

We sincerely thank the reviewers for their thorough evaluations, constructive comments, and positive assessment of our manuscript. We greatly appreciate the recognition of the value of our dataset, field observations, and the importance of providing CH₄ flux measurements from a tropical seagrass ecosystem, where such observations remain relatively scarce. The insightful comments have helped us substantially improve the clarity, rigor, and overall quality of the manuscript.

In response to the reviewers' suggestions, we have carefully revised the manuscript throughout. Specifically, we have clarified the distinction between CH₄ concentrations and fluxes, moderated interpretations and conclusions where appropriate, and ensured that all discussions are fully supported by the data. We have also expanded the discussion of variability, uncertainties, and methodological limitations, including the roles of CH₄ oxidation, lateral transport, and the potential contribution of ebullition, which was not directly quantified in this study. In addition, we have acknowledged the uncertainties associated with the fate of laterally transported CH₄ and avoided interpretations beyond the scope of the available observations.

To improve the presentation of the study, we have reorganized the manuscript by separating the Results and Discussion sections, allowing a clearer distinction between observations and interpretations. We have also revised the study area figures to better illustrate the sampling locations and spatial relationships among the seagrass meadow, sandy sediments, and adjacent coastal waters, including the addition of a conceptual diagram. Furthermore, we have removed the annual CH₄ budget and related extrapolations because the dataset does not include winter observations and therefore does not represent a complete annual cycle. The manuscript now focuses on the observed seasonal patterns while explicitly acknowledging the temporal limitations of the study. Finally, all figures have been updated with higher-resolution versions to improve their clarity.

We believe these revisions have significantly strengthened the manuscript and have addressed the reviewers' concerns. We are grateful for the reviewers' valuable comments, which have greatly improved the clarity, balance, and scientific rigor of our work.

Hsiao-Chun Tseng on behalf of all the co-authors

Please find below our point-by-point responses to the comments raised by the three reviewers.

RC1: ['Comment on egusphere-2026-1488'](#), Anonymous Referee #1, 13 May 2026

General comments:

The manuscript presents methane fluxes at the sediment-water and water-air interfaces from field campaigns conducted in Taiwan's seagrass ecosystem. The study shows that methane production is primarily associated with benthic processes at the meadow and provides observations from a tropical seagrass environment for which relatively limited data are available.

The manuscript is generally well structured, and the comparison with previous studies is particularly valuable. However, several parts of the discussion and conclusions currently overinterpret the results or use wording that is stronger than supported by the data. In particular, the manuscript would benefit from a clearer distinction between methane concentrations and methane fluxes, more cautious interpretation of the seasonal dataset, and a more explicit discussion of the large variability associated with some benthic flux measurements.

The manuscript would also benefit from clearer information regarding the exact locations and distances between the sites. Without this information, it is difficult to fully evaluate the independence of the "seagrass" and "sand" stations, particularly given that methane can be transported laterally in shallow coastal waters and could therefore influence the measurements there.

Overall, the manuscript contains an interesting dataset and has the potential to make a valuable contribution, but needs substantial clarification and rephrasing before publication. The conclusion should also be revised accordingly.

Reply: We thank the reviewer for the thorough evaluation of our manuscript and for the constructive comments. We appreciate the positive assessment of the dataset and the recognition of the value of providing methane flux observations from a tropical seagrass ecosystem, a region for which data remain relatively limited.

We agree that several aspects of the manuscript require clarification and more cautious interpretation. In the revised manuscript, we carefully distinguish between methane concentrations and methane fluxes throughout the text, moderate statements that may overinterpret the available evidence, and revise the discussion and conclusions to ensure that all interpretations are fully supported by the data. We have also expanded the discussion of variability in the benthic flux measurements and explicitly acknowledged the associated uncertainties and limitations.

In addition, we agree that clearer information regarding site locations and spatial relationships is needed. To improve clarity, we have revised the study area figure (Figure 1) to clearly identify the locations of the in situ incubation experiments at the seagrass and sand stations and added a conceptual diagram (Figure 3) illustrating the geographic distribution and spatial arrangement of the sampling sites.

Furthermore, following the reviewer's suggestion, we have reorganized the manuscript by separating the Results and Discussion sections to improve the clarity and readability of the presentation. We believe this revised structure facilitates a clearer distinction between observations and interpretations.

We appreciate these valuable suggestions and believe they have significantly improved the clarity, rigor, and balance of the revised manuscript.

Specific comments:

Abstract:

Due to the lack of winter measurements, the term “annual” should be removed. In addition, almost one full year separates autumn 2022 from summer 2023, making the extrapolation to an annual cycle uncertain. All statements here and in the text need to be rephrased.

Reply: We completely agree with the reviewer’s assessment. Because winter measurements were not captured, extrapolating the data to an annual scale introduces undue uncertainty. To address this, we have systematically revised the abstract and the main text to focus strictly on temporal and seasonal dynamics during the specific sampling periods, removing all “annual” scaling and budgets.

Introduction:

Line 44: The authors should not present methane as having a single GWP value of “27–80 times greater” than CO₂. Methane has a GWP of ~27 over a 100-year timescale and ~80 over a 20-year timescale. In addition, these values refer to GWP rather than SGWP (sustained-flux GWP).

Reply: We appreciate the reviewer’s insight. We agree with the reviewer that coastal ecosystems emit methane continuously rather than as a single pulse, using Sustained-Flux Global Warming Potential (SGWP) is scientifically more accurate. We have revised the manuscript, and the text now reads:

Line 46-48: ‘Despite these benefits, seagrass meadows are also recognized as sources of methane (CH₄), a potent GHG with a sustained-flux global warming potential 96 times greater than carbon dioxide (CO₂) over a 20-year time horizon on a mass basis (Neubauer and Magonigal, 2015).’

Line 47: The statement: “More broadly, the coastal ocean is a global hotspot for CH₄ emissions, accounting for 75% of oceanic CH₄ emissions” does not appear to be stated explicitly in Weber et al 2019 and should be rephrased more carefully. In addition, the estimate “with continental shelves alone contributing approximately 13 Tg CH₄ yr⁻¹ from Bange et al. 1994 is outdated. The manuscript should instead use the more recent estimates shown in Weber et al. (“Extrapolation of rate measurements from active seafloor seeps across areas of likely seepage suggests that global CH₄

ebullition from continental shelf sediments (0–200 m) likely falls between 18 and 48 Tg yr⁻¹, with a most likely rate of ~35 Tg yr⁻¹).

Reply: We thank the reviewer providing the construct comments and suggestion. We have removed the unsupported percentage statement, updated the recent reference and have modified the manuscript. The text has been rephrased and updated as follows:

Line 50-51: “More broadly, the coastal ocean represents a major hotspot of marine CH₄ emissions, with continental shelf sediments estimated to release 18–48 Tg CH₄ yr⁻¹ through ebullition alone (Weber et al., 2019).”

Methods:

Line 89: For transparency, the four sites should be indicated in Figure 1c. Since the study compares “seagrass meadow” and “bare sand” sites, the reader needs to know how far they are located from each other and from the surrounding coastal features. Methane can be transported laterally, meaning that methane measured at the sand site could potentially be influenced by emissions originating from the seagrass meadow.

Reply: We thank the reviewer for this helpful suggestion. We have revised Figure 1 to clearly show the locations of all four sampling sites, including the seagrass meadow and bare sand stations used for the in situ incubation experiments. These revisions provide a clearer representation of the study design and spatial relationships among the sampling sites, allowing readers to better evaluate the potential connectivity between them.

We acknowledge that lateral methane transport cannot be completely excluded in this shallow coastal environment and may influence methane concentrations in the water column. However, we do not expect it to substantially affect the incubation results, as these experiments were specifically designed to quantify benthic methane fluxes originating directly from the underlying sediments. We have clarified this point in the revised manuscript.

Figure 1 is modified as below:

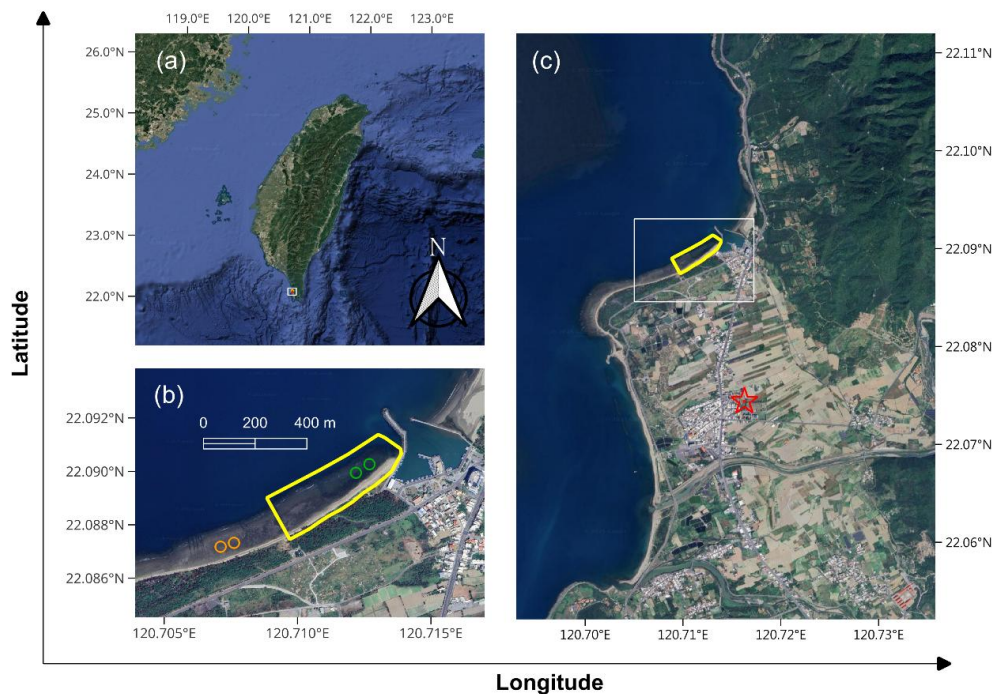


Figure 1. Study area. (a) Regional map showing the location of the study area. (b) Close-up view of the benthic chamber incubation sites. The yellow rectangle outlines the Haikou seagrass meadow. (c) Zoomed-in map of the study area. The red star indicates the location of the Checheng Weather Station.

Sections 2.2 and 2.3: The authors mention that the incubation experiments lasted between 2 and 5 hours “depending on sampling needs”, but these needs are not explained. Please clarify what controlled the duration of the incubations.

Reply: We thank the reviewer for highlighting this important oversight. The variation in incubation duration was not due to shifting experimental goals, but rather to unexpected logistical challenges in the field.

We have removed the phrase "depending on sampling needs" and revised the text to explicitly clarify it.

Now the text reads (Line 105-108): “The in situ benthic cultivation experiments were conducted during both day and night for approximately 2 to 5 hours, depending on wave and weather conditions as well as operational constraints. Specifically, although a standard 5-hour sampling interval was intended, poor weather conditions occasionally restricted diver deployment, resulting in shorter incubation periods.”

It is also unclear why natural water movement needed to be simulated above the chamber if the chamber was fully closed. Please clarify the role of the magnetic agitator and whether it simulated internal circulation within the chamber. Figure 2c is unclear. Zooming on one of the photos to describe the chamber in more detail could help.

Reply: The reviewer is entirely correct: the magnetic agitator was located inside the closed chamber, not outside or above it. Its purpose was to maintain internal water circulation to prevent the development of artificial stagnant boundary layers over the sediment surface during the incubation. The previous sentence didn't make it clear so we have modified this sentence to accurately reflect the setup and clarify the role of the agitator.

Revised Text (Line 108): “To ensure continuous water circulation, a magnetic agitator was positioned inside the chamber (Figure 4d).”

We also modified the Figure 4

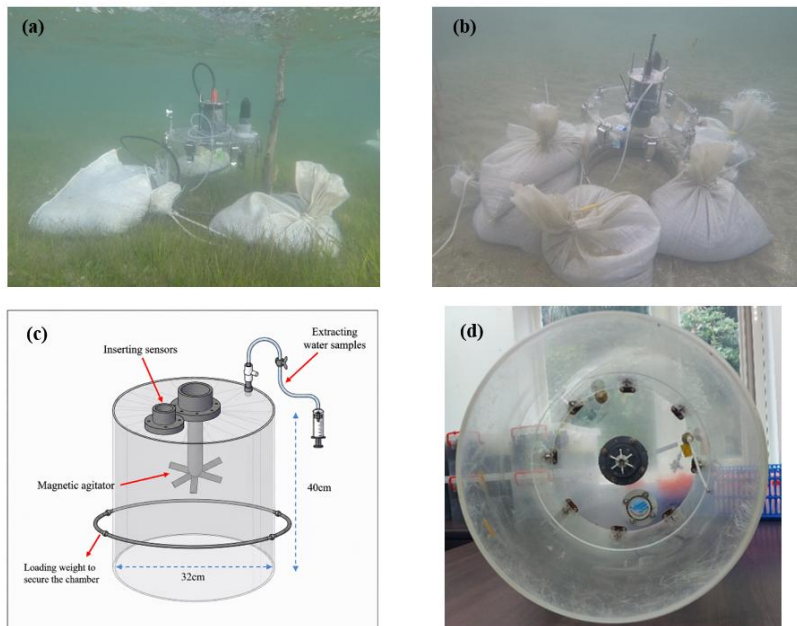


Figure 4. In situ benthic chamber incubation system deployed in (a) the seagrass meadow and (b) adjacent bare sediment, with (c) a schematic illustration of the chamber design and (d) an upward view from inside the chamber.

Sections 2.2 and 2.3 could likely be merged for clarity. After reading section 2.2, the reader is left wondering how the physical and chemical parameters were measured. In addition, the paragraph beginning with “water samples” belongs more naturally in section 2.3.

Reply: We thank the reviewer for this valuable suggestion. Following this excellent suggestion, we have completely merged Sections 2.2 and 2.3 into a single, cohesive section now titled "Section 2.2: In situ chamber incubation system, samples collection and chemical analysis"

It is also unclear how the porewater sampling was conducted relative to the chamber system. Finally, a pH sensor is mentioned but pH is not discussed later in the manuscript.

Reply: We appreciate the opportunity to clarify our sampling design and streamline our parameter reporting.

1. Porewater Sampling Clarification: The porewater sampling was conducted completely separate from the benthic chambers to ensure that the extraction process did not disturb sediment layering or affect the internal flux dynamics of the active chamber deployments. We have clarified this in the text and specified the sampling frequency.

Revised Text (Line 120-121): “Porewater samples were collected once daily from both the seagrass meadow and bare sand sediments at locations independent of the chamber incubation sites to avoid disturbing the ongoing benthic chamber incubations.”

2. pH Sensor Mention: While pH was initially measured as part of our baseline monitoring, preliminary analyses indicated that this parameter did not exert a significant statistical influence on methane dynamics in our system. To prevent reader confusion and keep the manuscript tightly focused on the primary drivers of CH₄ fluxes, we have removed the mention of the pH sensor from the Methods section.

Results and discussion:

Section 3.1

The section refers to “surface water conditions”, whereas the methods indicate that temperature, salinity, and dissolved oxygen were measured within the chamber. Please clarify whether the values presented correspond to chamber measurements or ambient surface waters.

Reply: We thank the reviewer for this constructive feedback. The values presented under “surface water conditions” correspond to the ambient surface waters surrounding the incubation setups, not the internal environment of the chambers. We first monitored surface water conditions and calculated sea-to-air methane fluxes, followed by analyses of sediment CH₄ production and benthic fluxes.

Since the values discussed in the text are not always directly consistent with Table 1 or figure 3, please clarify whether the reported values correspond to seasonal averages from all replicates. The source of the freshwater influence responsible for the lower salinity in the seagrass area should also be clarified. Again, the exact locations of the stations would help interpret these observations.

Reply: The reviewer is correct: unless otherwise noted, the values reported throughout the text represent seasonal averages aggregated across all replicates rather than individual raw data points or isolated ranges.

We have revised the phrasing in the text converting instances where concentration ranges were previously discussed into their corresponding average calculations—to ensure perfect alignment with Table 1 and Figure 5.

The updated text now reads as follows (Line 154-162):

" Seasonal mean temperatures ranged from 27.2 to 29.1 °C in the seagrass meadow and from 27.5 to 29.4 °C in the coastal water (Table 1). Daytime surface water temperatures were consistently higher than nighttime temperatures, with a more pronounced diurnal difference in the seagrass meadow than in the coastal water (Figure 5a). Salinity ranged from 30.21 to 33.73, with seasonal mean values slightly lower in the seagrass meadow (31.94–32.88) than in the coastal water (32.12–33.06; Figure 5b). DO saturation in the seagrass meadow exhibited pronounced diurnal variation (Figure 5c), increasing from nighttime averages of 87.0–94.5% to daytime averages of 109.3–124.4% (Table 1), likely driven by seagrass photosynthesis. In contrast, DO saturation in the coastal water was relatively stable, ranging from nighttime averages of 97.0–99.3% to daytime averages of 100.5–107.5% (Table 1). Seasonal mean DO saturation ranged from 98.9 to 109.4% in the seagrass meadow, slightly higher than that in the coastal water (99.9–102.8%; Table 1)."

Please discuss in more detail why autumn 2022 showed lower daytime methane concentration compared to the other seasons. This pattern is likely related to storm-induced wind and enhanced mixing, as suggested by the elevated wind speeds during this period.

Reply: We completely agree with the reviewer's assessment. The lower daytime CH₄ concentrations observed during autumn 2022 were indeed heavily influenced by meteorological forcing. We have expanded the discussion to explicitly incorporate this mechanism, noting the high wind speeds that drove physical degassing.

Revised Text now reads (Line 209-214): "The only exception occurred during autumn 2022, when the typical diurnal pattern was disrupted by a nighttime storm event characterized by heavy rainfall and strong winds. Elevated wind speeds likely enhanced turbulence, vertical mixing, and air–water gas exchange, promoting rapid physical degassing of dissolved CH₄ and resulting in substantially lower nighttime concentrations than those observed during the other sampling periods. This finding highlights the importance of episodic weather events in regulating short-term CH₄ dynamics in shallow coastal ecosystems."

In addition, with only three seasonal campaigns available, it seems too strong to state: "Dissolved CH₄ concentrations in seagrass waters exhibited clear seasonal and diurnal variability...". Please rephrase.

Reply: We agree with the reviewer's point that three campaigns are insufficient to claim a definitive seasonal cycle. We have rephrased this sentence to clarify that the variations occurred among the specific sampling campaigns/periods rather than asserting generalized seasonality.

Revised Text now reads (Line 205-210): "Dissolved CH₄ concentrations in the seagrass meadow exhibited pronounced diurnal variability, with surface waters generally peaking at night. This consistent pattern across most sampling periods likely reflects the influence of daytime photosynthesis, which increased DO saturation and may have suppressed CH₄ production or enhanced aerobic CH₄ oxidation, thereby limiting CH₄ accumulation in the water column. In contrast, lower nighttime DO saturation reduced oxidation potential and allowed sediment-derived CH₄ to accumulate in the shallow water column. The only exception occurred during autumn 2022, when the typical diurnal pattern was disrupted by a nighttime storm event characterized by heavy rainfall and strong winds."

I am also confused by the interpretation in Lines 153-156. If methane production is primarily associated with seagrass sediments, then the lower methane concentration in coastal water is not simply related to water depth and potential oxidation. It may instead reflect the absence of methane production in sandy sediment. Alternatively, methane observed in coastal waters could partly originate from lateral transport from the seagrass area. Again, this is difficult to evaluate without precise site locations.

Section 3.3

I am not convinced that the term "relatively stable" accurately describes methane production and fluxes in seagrass sediment, especially considering the 156% increase observed in autumn. Similarly, the description of "episodic peaks" in sandy sediment may be overstated given that only three seasons are available.

The authors state that “CH₄ production rates and fluxes were higher in seagrass sediments than in sandy sediment” but then note that “autumn 2022 presented an exception”. However, this exception is huge (sandy autumn flux = 388.9 ± 382.8 and seagrass autumn flux = 105.3 ± 284.2), and strongly influences the annual average, which weakens the broader claim that seagrass is the dominant hotspot.

Reply: We agree that describing the seagrass sediment fluxes as "relatively stable" overlooks the substantial 156% increase in autumn, and that "episodic" implies a frequency that a three-season dataset cannot fully substantiate. To address this, we have completely removed both terms from the manuscript and rephrased the text to more accurately describe the observed trends as a contrast between baseline conditions and discrete, storm-driven responses.

Revised Text now reads (Line 288-296): “Collectively, these biological and biogeochemical processes explain why seagrass sediments generally support higher CH₄ production and more sustained sediment-to-water fluxes than adjacent sandy sediments (George et al., 2020; Schorn et al., 2022).

An exception to this general pattern occurred during the autumn 2022 sampling, when daytime CH₄ production rates and benthic fluxes in sandy sediments exceeded those in the seagrass meadow, while nighttime incubations could not be conducted because of the storm event (Figure 6b). This anomaly was likely caused by heavy rainfall and strong winds that disturbed the unvegetated sandy sediments, promoting sediment resuspension and the rapid release of porewater CH₄. In contrast, the dense root and rhizome network of seagrasses stabilizes sediments and buffers against physical disturbance, making vegetated sediments less susceptible to storm-induced CH₄ pulses despite their generally higher background CH₄ production.”

In addition, the extremely large standard deviations reported in autumn 2022 should be discussed explicitly, as they indicate substantial temporal and spatial variability and uncertainty in the seasonal extrapolations.

Reply: We thank the reviewer for this insightful comment. We agree that the large standard deviations in autumn 2022 warrant explicit discussion regarding their underlying causes and their implications for seasonal extrapolations. To address this, we have expanded our discussion in the revised manuscript to explicitly acknowledge this uncertainty and to caution against over-extrapolating these storm-influenced autumn values across broader, storm-free periods.

The added text as follows (Line 297-304): " These contrasting responses were also reflected in the seasonal averages. Mean sediment-to-water CH₄ fluxes were $88.2 \pm 41.2 \mu\text{mol m}^{-2} \text{d}^{-1}$ in the seagrass meadow and $150.4 \pm 207.0 \mu\text{mol m}^{-2} \text{d}^{-1}$ in the adjacent sandy sediments (Table 2). However, the higher seasonal mean for sandy sediments was largely driven by the exceptionally high emissions recorded during the storm-affected autumn sampling. Excluding this episodic event, seagrass sediments consistently exhibited higher baseline CH₄ production and benthic fluxes, whereas sandy sediments generally maintained much lower emissions. These findings indicate that seagrass sediments function as persistent CH₄ sources sustained by continuous organic matter decomposition and microbial methanogenesis, whereas unvegetated sandy sediments are characterized by relatively low background emissions punctuated by irregular, disturbance-driven CH₄ pulses.”

Section 3.4

The two statements in the first sentence are not fully supported by figure 6. Spring 2022 shows a distinct pattern, with stronger methane concentration in the sand. This makes the following interpretation too strong. Similarly, the description of a “steady decline” below 12 cm is overstated, as some profiles clearly show increases at depth.

Reply: We completely agree with the reviewer's observation. Our original text overgeneralized the trends and overlooked the specific variations present in the depth profiles, particularly during Spring 2022. To address this, we have removed the words "consistently" and "steady decline" from the text. We have rephrased it to accurately present the depth-dependent variations and explicitly highlight the distinct trend observed in the sandy sediments during Spring 2022.

Revised Text now reads (Line 193-202): “In seagrass sediments, porewater CH₄ concentrations varied with depth, with mean concentrations generally highest within the upper 0–12 cm across all sampling campaigns, before declining below 12 cm depth. Seasonal differences were also evident, with the highest porewater CH₄ concentration recorded during autumn 2022 (423.2 nM), followed by summer 2023 (364.8 nM), whereas spring 2022 exhibited comparatively lower concentrations throughout most of the sediment profile (Figure 8). Seagrass sediments exhibited higher porewater CH₄ concentrations than sandy sediments in autumn 2022 and summer 2023, particularly within the upper 12 cm of the sediment column (Figure 8b and 8c). However, this pattern was reversed in spring 2022, when sandy sediments exhibited higher porewater CH₄ concentrations than seagrass sediments below 10 cm depth (Figure 8a). In sandy sediments, porewater CH₄ concentrations also varied with depth, although vertical gradients were less consistent among seasons than those observed in seagrass sediments. Concentrations generally remained within a narrower range across the sediment profile (Figure 8).”

The reference to Tang et al. 2025 should be explained in more detail. This study is highly relevant to the present work but is only mentioned briefly here. It should be placed into clearer context, particularly regarding differences in sampling periods. Tang et al. sampled in March-May 2023, whereas the present study was conducted in June 2023.

Reply: We completely agree with the reviewer that Tang et al. (2025) provides essential background context for our data and that the temporal relationship between the two sampling campaigns deserves explicit discussion.

We have revised the text to place Tang et al. (2025) into clearer ecological and chronological context. Specifically, we have clarified that their spring campaigns (March–May 2023) characterized the baseline sedimentary environment (showing an average TOC of 0.40% driven by aquatic plants and microbial processing) directly preceding our summer campaign (June 2023). This temporal progression indicates that the organic matter accumulation and priming characterized by Tang et al. in the spring likely served as the primary substrate fuel driving the enhanced methanogenic activity we observed in early summer.

The text now reads (Line 244-250): “According to Tang et al. (2025), sediments in the Haikou seagrass meadow contain an average total organic carbon content of approximately 0.40%, with organic matter derived predominantly from marine sources, including aquatic plant material and microbial processing. The accumulation of this organic substrate provides the fuel required for microbial methanogenesis and likely explains the elevated porewater CH₄ concentrations observed during the warmer seasons. Similar relationships between sediment organic carbon and CH₄ production have been reported in other seagrass ecosystems, where the decomposition of seagrass detritus enhances microbial activity and promotes methanogenesis (Schorn et al., 2022; Dai et al., 2026; Dai et al., 2025).”

The rest of the paragraph is a bit hard to follow because it mixes processes derived from literature and analogies with the present study. Some references are presented in a way that suggests direct observations in the present study. For example, lines 235-236 mention bubbles observed by Sun et al 2022, although these observations were made in a different environment.

Reply: We thank the reviewer for this helpful comment. We agree that the original text did not clearly distinguish between processes inferred from previous studies and observations from the present study, which may have caused confusion. In particular, the description of bubble observations cited from Sun et al. (2022) could be misinterpreted as direct observations from our study. To address this issue, we will revise the manuscript to clearly separate our results from literature-based interpretations. In addition, following the reviewer's suggestion, we will restructure the manuscript by separating the Results and Discussion sections, which will improve the clarity of the presentation and ensure that references to previous studies are clearly identified as supporting evidence rather than observations from the present study.

Section 3.5

The calculation of the annual methane emission of 78.3 moles is based only on three seasons which do not form a continuous annual cycle, excludes winter conditions and includes a storm in autumn. This limitation should be acknowledged more clearly and the wording should be moderated.

Reply: We thank the reviewer for this important comment and fully agree with the concern. The original annual methane emission estimate of 78.3 moles was derived from 3 seasonal measurements that do not represent a complete annual cycle, as winter conditions were not sampled and the autumn dataset included a storm event that could disproportionately influence the extrapolation. We agree that extending these observations to an annual budget introduces substantial uncertainty and may overstate the robustness of the estimate.

To address this issue, we have removed the 78.3-mole annual methane emission calculation from the manuscript, together with all references to annualized projections and annual budgets. Rather than attempting to qualify the extrapolation, we believe it is more appropriate to focus on the observed seasonal variability captured during our sampling campaigns. The revised manuscript therefore presents the methane fluxes within the context of the specific measurement periods and explicitly acknowledges the temporal limitations of the dataset, including the absence of winter observations and the influence of episodic storm conditions.

Line 251: It is the first time that missing winter data is acknowledged. This should be mentioned earlier.

Line 253: This sentence puts too much emphasis on oxidation. Lateral transport may imply that methane is eventually emitted to the atmosphere outside the meadow rather than oxidized locally. I suggest rephrasing to "This imbalance between sediment production and atmospheric emission suggests that a substantial fraction (here ~94%) of sediment-derived CH₄ was either oxidized within the water column or transported laterally before reaching the atmosphere."

Reply: We completely agree with the reviewer's distinction between local water column oxidation and physical lateral transport. Attributing the entire missing fraction solely to oxidation overlooks the significant role of hydrodynamic exporting pathways in coastal environments.

Following your excellent suggestion, we have rephrased the sentence to properly balance both potential fates of the un-emitted methane.

The comparison with other studies in Table 3 is very useful and valuable. However, including water depth for each study would improve the comparison.

Reply: We thank the reviewer for this excellent suggestion. We agree that including water depth provides critical ecological context for comparing methane and greenhouse gas dynamics across different seagrass systems.

As suggested, we have thoroughly re-examined the cited literature and added a new column for "Water Depth (m)" to Table 3. For several studies, specific water depth values were not explicitly reported by the original authors; in these cases, we have denoted the missing values as "Not Available" (NA) or provided general environmental descriptions (e.g., "Intertidal") where available.

Conclusions:

The conclusion should reflect the corrections previously suggested. In addition, it would benefit from a clearer distinction between methane concentrations and methane fluxes, as these concepts are occasionally mixed throughout the discussion.

Reply: We have thoroughly revised the Conclusion section to reflect all previous corrections (such as removing annualized scaling calculations and shifting to daily areal rates). Furthermore, we have performed a comprehensive text sweep to ensure that "concentrations" and "fluxes" are decoupled and applied correctly.

Technical corrections:

Line 54: Remove "the" before "CH₄ generated in sediments"

Reply: We thank for the reviewer's suggestion. We have modified it.

Figure 1: The squares representing the zoomed areas are difficult to see. I suggest making them thicker and red, and adding labels a, b and c to each panel. This figure would also benefit from locations, following Tang et al. 2025

Reply: We thank for the reviewer's suggestion. We have modified the Figure 1 and add a, b and c.

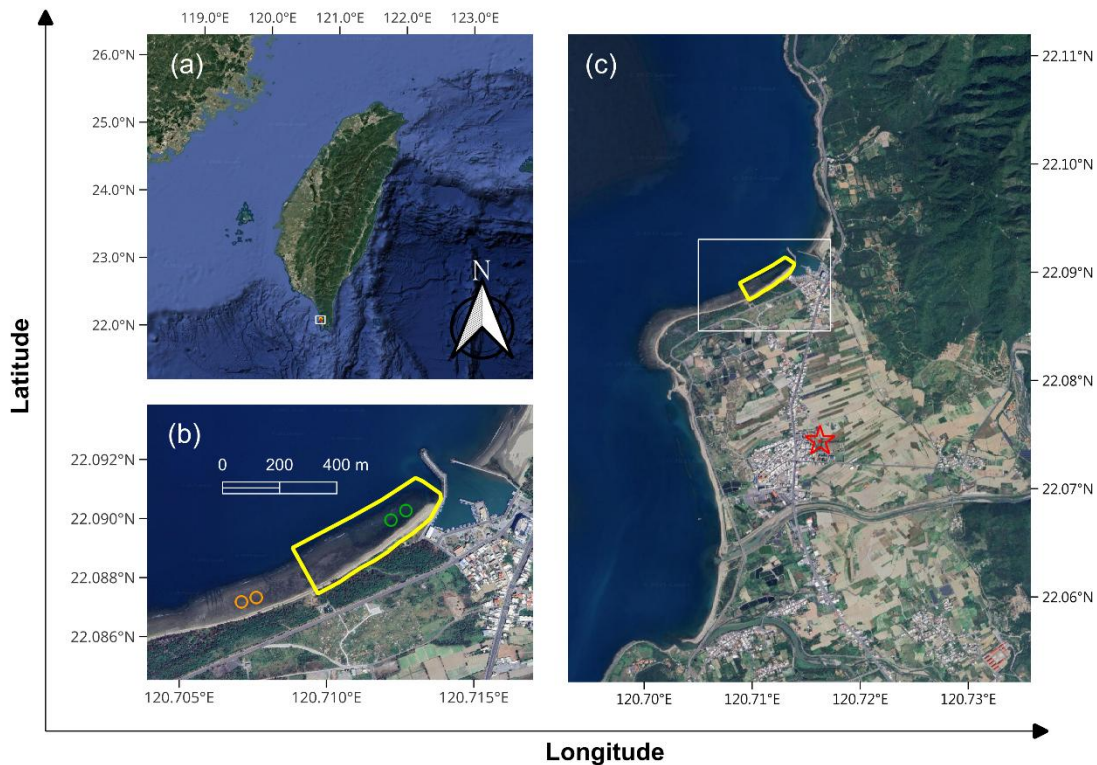


Figure 1. Study area. (a) Regional map showing the location of the study area. (b) Close-up view of the benthic chamber incubation sites. The yellow rectangle outlines the Haikou seagrass meadow. (c) Zoomed-in map of the study area. The red star indicates the location of the Checheng Weather Station.

Line 83: Sediment samples? Water samples? Please clarify

Reply: We thank the reviewer for pointing this out. We have updated the text to explicitly clarify that both water and porewater samples were collected during these periods.

We added new sentence read: “Over these periods, both water and porewater samples were collected.”

Lines 83-87: The seasons are introduced at the beginning of the section, followed later by “different seasons”. I suggest switching both sentences for clarity.

Reply: We agree with the reviewer that restructuring the sentence order creates a more logical flow from the general sampling approach to the specific seasons monitored. Following your suggestion, we have rearranged these sentences to establish the seasonal context more clearly at the beginning of the section.

Now the new sentences read (Line 87-95):

“Field investigations were conducted from May 19 to 23 (spring) and September 26 to October 4 (autumn) in 2022, and from June 13 to 17 (summer) in 2023. Winter fieldwork could not be conducted because of severe weather conditions. Surface water samples were collected at both the coastal water site and within the Haikou seagrass meadow during both daytime and nighttime across different seasons to examine the diurnal and seasonal variability of water-to-air CH_4 fluxes. In situ benthic chamber incubations were conducted simultaneously in the Haikou seagrass meadow and adjacent unvegetated bare sand areas during both daytime and nighttime to investigate the diurnal and seasonal variability of sediment-to-water CH_4 fluxes under changing environmental conditions. Both incubation sites were located in the subtidal zone and experienced

similar tidal depth variations. During each field campaign, porewater samples were also collected from the seagrass meadow and adjacent unvegetated bare sand sediments.”

Lines 144-145: Cite Figure 3b after the salinity range, and Table 1 after mentioning seasonal averages, since Figure 3 only shows night and day values.

Reply: We agree that Table 1 is the correct reference for the overall seasonal averages, as Figure 3 (now as Figure 5) specifically presents the diel (day and night) variations. Following your suggestion, we have adjusted the citation placements accordingly.

Lines 176-181: These lines simply repeat Table 2 and could be shortened.

Reply: We thank the reviewer for this suggestion. We have revised this section to remove the redundant data points and shorten the text, focusing only on the main trends and the peak values.

Line 224: Wind disturbance should also be mentioned as a contributing factor

Reply: We thank the reviewer for this valuable suggestion. Following your suggestion, we have explicitly incorporated wind disturbance alongside our existing discussion of environmental drivers.

Now the new sentence reads (Line234-244): “CH₄ production and benthic fluxes in the seagrass meadow exhibited clear seasonal variability, with the lowest rates occurring in spring and substantially higher rates during summer and autumn (Figure 7b). This seasonal pattern is likely driven by changes in hydrodynamic conditions that regulate organic matter accumulation within the meadow. At Haikou, strong winds and wave action prevail from October to April (Figure 2), limiting the deposition and retention of organic particles within the seagrass bed. Consequently, the availability of organic substrates for methanogenesis is reduced, resulting in relatively low CH₄ production and benthic fluxes during spring. In contrast, calmer conditions beginning in late spring and early summer favor the deposition and accumulation of organic matter, including seagrass detritus and epiphytic material, within the sediment. The enhanced availability of labile organic carbon likely stimulates microbial decomposition and methanogenesis, leading to the elevated CH₄ production rates and sediment-to-water fluxes observed during summer and autumn.

The seasonal increase in benthic CH₄ production is supported by the porewater CH₄ profiles, which showed markedly higher concentrations during summer and autumn (Figure 8).”

Figure 6: Please explain why error bars are absent from some porewater profiles

Reply: The error bars correspond to the range calculated from duplicate core samples. For certain specific depth intervals, error bars are absent because only a single sample was available for analysis, leaving no statistical range to plot.

To ensure this is clear to readers, we have added this explanation directly to the Figure 6 (now Figure 8) caption. Revised Figure 6 Caption:“...Error bars represent the range of duplicate samples; error bars are absent for depth intervals where only a single sample was available.”

Line 240: Refer to Table 1

Reply: We thank for the reviewer’s suggestion. We have modified it.

Line 243: Refer to Table 2

Reply: We thank for the reviewer’s suggestion. We have modified it.

Line 248: Consider adding “may also intensify”

Reply: We agree that this phrasing provides a more appropriately moderated and objective tone for our forward-looking climate projections. Following the reviewer’s excellent suggestion, we have updated this exact wording.

Now the text reads (Line 304-307): “As climate change is projected to increase the intensity and frequency of typhoons and other extreme weather events (IPCC, 2021), episodic CH₄ release from sandy coastal sediments may become an increasingly important component of coastal GHG budgets, even though seagrass meadows remain the dominant long-term hotspots for benthic CH₄ production.”

Line 304: Correct typo “approximarely”

Reply: We thank the reviewer for spotting this error. We have modified it.

RC2: '[Comment on egusphere-2026-1488](#)', Anonymous Referee #2, 22 May 2026

The paper presents a unique dataset and well-structured fieldwork analyses of methane fluxes across seasons and compares seagrass meadows with sandy environments in coastal Taiwan. This type of research is important for improving our understanding of the role of seagrass meadows in methane emissions and coastal methane cycling. The fieldwork and analytical approaches are generally well-designed however, the manuscript requires major revisions to adequately represent the study's scope and significance.

Reply: We sincerely thank the reviewer for the positive evaluation of our study and for recognizing the value of the dataset, fieldwork design, and analytical approach. We appreciate the reviewer's assessment that this work contributes to a better understanding of methane emissions and coastal methane cycling in seagrass ecosystems.

We also appreciate the reviewer's constructive critique regarding the presentation of the study's scope and significance. We agree that several aspects of the manuscript could be improved to more accurately reflect the strengths and limitations of the dataset and to better communicate the scientific implications of our findings. In response, we have carefully revised the manuscript, including restructuring the Results and Discussion sections, clarifying the distinction between observations and literature-based interpretations, moderating conclusions where appropriate, and more explicitly acknowledging the temporal and methodological limitations of the study. We believe these revisions have substantially improved the clarity, rigor, and overall presentation of the manuscript.

We thank the reviewer for the thoughtful comments, which have helped us strengthen the manuscript considerably.

Comments:

Introduction: The Introduction is currently too broad, contains substantial redundancy, and lacks a sufficiently clear explanation of the study's motivation, knowledge gaps, and objectives. The section requires restructuring. In addition, several citations do not refer the original studies appropriately. The entire Introduction is a single paragraph and should be divided into logically structured paragraphs.

Line 27-30: The information is somewhat redundant in the opening paragraph. It may be better to move more directly toward the relevance of seagrass meadows.

Reply: We thank the reviewer for this suggestion. We agree that the information in the opening paragraph was somewhat repetitive and delayed the introduction of the study's primary focus. In the revised manuscript, we have streamlined the opening section by reducing redundant background information and moving more directly to the relevance of seagrass meadows as important components of coastal carbon cycling and potential methane emission hotspots. We believe this revision improves the flow of the introduction and better establishes the context and motivation for the study.

Line 39: Consider including numerical values to better demonstrate how exceptional these systems are.

Line 40: This process is essentially photosynthesis, therefore, repetition of the previous sentence.

Line 41: maybe even millennia

Reply: We thank the reviewer for this helpful comment. We agree that adding specific numerical values reinforces the significance of these systems. We have revised the sentence.

The sentence now reads (L40-45):

“Seagrass meadows are among the most efficient natural carbon sinks in coastal environments because they sequester carbon through photosynthesis and store it in sediments for centuries to millennia (Lovelock and Duarte, 2019; Mcleod et al., 2011; Rosentreter et al., 2021b; Yau et al., 2023). In addition to long-term carbon storage, seagrasses influence seawater carbonate chemistry through calcium carbonate formation and dissolution (Natividad et al., 2026) and contribute substantially to coastal carbon cycling by exporting

dissolved organic carbon and functioning as net trophic habitats (Chen et al., 2026; Chen et al., 2024). Together, these processes enhance the climate mitigation potential of seagrass ecosystems.”

Line 44: SGWP: abbreviation does not seem necessary

Reply: We thank the reviewer for this suggestion. We have completely replaced the abbreviation and modified the sentence.

Now the text reads (Line46-48): “Despite these benefits, seagrass meadows are also recognized as sources of methane (CH₄), a potent GHG with a sustained-flux global warming potential 96 times greater than carbon dioxide (CO₂) over a 20-year time horizon on a mass basis (Neubauer and Megonigal, 2015).”

Line 48: Consider replacing Bange et al. with a more recent reference.

Reply: We thank the reviewer for the suggestion. We have updated the reference to a more recent study (Weber et al., 2019) and revised the text to reflect these recent estimates.

The sentence now reads (Line 48-51): “Depending on seagrass species, geographic setting, and environmental conditions, CH₄ emissions may offset up to 33.4% of the blue carbon sequestered in sediments (Rosentreter et al., 2021b; Yau et al., 2023; Eyre et al., 2023). More broadly, the coastal ocean represents a major hotspot of marine CH₄ emissions, with continental shelf sediments estimated to release 18–48 Tg CH₄ yr⁻¹ through ebullition alone (Weber et al., 2019).”

Line 57-58: Redundant information

Reply: We thank the reviewer’s comment. We have removed the redundant information.

Line 70: “Intensive field campaign” should be supported with numerical details

Reply: We agree with the reviewer's suggestion. We have revised the sentence to include the specific numerical details of our sampling timeline and strategy.

The sentence has been modified as follows (Line 70-72): “Through three intensive field campaigns combining surface water sampling, in situ benthic chamber incubations, and porewater profiling, we quantified CH₄ production and fluxes in both seagrass and adjacent bare sandy sediments across the sediment-to-water and water-to-air interfaces.”

Materials and methods:

Line 86- Sampling during late May and mid-June does not sufficiently represent two distinct seasons. If seasonal comparisons are intended, additional regional information describing seasonal environmental variability should be provided.

Reply: We thank the reviewer for the comment. We agree that the distinction between late spring and early summer may not be immediately apparent without additional regional environmental context.

To address this concern, we have expanded the description of seasonal conditions in southern Taiwan as well as add Figure 2 to show monthly mean air temperature, total precipitation, and mean wind speed recorded at the Checheng Weather Station during 2022–2023.

Line 121- More detailed information is required regarding the atmospheric CH₄ data, how these data were obtained, including data source, spatial and temporal resolution, and interpolation methods, if applicable. Since atmospheric CH₄ is a key component in flux calculations, this methodological aspect warrants a more comprehensive description.

Reply: We thank the reviewer for this valuable comment and agree that the atmospheric CH₄ data used in the flux calculations should be described in greater detail. In the revised manuscript, we have expanded the Methods section to provide additional information on the source and application of the atmospheric CH₄ data.

Specifically, atmospheric CH₄ concentrations were obtained from the NOAA Global Monitoring Laboratory. Because atmospheric CH₄ concentrations exhibit relatively small spatial and temporal variability compared to the much larger variability observed in surface seawater CH₄ concentrations, we used the NOAA atmospheric CH₄ record as the atmospheric boundary condition for the sea-to-air flux calculations. The

revised manuscript now includes details regarding the data source, temporal coverage, and the rationale for its use. We believe these additions improve the transparency and reproducibility of the flux calculations.

Results and discussion: This section should be separated into two sections. In the current structure, most subsections begin with a presentation of results, followed by broad, loosely connected discussion paragraphs. Many discussion points are insufficiently linked to the study's actual findings. A clearer structure that presents all results first, followed by a dedicated discussion section that directly refers to those findings, would substantially improve the manuscript.

Reply: We thank the reviewer for this insightful comment and agree that the original structure did not provide a sufficiently clear distinction between the presentation of results and their interpretation. As written, the integration of results with literature-based discussion may have obscured the connection between our observations and the broader processes being discussed.

To address this issue, we have restructured the manuscript by separating the Results and Discussion into two independent sections. The revised Results section focuses exclusively on the observations and analyses from the present study, while the Discussion section interprets these findings within the context of previous studies and relevant biogeochemical processes. We have also revised the discussion to more explicitly link each interpretation to the corresponding results and removed or condensed discussion points that were not directly supported by our observations.

We believe these changes substantially improve the clarity, logical flow, and scientific rigor of the manuscript.

Line 172. “Collectively” is vague. Which specific results support this statement? Flux estimates, spatial variability, or seasonal differences?

Reply: We appreciate the reviewer's diligence in pointing out this ambiguity. We have removed the word “collectively” and revised the sentence to explicitly specify the parameters supporting this statement, namely our benthic flux estimations, potential production rates, and porewater profiles. We have also separated the Results and Discussion sections to improve the clarity of the manuscript.

The text has been revised to read as follows (Line 215-232): “Compared with the seagrass meadow, adjacent coastal waters consistently exhibited lower and less variable dissolved CH₄ concentrations. This contrast is likely attributable to the substantially greater water depth (approximately 10 m) at the coastal site, which reduces the influence of sediment-derived CH₄ on surface waters. In contrast, the shallow water depth (0.5–1.5 m) within the seagrass meadow facilitates the transport of benthic CH₄ to the surface, resulting in consistently higher dissolved CH₄ concentrations. Although no significant seasonal differences in surface CH₄ concentrations were observed in either habitat, both the seagrass meadow and adjacent coastal waters acted as net sources of atmospheric CH₄ throughout the study period. Mean sea-to-air CH₄ fluxes were consistently higher in the seagrass meadow ($4.9 \pm 2.2 \mu\text{mol m}^{-2} \text{d}^{-1}$) than in the adjacent coastal waters ($2.7 \pm 2.0 \mu\text{mol m}^{-2} \text{d}^{-1}$; Table 1), indicating that seagrass habitats function as localized CH₄ emission hotspots. These estimates, however, should be interpreted as seasonal baseline fluxes rather than annual budgets because winter sampling could not be conducted due to severe weather conditions, and the autumn observations were strongly influenced by an extreme storm event. The absence of significant seasonal changes in dissolved CH₄ concentrations further suggests that seasonal variability in sea-to-air CH₄ fluxes was primarily governed by changes in wind speed rather than by variations in dissolved CH₄ concentrations, highlighting the dominant role of physical gas transfer in regulating atmospheric CH₄ emissions. Although seagrass meadows are widely recognized for their capacity to sequester carbon over long timescales (Duarte et al., 2013; Fourqurean et al., 2012; Mcleod et al., 2011), our findings demonstrate that they also function as localized atmospheric CH₄ sources. These results emphasize that evaluations of the climate benefits of seagrass ecosystems should consider both their carbon sequestration capacity and their CH₄ emissions, particularly as changes in hydrodynamic conditions and extreme weather events may alter CH₄ exchange between coastal waters and the atmosphere.”

Line 193-211. This section lacks a direct reference to the results presented and reads more like general background information. It should be rewritten to connect to the study's findings. There are also several overstatements.

Reply: We thank the reviewer for this valuable critique. We agree that the original section was overly focused on general background information and did not sufficiently relate the discussion to the findings of the present

study. We also acknowledge that several statements were too broad and could be interpreted as overstatements.

To address these concerns, we have completely restructured and rewritten this section. In the revised manuscript, the discussion is now explicitly linked to our observed results, with interpretations grounded in the data presented in this study. We have also moderated the language throughout the section, removed unsupported generalizations, and clarified where interpretations are based on previous studies rather than direct observations. We believe these revisions have strengthened the scientific rigor of the discussion and improved its relevance to the study's findings.

Line 222-224: There is insufficient evidence presented to support this statement.

Line 225-237: No clear reference is made to the current study results.

Reply: We agree with the reviewer that this section lacked explicit integration with our findings. We have thoroughly revised this paragraph.

New sentences now read (Line 255-267): “Porewater CH₄ concentrations were consistently higher than those in the overlying water, indicating that sediment diffusion represented the primary pathway supplying CH₄ to the water column (Yu et al., 2024). However, only a fraction of the CH₄ produced in sediments is ultimately emitted to the atmosphere because substantial oxidation can occur before reaching the water surface. During daytime, oxygen released by seagrass photosynthesis diffuses into sediments and the rhizosphere, stimulating methanotrophic bacteria that consume CH₄ and thereby reduce net emissions (Miller et al., 2017; Yau et al., 2023). In addition, the decline in porewater CH₄ concentrations with sediment depth observed in this study is consistent with previous findings that sulfate-dependent anaerobic oxidation of methane and competition for electron acceptors can limit CH₄ accumulation in deeper anoxic sediments (Rissanen et al., 2017). Although diffusion was the dominant transport pathway quantified in this study, ebullition may also contribute to CH₄ release when porewater CH₄ partial pressure exceeds hydrostatic pressure, allowing CH₄ bubbles to bypass oxidation within the sediment and water column (Sun et al., 2022). Therefore, the observed porewater CH₄ distributions likely reflect a dynamic balance among organic matter availability, microbial methanogenesis, aerobic and anaerobic CH₄ oxidation, and multiple transport pathways from sediment to the overlying water (Henriksson et al., 2024; Yau et al., 2023).”

Line 243-245: What evidence supports the conclusion that seagrass meadows are a persistent CH₄ source? Additionally, “severe weather conditions” should be defined more clearly.

Reply: We thank the reviewer for this comment and agree that the statement was insufficiently supported in the original manuscript. Our intention was not to imply that persistence was demonstrated solely by the present study, but rather to place our observations within the broader context of existing literature.

To clarify this point, we have revised the text and provided additional supporting references. Numerous studies have identified seagrass ecosystems as sources of CH₄, with reported methane emissions offsetting up to 33.4% of the blue carbon sequestered in seagrass sediments, depending on seagrass species, geographic region, and environmental conditions (Rosentreter et al., 2021b; Yau et al., 2023; Eyre et al., 2023). In the revised manuscript, we have moderated the wording to avoid overgeneralization and clarified that the characterization of seagrass meadows as CH₄ sources is based on a growing body of evidence from multiple studies rather than solely on the results of the present study.

We have also revised the Line 243-245. Now the text reads (Line 297-307): “These contrasting responses were also reflected in the seasonal averages. Mean sediment-to-water CH₄ fluxes were $88.2 \pm 41.2 \mu\text{mol m}^{-2} \text{d}^{-1}$ in the seagrass meadow and $150.4 \pm 207.0 \mu\text{mol m}^{-2} \text{d}^{-1}$ in the adjacent sandy sediments (Table 2). However, the higher seasonal mean for sandy sediments was largely driven by the exceptionally high emissions recorded during the storm-affected autumn sampling. Excluding this episodic event, seagrass sediments consistently exhibited higher baseline CH₄ production and benthic fluxes, whereas sandy sediments generally maintained much lower emissions. These findings indicate that seagrass sediments function as persistent CH₄ sources sustained by continuous organic matter decomposition and microbial methanogenesis, whereas unvegetated sandy sediments are characterized by relatively low background emissions punctuated by irregular, disturbance-driven CH₄ pulses. As climate change is projected to increase the intensity and frequency of typhoons and other extreme weather events (IPCC, 2021), episodic CH₄ release

from sandy coastal sediments may become an increasingly important component of coastal GHG budgets, even though seagrass meadows remain the dominant long-term hotspots for benthic CH₄ production.”

Line 252-254: More information is required for the overall circulation in the area, and more comments on the lateral transport of methane.

Line 255: This statement is not adequately supported by evidence.

Reply: We thank the reviewer for these important comments. We agree that the original manuscript did not provide sufficient information regarding the hydrodynamic setting of the study area and the potential role of lateral methane transport. To improve clarity, we have added a conceptual figure (Figure 3) illustrating the geographic setting of the study area and the potential pathways of methane transport. We have also expanded the discussion to better describe the possible influence of local circulation and lateral transport processes on methane distribution and fate within the seagrass meadow. We also agree that the statement in Line 255 was not adequately supported by the available evidence. In particular, the annual evaluation was subject to considerable uncertainty because winter observations were not available and the dataset did not cover a complete annual cycle. To address this limitation, we have removed the annual evaluation from the revised manuscript and instead focus on seasonal flux assessments based on the periods that were directly sampled. We believe this revision provides a more robust and defensible interpretation of the data while avoiding conclusions that extend beyond the scope of our observations.

Line 257-263: The discussion again lacks direct reference to the observed results.

Line 264: This appears to introduce a new subsection or topic and may require restructuring.

Reply: We thank the reviewer for this helpful comment. We agree that the original discussion did not sufficiently refer to the results presented in the study and that the transition at Line 264 introduced a new topic without a clear structural framework.

To address these concerns, we have revised this section and restructured the manuscript. Specifically, we have separated the Results and Discussion sections and reorganized the discussion to ensure that interpretations are directly linked to the corresponding observations and findings from the present study. We have also revised the section beginning at Line 257 with direct reference and incorporated it into a more appropriate structure within the Discussion, improving the logical flow and clarity of the manuscript.

Line 277- 280: The interpretation appears overstated relative to the scope of the presented data.

Reply: We thank the reviewer for this comment and agree that the interpretation was stronger than warranted by the available data. Our intention was to provide a possible explanation for the observed patterns; however, we acknowledge that the original wording may have implied a level of certainty that was not supported by direct evidence.

Line 281“Taken together” is unclear. Which findings are being synthesized here? If this serves as a concluding statement, it may fit better in the conclusions.

Reply: We thank the reviewer’s comment. To improve the organization of the manuscript, we have relocated this concluding statement to the Conclusions section.

Line 293-295: This also contains overstatements.

Reply: We thank the reviewer for this comment and agree that the interpretation might be stronger than warranted by the available data. In the revised manuscript, we have moderated the language and reframed the interpretation as a potential mechanism rather than a demonstrated conclusion. We have also ensured that the discussion remains closely tied to the observations presented in this study and have explicitly acknowledged the limitations of the dataset where appropriate. We believe these revisions provide a more balanced and evidence-based interpretation of the results.

Line 304: “78.3 moles” requires a complete unit description (e.g., mol m⁻² yr⁻¹).

Reply: We thank the reviewer’s comment. We have removed the annual evaluation from the revised manuscript.

Line 310-311: The statement seems redundant

Reply: We thank the reviewer's comment. We have removed the redundant statement from the revised manuscript.

We thank Professor Knut Ola Dølvén for this thoughtful and detailed assessment of the atmospheric methane flux calculations and the fate of sediment-derived methane. We particularly appreciate the reviewer's effort in illustrating the potential importance of methane oxidation and atmospheric exchange processes, as well as the uncertainties associated with the ultimate fate of laterally transported dissolved methane.

Regarding the use of the atmospheric equilibrium concentration in the sea-to-air flux calculations, our intention was to quantify the observed air-sea CH₄ exchange following the widely adopted bulk flux approach of Wanninkhof (2014). We acknowledge the reviewer's point that alternative baseline assumptions may yield different perspectives on the contribution of local CH₄ production to the atmospheric CH₄ budget. However, based on current understanding, surface ocean waters are generally supersaturated with respect to atmospheric CH₄, and the global ocean is recognized as a natural source of atmospheric CH₄, contributing approximately 1–4% of annual global CH₄ emissions, with estimated diffusive fluxes of 2–6 Tg CH₄ yr⁻¹ (IPCC, 2013; Weber et al., 2019). Thus, the calculation for the Eq (3) (in the comments) may reduce the importance of seagrass as a CH₄ source.

We agree with the reviewer that our approach does not account for methane transport via free gas bubbles (ebullition). As we did not directly quantify ebullition during this study, we have added a discussion of this limitation in the revised manuscript and acknowledge that bubble-mediated transport may represent an additional pathway for CH₄ release that is not captured by the dissolved methane flux calculations.

Furthermore, we appreciate the reviewer's discussion regarding the potential fate of laterally transported dissolved CH₄. We agree that the proportion of CH₄ ultimately oxidized versus released to the atmosphere remains highly uncertain and depends on hydrodynamic transport, atmospheric exchange rates, and microbial oxidation processes. We have revised the discussion to more clearly acknowledge these uncertainties and to highlight that a portion of the dissolved CH₄ transported away from the study site may ultimately contribute to atmospheric emissions. At the same time, we note that constraining these processes would require additional measurements and modelling beyond the scope of the present study.

Finally, in response to comments from all reviewers, we have removed the annual methane budget calculation and associated annualized interpretations from the manuscript because the dataset does not include winter observations and does not represent a complete annual cycle. The revised manuscript instead focuses on seasonal methane fluxes observed during the sampling campaigns and explicitly acknowledges the limitations associated with extrapolating beyond the measured periods. We have also updated all figures with higher-resolution versions.

line-by-line comments

Line 24-25: Check my comment on dissolved atmospheric flux. This sentence should also be slightly modified.

Line 61 or Line 69: New paragraph. In general, split the text blocks more.

Reply: We thanks Prof. Dølvén's suggestions and have modified it.

Line 131: Does the insertion of the chambers affect the methane flux to the overlying water? If there's a chance

for this, it should be commented on.

Reply: We thanks Prof. Dølvén's comments and add the limitation of the experiment. Now the text reads (Line 324-333): "Although in situ benthic chamber incubations are among the most representative approaches for quantifying sediment-to-water fluxes because they preserve the natural sediment-water interface while maintaining ambient light conditions, hydrodynamics, and biological interactions (Mallon et al., 2022), the chamber environment may still influence measured fluxes. Over the incubation period, changes in oxygen availability and microbial activity can alter CH₄ production and oxidation rates, particularly during longer deployments (Villnas et al., 2012). In addition, environmental conditions within the chamber may become more stable than in the surrounding environment, potentially enhancing photosynthetic activity and metabolic rates (Olivé et al., 2016). Conversely, shading by the chamber and suspended particles may reduce light penetration and photosynthetic efficiency, thereby affecting oxygen production and CH₄ oxidation

(Mallon et al., 2022). Although these factors are unlikely to alter the observed habitat-specific and seasonal patterns, they may influence the absolute magnitude of the measured benthic fluxes.”

Line 145: Is DO (dissolved oxygen) defined?

Reply: Yes, it's in Line 109-110: “Water samples were collected using a three-way valve installed on the incubator, while temperature, salinity, and dissolved oxygen (DO) were measured simultaneously.”

Line 155: Probably not – horizontally advected, surface water comes from somewhere else.

Reply: We thanks Prof. Dølven's comments and have modified it.

Now the text reads (Line 215-220): Compared with the seagrass meadow, adjacent coastal waters consistently exhibited lower and less variable dissolved CH₄ concentrations. This contrast is likely attributable to the substantially greater water depth (approximately 10 m) at the coastal site, which reduces the influence of sediment-derived CH₄ on surface waters. In contrast, the shallow water depth (0.5–1.5 m) within the seagrass meadow facilitates the transport of benthic CH₄ to the surface, resulting in consistently higher dissolved CH₄ concentrations. Although no significant seasonal differences in surface CH₄ concentrations were observed in either habitat, both the seagrass meadow and adjacent coastal waters acted as net sources of atmospheric”

Line 163: Weird μ sign

Reply: We thanks Prof. Dølven's comments and have modified it.

Line 164: Likely or not? You do the calculation, so you can determine this

Line 165: New paragraph

Line 176-181: What does the range mean? Standard deviation? Not obvious, since the distribution is not Gaussian.

Line 191: Ref my comment on Line 131

Line 242: Higher methane fluxes in sandy areas? Is this correct? This is a bit confusing and the text should clarify expectations and results for sandy vs meadow environments better. Is the sandy area a “control” area?

It does not seem like this since the flux seems to be periodically larger there.

Reply: Thank you for this helpful comment. We agree that the original text was unclear. We have revised the manuscript to clarify this point and added a new discussion section (Section 4.3, Contrasting Benthic CH₄ Dynamics Between Seagrass and Sandy Sediments) to provide a more detailed comparison of the two environments.

Line 252: It is unclear what is meant by “At this concentration, only 0.39 mol CH₄ was present in the water column at any given time.” What water column?

Reply: Thank you for this helpful comment. We agree that the original text was unclear. We have revised the manuscript to clarify this point. Now the text reads (Line 318-320): The relatively small standing stock of dissolved CH₄ in the water column (0.33–0.43 mol across seasons), despite substantial benthic inputs, further supports the rapid turnover and efficient redistribution of CH₄ within this shallow, hydrodynamically active seagrass ecosystem.