Response to Reviewer #2

Authors response: We appreciate the reviewer's constructive comments, which have helped improve the clarity and focus of the manuscript. We reorganized the section on the analysis of the atmospheric data from Westerland and added two new describing figures in the Appendix. Our detailed responses to each comment are provided below, with the reviewer comments depicted in black, the author reply in blue.

The paper by Schmidt et al. describes the result of a 1-day field study to identify the source of methane from a sand extraction site offshore Sylt, which had been recognized based on continuous measurements of the atmospheric mole fraction of the ICOS station Westerland. While the findings are interesting and surely deserve publication, and while it is clear that several questions cannot be answered by the result of a one day survey, there are some points that should be clarified or exploited a little bit more before final publication. This bears also the potential to increase the impact of the study. The most important suggestion is to expand the exploitation of the atmospheric data from Westerland, which could help to add substantiate some statements which are so far rather unsupported.
I start to address the major points, followed by some minor mostly editorial aspects.

Major:
The authors should expand the information on the magnitude and distribution of sand extraction in the North Sea/European/Global context in the introduction. They do so later in the discussion, also reasoning why extrapolation of results is difficult, but should put some information in the introduction. That would leave room in the discussion to be even a little bit more quantitative, potentially introduce a European wide map etc. In that regard, the short statement on the European perspective in Lines 99-100 should be moved out of the site description, where it does not belong.
Authors response: We have added a short paragraph in the Introduction providing context on marine sand extraction in the North Sea and Europe. At the same time, the introduction remains focused on the local study system, as this study describes a site-specific phenomenon. Because environmental and operational conditions vary substantially between extraction sites, we avoid implying a direct extrapolation of the observed methane signals to larger spatial scales. Following the reviewer's suggestion, the short statement on the European perspective previously included in the site description (former Lines 99–100) has been moved to the Introduction. The additional paragraph in the introduction reads now:" In 2022 alone, approximately 1 million m^3 of sand were dredged from this area. From Westerland II, the extracted aggregates are transported to the beach and foreshore on the west coast of Sylt (LKN.SH 2022). On a broader scale, marine aggregate extraction in the European North Sea is estimated to roughly 100,000 kt per year, corresponding to about 62.5 million m^3 per year, and is used for construction, land reclamation, and beach nourishment (Porz et al., 2026). In addition, theThis topic of marine aggregate extraction is relevant throughout Europe and can be regarded as a use of marine resources of increasing relevance for the future (e.g., BSH, 2021)

Methodology:
Lines 119 to 120: Instrumentation of the meteorological measurements should be given.
Authors response: We added the instrumental information of the meteorological sensors.

RC2: Line 137: while it is stated how time lapse was corrected for the atm. measurements, the delay and info on flow rate / potential contamination in the pump/bucket, water supply should be briefly addressed
Authors response: This section is now elaborated to: “The time lag between water intake and corresponding signal detection was determined in the laboratory by switching the inlet between aerated freshwater and saltwater. All data were corrected accordingly. To convert the relative concentrations (ppm) reported by the GGA to absolute concentrations (nmol L-1), discrete water samples were collected during the day (Bussmann et al., 2024). The CH4 concentration in these samples was analyzed by gas chromatography following Magen et al., 2014. A correlation between the methane concentration of the water samples and the readings of the GGA at the same respective time was used to convert all GGA readings to concentrations of dissolved methane. To test the lower sensitivity of the setup, aerated freshwater with an equilibrium concentration of 2.9 nM was measured in the laboratory, and the instrument readings gave a concentration of 2.3± 0.3 nM.”

Line 147: it is mentioned that the ASE exchange is calculated after Wanninkhof 2009, while line 154 and 155 clearly state that Nitghingale 2000 was used. Was something else extracted from Wanninkhof 2009; or is this just an erratum?
Authors response: The Equation 1 on the principle of the diffusive flux calculation is taken from Wanninkof 2009, while the influence of the wind speed on the gas exchange (Equation 3) is taken from Nightingale.

Line 179: it is stated that only one sample was measured for surface waters, but in line 167, it is stated that there were 3 samples taken for each surface point. Likely, the authors measured all three samples per site. So for the surface, you measured 3x one sample and for the deep water, you took 3 measurements from one sample? Please clarify.
Authors response: Lines 176-181: Section 2.4 has been revised to clarify the sampling numbers and to explicitly differentiate between surface transect and depth profile sampling. We modified to: “Surface water samples were collected at 11 locations along the transect, and depth profiles were obtained at 4 locations: location #1 at 3 depths and locations #2–#4 at 2 depths each. At all sampling locations and depths three 1 L glass bottles were filled to the brim with seawater. Each glass bottle was used to obtain one gas sample for the measurement of CH_4 concentration, $\delta^{13}C$-CH_4, and $\delta^{2}H$-CH_4 values (n = 1). Surface samples were collected via a tap connected to the shipboard pump system (no filters or desalinators), while depth profile samples were filled directly from the Niskin bottle.”

Missing information in methods:
It is mentioned that two air-inlets, one at 8 m and one handheld inlet for bottom near atmospheric sampling, were available, to be selected by a 3 port valve. But it is not clear which inlet was used when. So far, there is only indication for one transect sampled twice, one time at 8 m and one time handheld near the surface. Was that the only occasion the sea surface near sampling was performed. Please specify.
Authors response: We added a new figure to the appendix (Fig A4) that shows at which times each intake line was used. We added the sentence “The hand-held sampling line was used once during the first transect from dredging area 1b to 1a (the northern direct transect in Fig. 5b) and a second time during the transect above the mud track from the actively dredging hopper dredger (south of the ship in Fig. 5b).”

In line 366 (discussion), it is mentioned that earlier mobile air sampling was used to come to the conclustion that the methane source detected at ICOS station Westerland was likely offshore and likely in direction of the extraction site. However, this part is not mentioned at all in the method part. Please add.
Authors response: Lines 121-124 describe that the mobile measurement device was used on the ship and in the vicinity of the station for measurements. “ The atmospheric CH4 and CO2 mole fractions were measured on board of RV Mya II, **at the beach nourishment site**

in List, and in the surroundings of the atmospheric monitoring station Westerland using an Optical Feedback-Cavity Enhanced Absorption Spectroscopy (OF-CEAS) trace gas analyser (LI-7810, LI-COR, Lincoln, USA)."
To improve clarity, we added further information to the manuscript. "For measurements conducted in the vicinity of the Westerland station and at the nourishment site, the instrument was carried in a backpack and used to determine CH_4 concentrations in ambient air via a 3 m long Teflon tube or in soil air using a static chamber."

Results:
It is not clear for me why the atmospheric duration of the record chosen to demonstrate the detection of the spikes in CH4 is so short (Fig 3). Apparently, the authors picked the same period in 2022 as is discussed in 2023 in connection with the campaign. However, data from station Westerland are available for a long time (as the dredging activity). Moreover, the high frequency data should be available at least since 2021, though the effect might be traceable also in the 1 min data set obtained before. The value of the atmospheric record over a longer time period is that several assumptions and statements could be proven by looking at the atmospheric data for a longer time period, together with wind re-analysis data (or local wind data from the station).

- The statement that this phenomenon occur mostly in summer; more specifically, that it occurs mostly while active dredging occurs.
- The statement that the emissions occur mostly during low tide, which cannot be judged based on these few days that are displayed currently in Fig. 3.
- This analysis could also further narrow down the wind direction which is required for the emissions to be visible in the station's record. This could shed some light on the question whether the weaker signal in 2023 and some of the variability observed in the strength is a function of the wind direction rather than the source strength.

Apparently, this work was at least partly done for the year 2022, as indicated in the lines 247-254. I think this part should be moved up, before the results for 2023 (so avoiding the 2022-2023-2022 jump), and substantiated by Figures /Tables. Currently these very important statements are based on data not shown.
Authors response: We followed the reviewer's recommendations and reorganized the chapter to provide a clearer structure. The analysis of the 2022 data was moved earlier in the chapter and further expanded. That the CH_4 spikes occur mainly during the summer has now been supported with an additional figure (see Appendix Fig. A2). Other characteristics, such as wind dependence and tidal influence, are already clearly illustrated for our example week in Figure 3. We examined all the 2022 data individually and applied various filtering criteria to derive these characteristic patterns. We added the sentence " Figure A2 shows a histogram of the number of days with CH_4 spikes per month in 2022, indicating that most spikes occurred in June, July, and August, some in May and October, and very few in January and March."

It would be also interesting whether there are times in the dredging season when no ship is operating. So the question on whether the emissions are connected to the active action of extraction or to the characteristics of the young seafloor depressions caused by the extraction could be narrowed down further. This is particular important as direct linkage to the extraction action, which is somehow implied by the field data (highest emissions in direct vicinity to the extraction activity and related to a plume of suspended material) is difficult to be linked to a tidal control (assuming there is no reason to believe that extraction activities are mostly executed during low tide, something which would be important to know).
Authors response: We thank the reviewer for this helpful suggestion. To address this point, we analyzed dredger activity data from 2022 and included the following information in the manuscript: " A comparison with sand dredger activity data showed that in June 2022, when

dredging operations paused for two days, the CH_4 peaks continued to follow the usual tidal pattern (see Fig. A3), indicating that the emissions are not exclusively associated with the active extraction process but may also be influenced by the characteristics of the young seafloor depressions created by dredging."

Figure 5 and Lines 273-277
If I understood it correctly, one transect near the ship was measured twice, one time with the 8m inlet and one time with the near—surface handheld inlet, and the considerable difference of 63 ppb (mean), the authors infer that there is a local source, leading to the near surface gradient. I strongly suggest to show these data separately, either in the text or in the appendix, as two distinguishable data sets against latitude, longitude, or distance, and indicate the position in Figure 5. The presentation in Fig 5 is not suitable to display the finding.
Authors response: To clarify, we have added the following text and an additional figure in the Appendix showing the timing of the use of the different sampling lines. "The hand-held sampling line was used once during the first transect from dredging area 1b to 1a (the northern direct transect in Fig. 5b) and a second time during the transect above the mud track from the actively dredging hopper dredger (south of the ship in Fig. 5b). An additional figure showing when the different sampling lines were used is included in the Appendix (Fig A4)."

Line 293 to 295: The sentence starting with "The comparison between …". I am dismayed, but I cannot understand the argument given in this sentence. Maybe rephrase?
Authors response: This sentence is now deleted, as it is really confusing.

Line 308-309: what do you mean by "similar range" here. As you state correctly, the Westerland II values are clearly more positive (or less negative).
Authors response: We agree that the term "similar range" was misleading, as the Westerland II values are clearly more positive (i.e., less negative). For clarity, we have deleted the phrase "but in a similar range." The sentence now reads: "Thus, the δ^2H–CH_4 values from Westerland II were more positive compared to the North Sea samples."

Line 346 and Fig 10b. Given the limited range of data and the relatively large error range of the data, I found the error on the intercept (66 ± 6) astonishingly small. And I was astonished that, while you apparently used a York fit because of uncertainties of the data in both dimensions, the data displayed have only an error indicated for the Delta value. Please check.
Authors response: We have carefully checked the program code and the input data, and everything is correct. The delta errors for the CH_4 measurements are between 0.2 and 0.4 ‰; however, the CH_4 concentrations themselves are very small (<0.5 ppb) leading to an error of the 6‰ for the intercept.

Discussion
Lines 366 to 368. As mentioned further up, this comes "out of the blue" as these measurements were not introduced or specified in the method section.
Authors response: We have provided more details of these measurements in the methods section. "The atmospheric CH_4 and CO_2 mole fractions were measured on board of RV Mya II, at the beach nourishment site in List, and in the surroundings of the atmospheric monitoring station Westerland using an Optical Feedback-Cavity Enhanced Absorption Spectroscopy (OF-CEAS) …… For measurements conducted in the vicinity of the Westerland station and at the nourishment site, the instrument was carried in a backpack and used to determine CH_4 concentrations in ambient air via a 3 m long Teflon tube or in soil air using a static chamber."

Line 378: “Therefore we did not expect …”. Please extend a little on the rationale. One could also argue, given the shallow water depth, that the storm induced resuspension leading to enhanced concentration. Apparently not the case, but the rationale for the statement is not explained.
Authors response: We added following explanation: “The effect of a storm on atmospheric and dissolved methane concentrations can be described in two ways. On the one hand, sediment resuspension caused by increased bottom stress from wind and waves could increase the release of methane from sediments. On the other hand, a storm can substantially increase the gas transfer velocity, thereby increasing the diffusive flux of methane out of the water. The overall effect on the atmospheric methane concentration varies depending on water depth and sediment structure. As we only observed minor enhancements, we assume that the effect of sediment resuspension was negligible.”

Lines 386-389: Using READY to estimate the relation between offshore source strength and signal strength at the Westerland station adds value to the manuscript. However, it is not fully exploited. To what extend could larger peaks also result from a better matching wind direction or differences in flow regime? It would also be interesting to do a run for some of the very strong peaks observed in the 2022 data, which appear substantially larger than 15 ppb. As you ran the model already, adding this information might be easily done.
Authors response: The observed CH4 peaks during the ship campaign were around 0.15 ppb. For the READY simulation, we tested peaks of 0.15ppb and also 15 ppb, which are more typical for 2022, in order to illustrate the expected range. The aim is to provide a case study for typical conditions rather than to explore all possible peak scenarios with this simple model.

Line 390: as mentioned, I think something on the extend of dredging for marine aggregates should be already mentioned in the introduction, and you might consider to extend a map here, at least in the appendix.
Authors response: We have revised the Introduction to include additional context on the extent of marine aggregate dredging in the European North Sea. We now cite Porz et al., 2026, which provides maps of dredging and dumping locations in the North Sea.

Lines 468-470. The attribution of the concentration and isotopic findings at station #4 to oxidation is questionable, given the very short residence time of the water in the suggested methane source area. Turnover rates at methane concentration levels as reported in this study are likely weeks to months (e.g. Nauw et al., 2015). Displacement of water, i.e. hydrographic reasons, appear more likely.
Authors response: We agreed. The section has been revised to reflect that the isotopic enrichment and decreased CH_4 concentrations may result from several processes, including mixing or exchange with water masses characterised by different CH_4 sources or a higher degree of prior CH_4 oxidation. While CH_4 oxidation cannot be excluded, hydrographic drivers such as water mass displacement are now explicitly acknowledged as equally plausible explanations. The revised text reads now: “At location #4, CH4 concentrations decreased with depth while δ13C-CH4 and δ2H-CH4 values changed towards more positive values. This pattern may reflect several processes. While CH4 oxidation could contribute to the isotopic enrichment of the residual CH4 pool, mixing or exchange with water masses influenced by different CH4 sources and thus characterized by different isotopic values of CH4 or a higher degree of prior CH4 oxidation may equally explain the observed concentration and isotopic gradients.”

Last sentence of conclusion: While the findings of this work are really novel and exiting, before calling for long-term observation and mitigation strategies, an assessment of the importance of the process in comparison to other sources would be needed.
Authors response: We have revised the sentence according to the reviewer's recommendation to: "Due to the frequency of dredging operations and their potential impact on regional GHG budgets, more investigations are necessary improve our understanding on the importance and magnitude of anthropogenic CH_4 sources in coastal environments."

Minor:
Line 30: … dredging site COMMA characterized… done
Line 59: ..inputs from riverine => from rivers done
Line 72: approximate volume of more than .. change to approximate volume of 57 million …. (the No is later specified to 56.9) done

Line 91: … depth of 20m below seafloor => 20 m deep (you characterize the depression relative to the surrounding. Done

Line 92: The infill … ; sentence has to many "and"; maybe split to two sentences? Done

Fig 1: please give source of bathymetric data
Authors response: The bathymetry is based on different sources, which we added in the caption of Fig 1.

Line 357: … (wind from the west) requires => calls for done

Line 370: "Circling around the vessel resulted in increased atmospheric CH4 concentrations" Please rephrase – the increased concentrations are not a result of the movement of the Mya.
Authors response: We rephrase to "Elevated atmospheric CH_4 concentrations were observed while circling around the vessel."

Line 378: "In addition, storm Poly, passed …" ; Remove the commas done

Lines 406-407: Dissolved CH4 concentrations have been measured in the area south of our study area, but with the same method. => … study area, using the same method; I cannot see the contradiction indicated by the word "but".
Authors response: We changed to: "**With the same method** dissolved CH_4 concentrations have been measured in the area south of our study area."

Line 479: remove one "the ship campaign" done
Line 480 : none AT all done

Line 482-483: "Such atmospheric measurements …". I do not understand the argument. How can you calculate the emissions just from atm. concentration measurements? And do you refer to the atm. measurements at station Westerland or at the extraction site
Authors response: We modified the sentence to :" Such atmospheric measurements in combination with an atmospheric transport model….. "