-0.2 (p=0.61)	-0.12 (p=0.74)	-0.25 (p=0.49)	0.67 (p=0.04)	0.34 (p=0.33)	-0.27 (p=0.44)	0.45 (p=0.19)	-0.3 (p=0.40)	-0.3 (p=0.40)	0.16 (p=0.66)	0.38 (p=0.28)	0.66 (p=0.17)	0.66 (p=0.17)	0.67 (p=0.17)
CO_2	0.23 (p=0.52)	0.02 (p=0.96)	-0.33 (p=0.39)	-0.64 (p=0.05)	-0.06 (p=0.88)	-0.59 (p=0.07)	0.09 (p=0.81)	0.67 (p=0.04)	-0.24 (p=0.51)	-0.2 (p=0.59)	0.15 (p=0.68)	-0.13 (p=0.71)	0.1 (p=0.77)	0.52 (p=0.12)	0.6 (p=0.06)	0.43 (p=0.42)	0.43 (p=0.42)	0.41 (p=0.43)
N_2O	-0.82 (p=0.01)	-0.34 (p=0.38)	0.47 (p=0.24)	-0.17 (p=0.68)	-0.33 (p=0.43)	0.04 (p=0.92)	-0.35 (p=0.36)	-0.53 (p=0.15)	-0.53 (p=0.15)	0.32 (p=0.40)	-0.72 (p=0.04)	0.34 (p=0.38)	0.09 (p=0.82)	-0.71 (p=0.04)	-0.78 (p=0.02)	0.6 (p=0.24)	0.6 (p=0.24)	0.64 (p=0.20)

Figure 3 . Spearman correlation matrix between environmental parameters and CH_4, CO_2 and N_2O pooled for April and September/October. Blue indicates a negative correlation, red indicates a positive correlation. Significance (at the 95% confidence level) is indicated by p-values.

Additional to the heatmap plot, we added scatterplots to the appendix to visualize the relationships color-coded by season (see figures below).

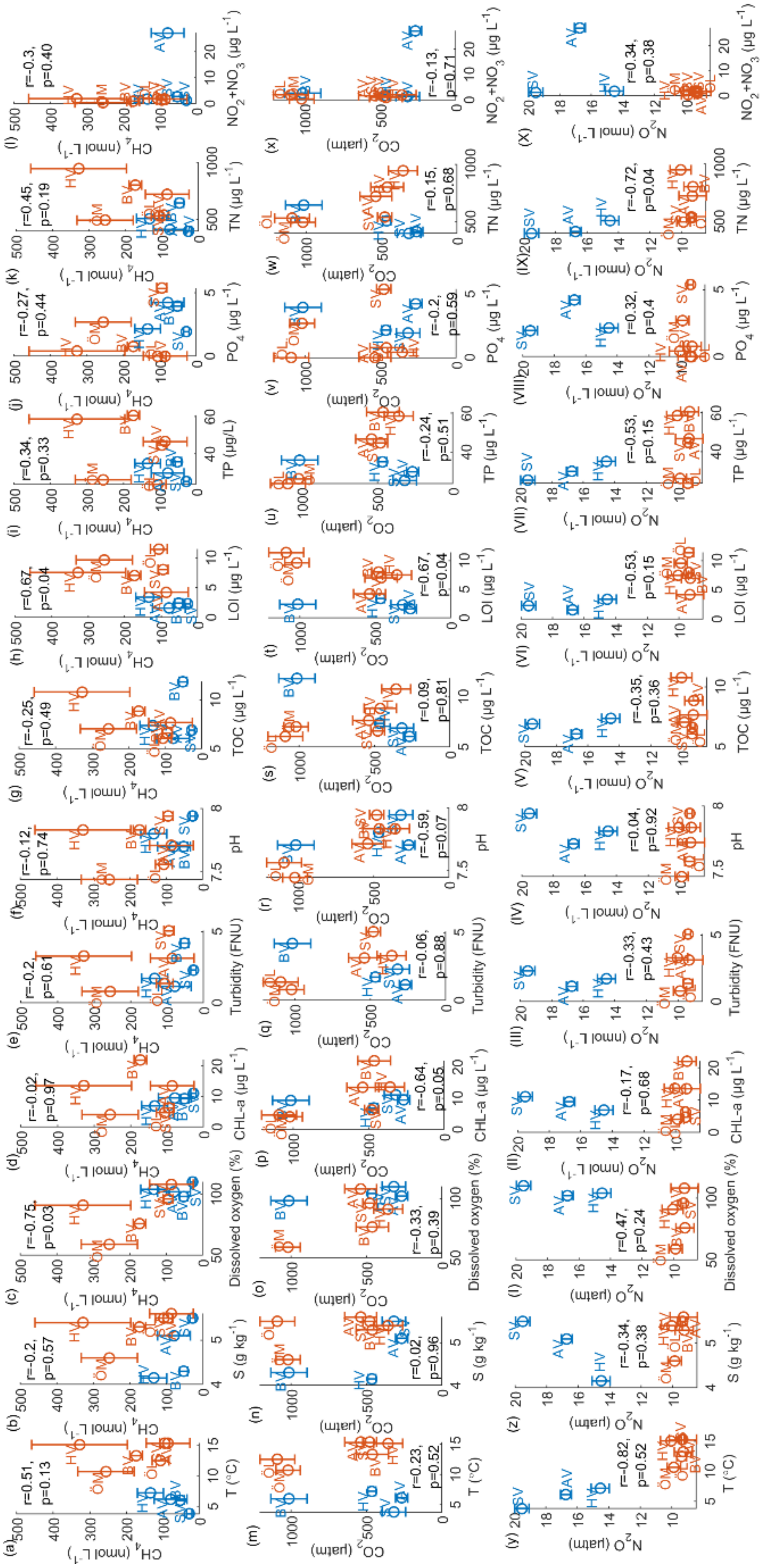


Figure A7: Relationships between seawater properties and GHG concentrations.

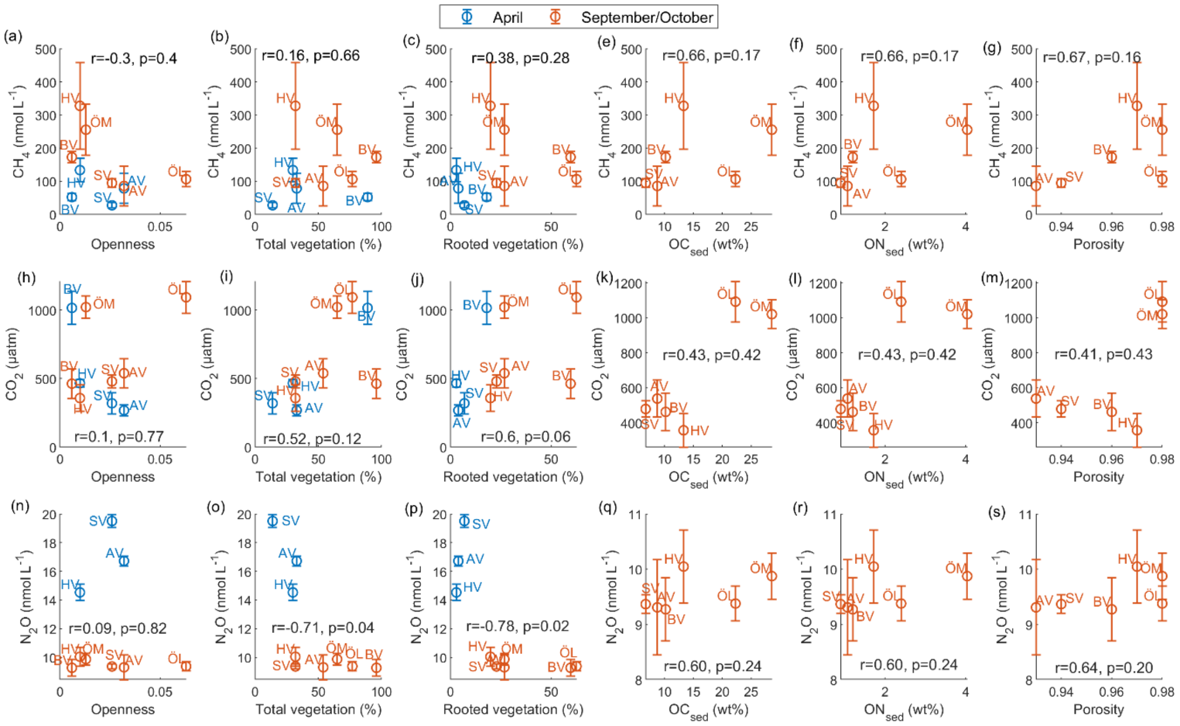


Figure A8: Relationships between bay characteristics (openness, vegetation cover and sediment properties) and GHG concentrations. Sediment data is only available for September/October.

The only quantitative analysis of correlations in this text is the one between N2O and CH4. While some of the discussion point in this section are really interesting, the interpretation of the correlation (and of the discussion on CH4 and N2O concentration suffers from the beforementioned lack of detailed assessment of the physical drivers. Here, for instant, it is clear that the main control of N2O is temperature, and this is likely also true (mentioned in the text) for methane, in the opposite way, as in the one case, control on solubility is dominant (N2O), while in the other, the control on production seems more important (CH4).
Stringent analysis could potentially help to reveal why and when the inverse correlation of CH4/N2O changes the direction at CH4 higher 250 nM.

We thank the reviewer for their valuable comment. We would like to clarify that the temperature for the measurements in Högklykeviken in September did not vary significantly (15.5°C inside the bay versus ~14°C outside the bay). As such we would like to argue, that the temperature effect on solubility and GHG production is likely minor. We added a discussion of alternative mechanisms that can change the CH_4–N_2O relationship in shallow bays. Unfortunately, sediment and seawater properties are only sampled in the center of each bay, limiting our discussion to speculations. In order to resolve which of these factors operates in our bays would require targeted process data. While porewater profiles and microbial gene assays were collected in the center of each bay on each sampling day, these yet remain to be analyzed and are beyond the scope of the present study.

We added the following text to the manuscript: "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., 2015, 2018; Foster and Fulweiler, 2016; Barnes and Upstill-Goddard, 2018; Tang et al., 2022). 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."

As pointed out correctly by the authors, the high variability in shallow coastal waters need more data, but also the best possible research to assign drivers and controlling parameters. Here, the paper could be largely improved.

We thank the reviewer for this comment. We agree that the high variability observed in shallow coastal systems highlights the need for both expanded datasets and robust analytical approaches to identify controlling drivers. In the revised manuscript, we have strengthened the analysis by adding correlation analyses between GHG concentrations and key environmental parameters, linking observed spatial patterns to potential physical and biogeochemical controls, and clearly distinguishing between statistically supported findings and more speculative interpretations in the Discussion. At the same time, we acknowledge the limitations of the present dataset with respect to fully resolving causal mechanisms.

Some areas of concern:

Askö Data: In Section 3.1.4, for the N2O -CH4 relationship, the data from observations in Asko are suddenly introduced, completely "out of the hat". Asko is a different setting, the method, sampling etc. has not been introduced, and there is no real link to the rest of the study. It honours the authors that they want to publish the data set, but it does not connect to the rest of the study. So I strongly recommend to remove this part, which is not introduced in the methods or site description, nor attached to the study. If these data are "left overs" they can be easily uploaded with relevant metadata to a publicly available data base. It appears that the authors were a little unsure here themselves, as if I am not mistaken, the data are not plotted correctly (Fig 3a). I am very sure that the data referenced as March data are actually September data and vice versa.

Based on the reviewer's suggestion, we removed the data from Askö in the revised version.

Method information:

There is a lack of information on some of the methodological aspects: how and when was the sensor calibrated with which kind of calibration gases? What is the expected accuracy / precision of the measurements? Which data were used for the calculation of the concentrations of the "inner bay" and the outside Bay (see further above). When calculating mean values (e.g. for fluxes), were they averaged simply over the No of regions, or were they area-weighted. Please add the information needed to assess the methodological part of the manuscript.

Thank you for this comment. We have now added detailed information on sensor calibration and instrument performance to the Methods section. 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. According to the manufacturer's specifications, the 1-min precision is <300 ppb + 0.05 % of reading (typical 74 ppb) for CO_2, <7 ppb + 0.05 % (typical 0.1 ppb) for CH_4, and <10 ppb + 0.05 % (typical 1.1 ppb) for N_2O. These values are several orders of magnitude smaller than the concentration ranges measured in our study (CO_2: 300–1000 ppm; CH_4: up to 250 ppm; N_2O: 0.34–0.36 ppm), ensuring that instrumental precision does not constrain the interpretation of our results. 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.

We have added a description of how inner versus outer bay areas were defined (see response to comment above).

Furthermore, we have clarified how mean values were determined in the methods section and throughout the manuscript. The following sentence was added to the methods description: "For cross-bay comparisons, concentrations were first averaged within each bay, and summary statistics (e.g., median) were then calculated across bays using one value per bay, treating each bay as an independent unit rather than applying area-weighted averaging."

Missing N2O data for Bodviken: this should be explained. It is mentioned several times in the text, and difficult to understand, as the authors use an instrument measuring all 3 gases simultaneously, and usually the instrument fails completely or not; there is surely an explanation, but please explain in the text.

Due to technical issues, measurements in Bodviken in April were conducted with a G2201-i Picarro instead of a G2508. Since the former does not measure N_2O, this data is not available for this sampling day in April. We have added the following text to section 2.2: "April measurements in Bodviken were conducted using a Picarro G2201-i instead of the G2508, which measured the concentrations of CO_2 and CH_4 but not N_2O."

Minor issues:

Abstract:

Line 7: “seasonal variation concentrations” – Wording

We have corrected the wording to “seasonal variations **of** concentrations”

Line 10: It is unclear why the finding of a shift in N2O-CH4 relation slope indicates a shift in biogeochemical processes; see also comments further above

We agree that this point required clarification. We have added a more in-depth discussion of different factors that could potentially impact the CH_4-N_2O relationship in chapter 3.1.4 (see earlier comment). In order to keep the abstract concise, we refrained from discussing the biogeochemical processes in detail and instead rephrased the text as follows: “CH_4 concentrations below 250 nmol L^{-1} negatively correlated with N_2O, while higher CH_4 levels showed a positive correlation, suggesting differences in the dominant sedimentary microbial pathways**.**”

Line 11: what is meant by “anthropogenically degraded”?

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 rephrased the text as follows: “One bay that is subject to substantial human impacts (e.g. dredging, high nutrient loading, reduced vegetation cover) […]”.

Line 12:”methane emissions that surpasses CO2 update”: first of all, most regions had no CO2 update; second: this is not clear without introducing the fact that the authors calculated CO2 equivalent fluxes – WORDING

We added the following text (in bold) to be more specific: “**CO_2-equivalent** CH_4 emissions that surpassed CO_2 uptake **in this particular bay**”

Intro:

Line 33: strictly speaking, N2O is a by-product of nitrification, but an intermediate of denitrification

We now specify this is in the text: “N_2O is generated as a by-product of nitrification or as an intermediate of denitrification”

Line 37-38: statement on aerobic oxidation needs a reference

We have 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

Line 39-44: the manuscript completely ignores anaerobic methane oxidation, both here, and also in discussing the effect of T on methanogenesis; in fact, both methanogenesis and methanotrophy are T-dependent.

Here, we added: "dissolved CH_4 may be aerobically oxidized by methanotrophic bacteria (Hanson, 1996) **or consumed by anaerobic methanotrophic archaea (Knittel & Boetius, 2009).**

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

Line 55-64: see comment above; the authors did not really fully exploit the possibility to scaling based on driver analysis, nor did they really make use of the high temporal resolution in this study.

In the revised version we have added a Spearman correlation analysis. However, contrary to our expectations we could not show distinct correlations between GHG concentrations and seawater/sediment properties indicative of an eutrophication gradient. This is likely due to the limited number of data points - seawater/sediment properties were only measured in one central location of each bay. With respect to temporal resolution, we have now addressed the limitation of our study that our measurements were always conducted during daytime and thus cannot resolve a diurnal cycle, leading to a systematic bias when upscaling our measurements to fluxes. We have addressed this issue in more detail in the corresponding comment above.

Methods:

Chapters 2.1 and 2.2.: as stated above, give more details on method, and introduce the outer Bay sites; also please explain which data were used for the inner and outer Bay mean concentration value (just on red triangle spots, or all data in the inner Bay; outer Bay I cannot tell, it is so far not indicated in the text at all. Also, be more specific about the timing and duration of the data acquisition (midday vs. several hours of measurements).

We added the following text to section 2.2.1: "Sampling durations lasted between 60 and 90 minutes (typically ~75 minutes). Measurements were conducted both inside and outside bay areas. To distinguish between "inner bay" and "outer bay" sampling points, we delineated the bay boundary at the narrowest part of the inlet connecting each bay to the open Baltic Sea. This location represents the transition in water exchange, residence time, and mixing characteristics."

For continuous GHG measurements, all data points with in inner and outer bay areas were used. Seawater and sediment properties were only sampled in one location in the center of each bay (as indicated by the red triangle and grey dot in Figures A1-A6). To conduct the Spearman correlation analysis, GHG measurements were averaged per bay and correlated with the seawater/sediment properties.

Line 95: Partial pressures do NEVER have the unit ppm

We adapted the text as follows: "Mole fractions (in ppm)"

Line 98: " atm=106 ppm" ; same, please be scientifically correct here

We adapted the text as follows: "1 ppmv corresponds to 1 µatm at an ambient pressure of 1 atm"

Section 2.2.3. See remarks above on wind speed data availability, discussion of use of ASE exchange parameterization and its implication for comparison of fluxes with other studies

As addressed above, we now use wind data from the ICON-EU model for the respective sampling locations and days. In the response to the earlier comment on the k-parameterization, we also added a discussion why we chose this particular parameterization. Furthermore, we now address the use of different parameterizations in comparison to other studies (table 5).

Section 2.3.4: Line 158: “..most relevant for …” - not fully true for methane, where deeper sediment information might me needed to understand flux or ebullition behavior

We agree that for methane, processes occurring deeper in the sediment column, such as methanogenesis and ebullition, can play a critical role in determining fluxes to the water column and atmosphere. We have therefore revised the wording. In the revised manuscript, we clarify that the uppermost sediment layer (0–1 cm) is most relevant for characterizing sediment–water exchange processes and redox-sensitive transformations directly affecting dissolved GHG concentrations in surface waters, while acknowledging that deeper sediment layers may be important for understanding total CH_4 production and ebullition dynamics, which were not explicitly resolved in this study.

Results and Discussion

3.1 Outside Bay areas not mentioned or described before or anywhere after in the text (se general comment further up)

Thank you for pointing out the need for clarification. We have now expanded the description of how inner versus outer bay areas were defined by adding the following sentences to section 2.2.1: “To distinguish between “inner bay” and “outer bay” sampling points, we delineated the bay boundary at the narrowest part of the inlet connecting each bay to the open Baltic Sea. This location represents the transition in water exchange, residence time, and mixing characteristics.”

Lines 171-173: The lack of consistent pattern between bay openness and CO2 concentrations (see more general comment above on quantitative correlation analysis) indicates that there is no major control of this parameter on CO2 concentration, but not that there are high spatial and temporal variability in CO2dynamics”. This is no substantial statement.

We now removed the second part of the sentence “~~indicating high spatial and temporal variability in CO_2 dynamics~~”

Lines 184 and 185 „... share ...GHG emissions“ ; Bold and unsupported statement, based just on the similarity of a mean concentration value (for CO2). So why is this used to speculate on GHGs in general. For CH4, for instance, it is well established that freshwater and brackish water are quite distinct due to the role of processes involving sulphate.

We thank the reviewer for their comment and agree that the respective sentence is an overstatement. We have now replaced it with the following sentence: “The sheltered nature of the bays may resemble lake-like conditions with respect to air–water CO_2 exchange, but not necessarily other gases”

Lines 188-190. This might be a good place to extend on related work who actually did that, also showing how important it is, like e.g. Honkanenen et al. 2021 or Pönisch et al. 2025. However, this is again a statement true for all three GHGs, so maybe the best place to discuss this, and with it the limits of the study here, would be after the reports on the individual GHGs.

We have added the following text to discuss diurnal variability and potential bias of sampling at the same time: “Recent studies by Honkanen et al. (2021) and Pönisch et al. (2025) reported diurnal variability in surface-water pCO_2 and CH_4 in the Baltic Sea that could be linked to biological and physical drivers such as solar radiation, temperature or biological activity. We acknowledge that our measurements, which were always conducted around noon, do not capture these diurnal fluctuations and thus likely introduce a small but systematic bias relative to true daily mean conditions. While such measurements remain valuable, more extensive, long-term monitoring is required to identify the environmental parameters that drive these systems to function as CO_2 sources or sinks across different temporal scales."

Line 204-206: „while ...aerobic methanotrophs: Missing reference. Also, there is also a wealth of literature on rooted vegetation actively transporting methane, which would escape the flux measurements. This should be briefly mentioned as well.

This sentence was removed in the revised version as it no longer aligns with the results of the Spearman correlation analysis of the pooled data.

Lines 216-217: while it is true that the system would measure dissolved methane from the seafloor and dissolved by bubble dissolution, it would be then important to state that bubble-mediated transport itself escapes the device, and in fact that trapped bubbles could be an issue for the measurements. Maybe the authors did some work to protect the inlet from ascending bubbles? If so, it would be good to mention this in the method section.

In this study we did not take measures to protect the inlet from ascending bubbles. We have added the following sentence to the text "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 CH4 from both benthic diffusion and bubble dissolution. Consequently, the observed CH4 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 CH4 and the environmental parameters described above."

Line 210: enhanced temperatures also enhance aerobic oxidation rates, though apparently the effect on methanogenesis "wins" here.

We added "Elevated temperatures accelerate […] methanogenic rates [...] **as well as aerobic oxidation rates**."

Line 211-212: Stratification would enhance concentrations below the stratification gradient, but lead to lower concentrations in the top layer. Please be clear in your argument here.

The reviewer is right to point this out. We have now rephrased the text as follows: "Warmer seawater temperatures also decrease CH_4 solubility and enhance stratification, resulting in elevated CH_4 concentrations below the thermocline and lower concentrations above it."

Lines 216-217: while it is true that the system would measure dissolved methane from the seafloor and dissolved by bubble dissolution, it would be then important to state that bubble-mediated transport itself escapes the device, and in fact that trapped bubbles could be an issue for the measurements. Maybe the authors did some work to protect the inlet from ascending bubbles? If so, it would be good to mention this in the method section.

See answer above.

Chapter 3.1.3 as mentioned in general comments; a quantitative discussion of N2O concentrations in relation to temperature is needed, which is clearly the dominant driver. Or the discussion could be based on the calculated N2O saturations which eliminate this effect. Here, it would help to distinguish whether a part of the difference is temperature rather than production related.

We have added the following discussion of the temperature effect on N_2O to the manuscript (in context of the added Spearman correlation analysis: “The negative relationship of N_2O and temperature is likely driven by two key factors: (1) increased N_2O solubility at lower temperatures, and (2) the temperature sensitivity of denitrification enzymes. 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).”

Tables 2-4: again consider the general comment on integrated research on the drivers and the concentrations.

Following the reviewer’s general comment on integrating drivers with concentrations, we have now included a Spearman correlation analysis to contextualize the values presented as environmental drivers of GHG concentrations across the bays.

Chapter 3.1.4. while some aspects of the discussion are quite interesting, the analysis suffers from the investigation of T as mean driver before; here again having a look on whether deviations could be found on the saturation level could overcome this. Most of the N2O data will be on a straight line then, and it would be interesting to see how the trend at CH4 > 250 nmol displays on the saturation level.

Changes in temperature could be a reason for the difference in N_2O concentrations in April versus September/October. However, the temperature range encountered in Högklykeviken in September was narrow (14–15.5 °C), and therefore solubility effects were limited. Presenting the relationships in saturations instead of concentrations exhibits the same patterns (see figure below). As such we have decided to keep presenting the data in concentrations.

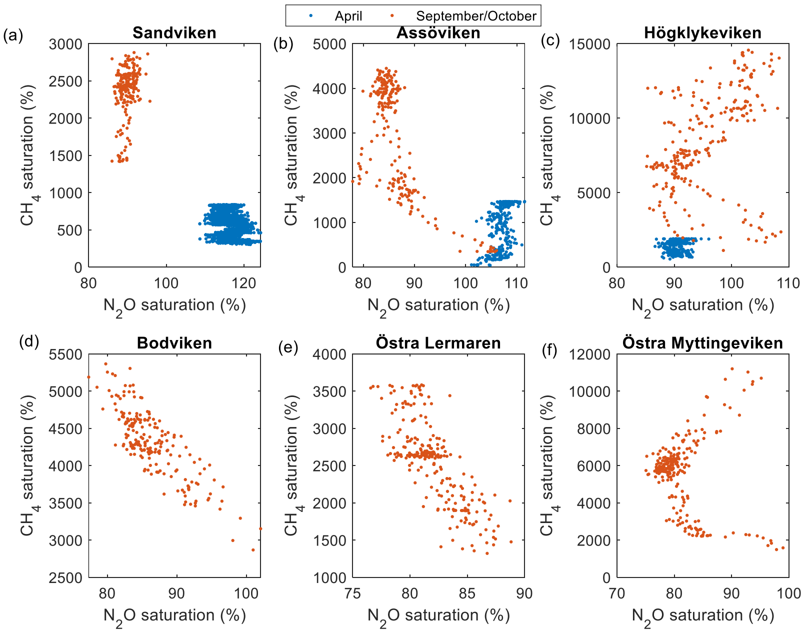


Figure: Correlations between N2O and CH4 saturations across different bays and seasons

Still, see my general comment on semi-quantitative statistical evaluation.

We understand this comment as referring to the limited quantitative attribution of drivers underlying the observed CH_4–CO_2 relationships. In the revised manuscript, we have addressed this by adding a correlation analysis between GHG concentrations and key environmental parameters. We have also revised the discussion to avoid causal interpretations and instead discuss the observed relationships in the context of established biogeochemical processes reported in the literature.

Line 268-272: "Since …. . … anomalous patterns". This section is purely speculative.

We thank the reviewer for pointing this out. We agree that the original text implied mechanistic explanations that cannot be supported by the available data. Oxygen was measured only at the central bay stations, and therefore local conditions at the CH_4 and N_2O peaks are unknown. To avoid speculation, we have revised the paragraph to remove unsupported mechanisms and now emphasize the limits of inference and the need for additional measurements to resolve these small-scale anomalies: "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. "

Figure 3: Plot a should be removed, as not part of the presented study (and likely wrong seasonal attribution)

We now removed subplot a) based on the reviewer's suggestion.

3.2.

Line 283-284 „... similar to the complex ..." This similarity is an artifact of using basically a mostly constant transfer coefficient (i.e. fixing the wind speed); see general comment.

In the revised version, we use wind speeds from the ICON-EU model at the respective sampling locations and days and have now removed the sentence highlighted by the reviewer.

295-296: Again: if flux discussion remains, nearby wind speed should be addressed, as well as the model of use and its impact for the flux comparison of table 5.

We added a discussion of the k-model used in this study in comparison to the parameterizations used in other 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.”

3.3.

Line 301 “ … with a median …” Here and elsewhere: please describe how data were averaged (just one No pe area, or “area weighted …”.

For each bay, we first calculated the mean using all measurements collected within that bay. To summarize conditions across bays, we then calculated the median of these six bay-level values, treating each bay as one independent unit. This approach is intentionally not area-weighted, as our aim was to compare bays as ecological entities. We now explicitly state this in the revised manuscript.

Line 309: To assess the regional significance, one would need to know typical flux estimates from similar areas on land or over open water.

We thank the reviewer for this important comment. However, a direct comparison of area-integrated CO_2-equivalent fluxes across ecosystems is challenging, as studies often report different subsets of greenhouse gases and cover vastly different spatial extents. In section 3.2 and table 5 we present areal flux intensities in comparison with other aquatic systems while the area-integrated fluxes presented in section 3.3. are intended only to provide a first-order estimate of potential regional relevance and are not directly comparable across studies or ecosystems due to differences in spatial extent and the inclusion of different greenhouse gases. We have added the following sentence to the discussion of the area-integrated fluxes: “Scaling these fluxes to the estimated total area of shallow enclosed bays provides a first-order indication of their potential regional relevance, but should not be interpreted as a closed regional budget due to spatial heterogeneity and limited spatial coverage.”

Figure 4: I found it really interesting that Högklyeviken is characterized by highest CH4 fluxes, but also as the only region with a net carbon dioxide uptake in both seasons; do the authors see any reason for that (I cannot see a link to the fact that the phosphate-binding experiment took place there …). This is just reviewer’s curiosity, so ignore if not worthwhile pursuing …

We thank the reviewer for their comment and agree that the high CH_4 fluxes combined with net CO_2 uptake in Högklykeviken warrant explanation. The CO_2 uptake likely reflects short-term dominance of pelagic photosynthesis during sampling, while the elevated CH_4 emissions point to active anaerobic processes in the sediments—processes that can decouple in time and space. Högklykeviken was selected for the phosphate-binding experiment because it was expected to be the most disturbed bay, but our environmental measurements show no significant differences to the other bays in terms of nutrient status or other indicators of anthropogenic degradation.

Conclusion

Line315: „.. shallow Baltic Sea“ – I think the authors should make clear that the data are from a very small part of the Baltic Sea and surely not fully representative for the (entire) Baltic Sea.

We have now replaced “Baltic Sea” with “wider Stockholm archipelago”

Line320 : …dominates CO2-equivalent fluxes.

We added “CO2-equivalent”

Line 324-329 surely need revision after addressing the review’s comments.

In response to the reviewer’s comments, we have revised this paragraph to better align our conclusions with the semi-quantitative nature of the analysis, to moderate claims of novelty, and to more clearly distinguish observational patterns from mechanistic attribution: “In the two bays with the highest concentrations of CH_4, 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.

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.”

Line 332: “However …. budgets” - shallow bays are not really overlooked, and so far, the data presented were quite in line with earlier findings; so I do not understand the statement

We removed “overlooked”

Line 347: Data Availability:It is of utmost importance that all data , also the auxiliary data, are publicly available upon publication of the paper, once accepted.

We will submit our data to the SOCAT and MEMENTO database once the manuscript revision has been finalized.

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 $L^{-1} \approx 91\%$) and Östra Myttingeviken ($O_{2,dissolved} = 5.6$ mg $L^{-1} \approx 59\%$), 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.

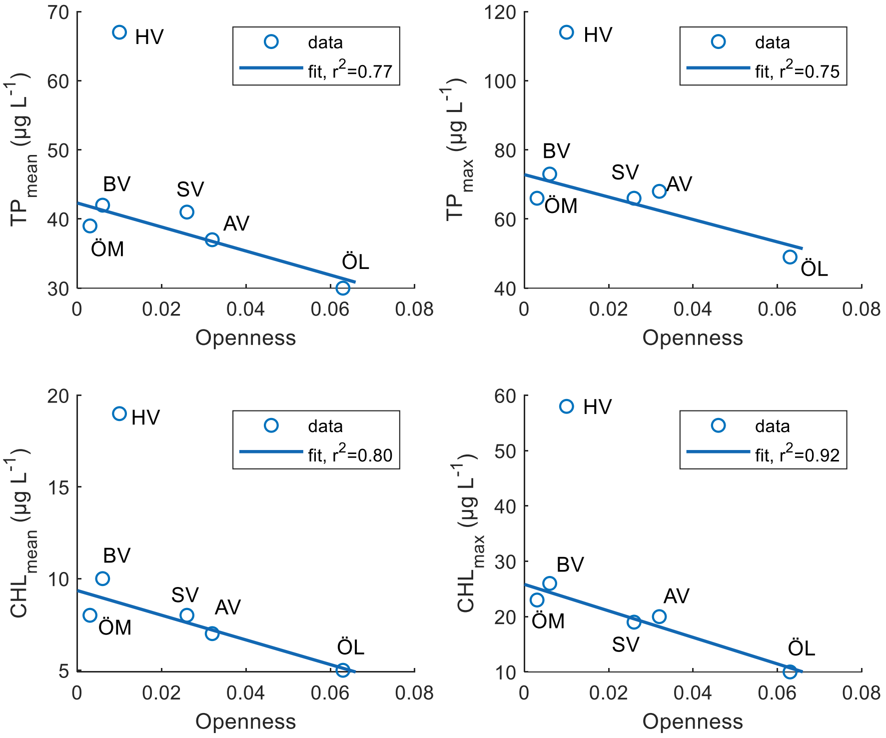
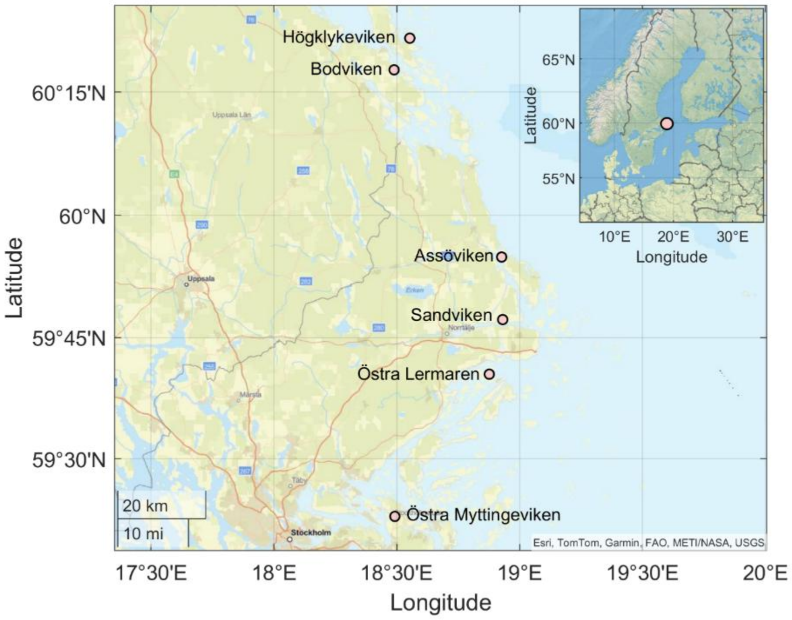


Figure: Correlations between openness and total phosphorus (TP) and chlorophyll a (CHL).

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 $Sc_{\text{this study}}$, "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 CO2 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 ($O2,dissolved = 8.3mg\ L^{-1} \approx 91\%$ saturation) and Östra Myttingeviken ($O2,dissolved = 5.6mg\ L^{-1} \approx 59\%$ 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"