	Comment	Response and action taken
Reviewer 1		
Overall comment	This is a very interesting, nicely conducted study that deserves publication. The experiment has been conducted with care and the research question is clear. However, some methods need to be described in more detail, some results should be reported in more detail and some sections of the discussion should be expanded to exploit the scientific benefit that the data harbours.	We would like to thank reviewer 1 for taking the time to give their thorough feedback and useful comments on our paper. We expanded the different sections as requested to explain the methodology and results in more detail, and also expanded on the discussion. For in depth replies, actions taken, and changes in the manuscript please see the comments below.
Major comments	General: Were there no roots in the soil/sediment cores? If mangrove soils store carbon sequestered by the trees, this carbon must get into the soil somehow.	Roots and undecomposed organic matter were avoided where possible as the aim was to estimate greenhouse gas (GHG) emissions from the soil rather than the mangrove trees. Given the thickness of roots at the sampling depth, these were relatively easy to avoid. It is well documented that the density of pneumatophores is related to GHG emissions (e.g. Lin et al, 2021; Sheng et al, 2021). The soil organic carbon comes from a combination of decomposed mangrove roots, leaves, and organic matter from other ecosystems (allochthonous inputs), which is decayed by primary consumers. Action taken: To explain better, we added a sentence in the methods section explaining that we avoided the roots when sampling. It now reads: *"While sampling the sediment, roots and undecomposed organic matter were avoided as the aim was to estimate GHG emission from the soil rather than the mangrove trees."*
	L 40-43: Maybe add a sentence elaborating how this aridity influences mangrove growth. Does it also play a role in selecting the dominant mangrove species?	Thank you for the suggestion. We updated the text to clearly define *Avicennia marina* as the dominant mangrove species in the Red Sea and added more information on the adaptations of *A. marina.* It now reads: *"Consequently, Avicennia marina, the dominant mangrove species in the Red Sea, is better adapted to the high salinity and aridity, and found predominantly as monospecific mangrove stands (Khalil, 2015). Rhizophora mucronata are also found within the Red Sea but predominantly in Southern regions where there is lower salinity (Khalil, 2015; Ahmed & Abdel-Hamid, 2007)."* Moreover, we added the following text:

		"The conditions in the Red Sea result in reduced growth of A. marina, with trees only reaching 2-3 meters compared to over 16 meters in Australia (Mackey, 1993) The high temperatures, and nutrient-limited conditions prevalent in the Red Sea result in stunted growth and dwarf forms of mangroves (Almahasheer et al., 2016)."
Minor comments	L 44: Add "also" following "…mangroves is…" as the small tidal range is probably not the only constraint on mangrove distribution.	Thank you for the correction. We modified the text accordingly. It now reads: *"In the central Red Sea, the distribution of mangroves is also constrained by the small tidal range, which is typically less than 1.5 m (Blanco-Sacristán et al., 2022)."*
	L 59: What did Sea et al. (2018) find out? It is only one study, but probably one with some results.	In the Sea *et al.* (2018) paper it reads: Diel CO2 and CH4 emission rates ranged from −3452 to 7500 µmol CO2 m−2 d−1 and from 0.9 to 13.3 µmol CH4 m−2 d−1 respectively It now reads: *"Fluxes have been found to range from −3452 to 7500 µmol CO2 m^{-2} d^{-1} and 0.9 to 13.3 µmol CH_4 m^{-2} d^{-1} across different locations in the Red Sea (Sea et al., 2018)."*
	L 115-116: I have a hard time imagining that 1 2h of equilibration time are sufficient. Did you test whether these 12 h suffice indeed?	This was substandard word choice and we apologize for the confusion. For clarification, the term "Equilibration" was only intended to mean the time between closing the top lid and the T0 measurement, which has now been replaced by the term 'stabilization' (Garcias-Bonet & Duarte, 2017). In April, May and July (2021) collection was in the early morning. After transportation to the lab the cores were placed into the incubator to keep them at target temprature. One by one, bottom and top lid were exchanged from "collection lids" to air-tight lids used for the duration of the experiment. After sealing/closing the lids, we allowed 1 hour before taking the T0 measurement (following the established protocol of Sea et al., 2018). Due to variability in the tides, we decided to move sampling time of the soil cores to the afternoon on the day before. This was necessary due to unpredictable tidal conditions and logistical challenges of early morning sampling. The cores were then stored in the incubator at target temperature (with open top lid) approximately 12 hours until closing them and taking T0 measurement. Leaving the cores unsealed and undisturbed for this period was to allow for regular gas exchange following disturbance of the soil caused by collection in the field. Moreover, we wanted to avoid creating anoxic conditions in the sediment and water by closing the core too soon before the experiment. In previous studies using similar methodologies (e.g. soil incubation, gas sampling), soil cores were left for a period between 1 hour to 48 hours before the first gas sample collection (Kristensen et al., 2000; Werner et al., 2014; Christiansen et al., 2015; Perez-Villalona et al., 2015; Sea et al., 2018). If there

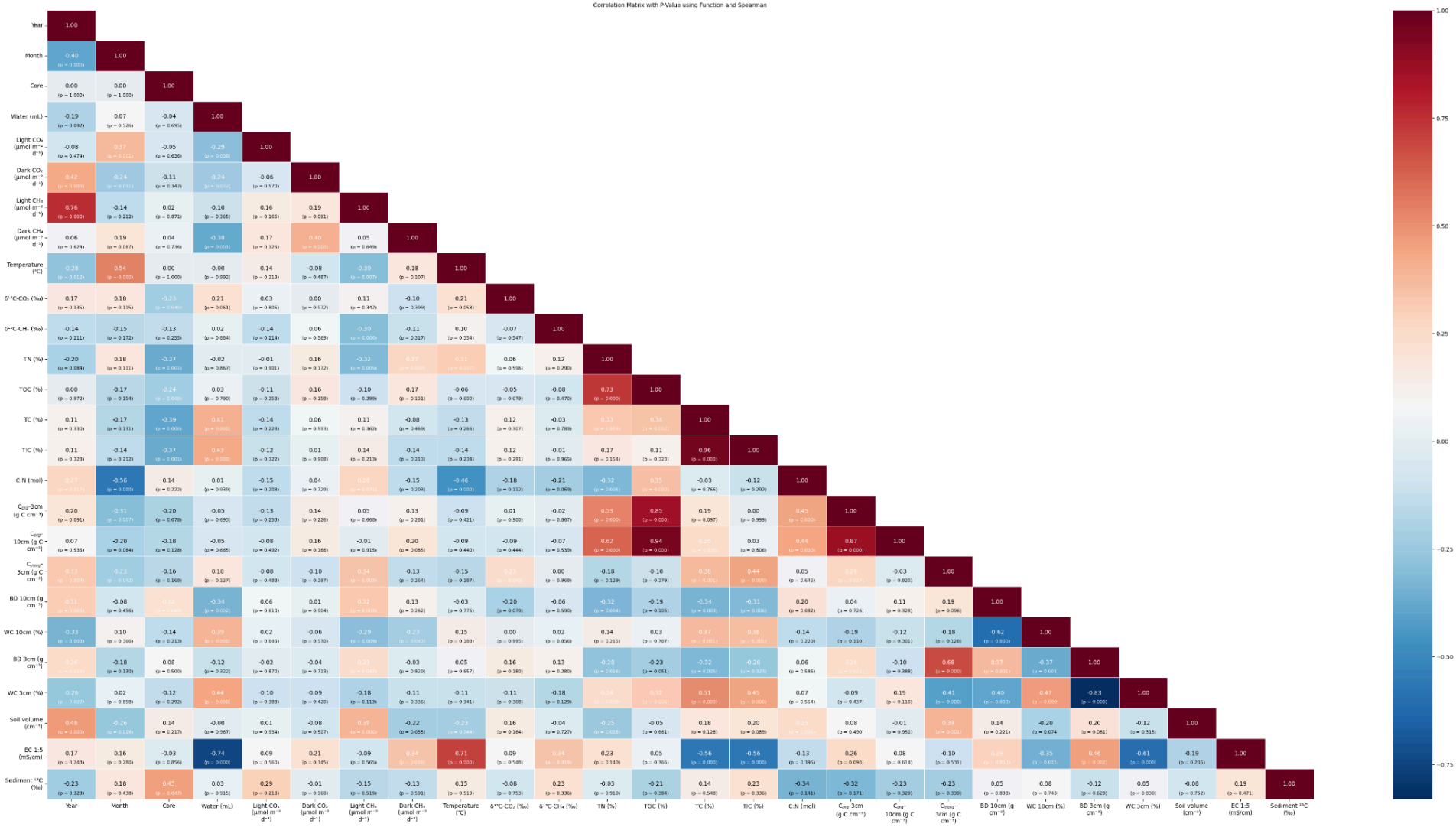
		was water present, extra water was sampled with the sediment cores and also placed in the incubator to keep the temperature stable. In the morning of the start of the experiment, water was exchanged, if present, 1 hour before the collection of the first gas sample (T0) (Kristensen et al., 2000; Sea et al., 2018). The average CO_2 concentration (ppm) for T0 across the whole duration of the study was 547 ± 88 ppm, within a normal range for a laboratory during a period of low human activity (Hussin et al., 2017). In April, May and Jul the average was 533ppm, and the T0 measurement was not significantly affected by early morning sample collection, compared to collection the afternoon before the T0 measurement. Action taken: The text in the methods has been updated to clearly reflect this and minimize ambiguities. It now reads: *"Two sets of cores were collected each month. The first set of cores comprised four large clear PVC cylinders (height: 30 cm, diameter: 9.6 cm) inserted into the soil to a depth of 10 cm and retrieved without disturbing the soil layers. If water was present during sampling, it was retained within the cylinder up to a maximum height of 10 cm to ensure a minimum of 10 cm of air headspace for incubation, and without disturbing the soil-water interface. Initially, in April, May, and July (2021) collection of the soil cores was conducted in the early morning hours allowing for sufficient time to transport and process soil cores with a stabilisation period for 1 hour between sealing the core and taking the T0 gas sample at 7am (following the protocol of Sea et al, 2018). Subsequent sampling events, until the study conclusion, were conducted late afternoon on the day before for logistical reasons, with the cores left unsealed in the incubator under darkness to mirror night-time conditions. Leaving the cores unsealed and undisturbed was to allow for regular gas exchange following disturbance of the soil caused by collection in the field, and to avoid the creation of anoxic conditions in the sediment and water overnight before the start of the experiment. If water was present at the time of sample collection, extra water was sampled with the sediment cores and also placed in the incubator to keep the temperature stable. On the morning of the start of the experiment, water was exchanged, and the air-water interface of the sealed cores was allowed to stabilize for 1-hour before the collection of the T0 gas measurement at 7am (following the protocol of Sea et al, 2018). The 1-hour stabilization was not required for cores without the water phase there was no water to exchange, and no water-soil interface to influence gas exchange dynamics. There was no significant difference in T0 concentrations with or without water."*
	L 117-118: What is "light"? Laboratory light intensity? Was there a climate chamber to provide the high light intensity prevailing in the region? Or were the cores outside the lab for the "light" phase?	The text was modified to specify the light intensity within the laboratory incubator, which was 125 micromoles m^{-2}/sec^{-1} of light irradiance according to the manufacturer

		with the lights set to 100% brightness, reflecting the light intensity in the study region. It now reads: *"Three gas samples of 25 mL per core were taken starting at 7 am (T0), after 12 hours of light (T1), and the final sample (T2) after 12 hours of darkness. For the duration of the light condition, incubator lights were set to 100 % intensity at* 125 µ mol m^{-2}/sec^{-1} irradiance *(I-30L, Percival, Geneva Scientific LLC, Fontana Wisconsin, USA)"*
	L 149-150: How was bulk density determined?	We apologize for the oversight of adding the equation to the main manuscript. Action taken: We added equation to the main text, which can also be found in supplementary material. It now reads: *"The soil samples from the small cores were dried at 60 °C to a constant weight. Bulk density was calculated using the equation below (Howard et al., 2014).* $Soil\ bulk\ density\ (g\ cm^{-3}) = Oven\text{–}dry\ sample\ mass\ (g) / Sample\ volume\ (m^3)$"
	L 185-194: It would be nice to see, somewhere in this passage, the %Corg, which is a useful parameter in soil science.	%TOC, with standard error, has been added to this section. It now reads: *"Additionally, the seaward site had a lower Corg concentration, averaging 5.53 mg* $C_{org}\ cm^{-3}$ *(0.34% ± 0.017%) compared to an average of 9.52 mg* $C_{org}\ cm^{-3}$ *(0.72% ± 0.021%) at the landward site throughout the entire sampling period."*
	L 310-311: Why is this sentence underlined?	Thank you for identifying this error, the issue has been fixed.
	L 338-339: Yes, these differences are not fully resolved. But one reason could be different kinds of transport processes inducing CH4 release (ebullitions vas. Diffusive flux). I suggest elaborating this issue a little.	We agree with the comment. This study did not differentiate between diffusive flux and ebullition, but it is true that variable transport processes are an explanation for these differences. Action taken: A few sentences on the kinds of transport processes have been added to the discussion. It now reads: *"The causes for the large differences in GHG flux between sites within the same mangrove stand are not fully resolved, although it is likely that there is microscale variation due, in part, to different gas transport processes. The release of* CH_4 *from the soil via ebullition has particularly high spatial variability within sampling sites*

		(Baulch, 2011; Chuang et al., 2017). Furthermore, the episodic nature of ebullition events may distort the flux calculation which assumes a linear concentration change over time, as is the case with diffusive flux (Jacotot et al., 2018). The possibility of active ebullition in saline, undisturbed mangrove ecosystems requires further investigation, as to-date, no study has found ebullition to be a significant pathway of CH_4 release under these conditions (Cotovicz et al., 2024). Considering this small-scale variability, it is important to emphasise the need for comprehensive assessments in individual mangrove ecosystems as GHG flux is highly site-specific."
	L 366-384: This issue should be better exploited, also in the results section. Which soil parameters influence acetoclastic and hydrogenotrophic methanogenesis? When do mangroves grow most strongly in the Red sea? This should be the time of a higher rate of acetoclastic CH4 production. Does d13C efflux change with season?	To clarify, the measurement of acetoclastic and hydrogenotrophic methanogenesis was not incorporated within our study design, so unfortunately we cannot present this within our results as this would have required additional measurements. E.g. Inhibition of methanogenesis with 2-bromo-ethane sulphonate, measurement of ^{14}C isotopes (Kotsyurbenko et al., 2004), $\delta^{13}C$ of the methyl carbon of acetate (ac-methyl) (Penning et al., 2006), or radiotracer incubations (Weston et al., 2014). We have, however, elaborated on the results of the ^{13}C isotopic signatures by including some additional statistical analysis. The carbon isotope composition of CO_2 and CH_4 did not significantly change between seasons (Kruskal-Wallis test: $\delta^{13}C\text{-}CO_2$, $p = 0.62$; $\delta^{13}C\text{-}CH_4$, $p = 0.66$). There is also no specific season of growth for *A. marina* in the Red Sea. There is considerable interannual and geographic variability, which is largely dependent on temperature and humidity (Almahasheer et al., 2016). As there is no defined growth period, unfortunately, isotopic signatures and fluxes cannot be analysed in conjunction with this. However, to fully address this comment, we have included a Spearman correlation matrix (inserted below) including significance values for all soil parameters, ^{13}C signatures, fluxes, and environmental conditions (uploaded as supplimentary material). The design of this correlation matrix has been included in the 'Data analysis' section of the methods. From this analysis, there were several soil and environmental variables significantly correlated with stable isotope signatures. These variables were; electrical conductivity, core replicate, and inorganic carbon for the top 3cm of soil. Another notable finding of this analysis showed there was no significant statistical relationship using the Spearman correlation coefficient between $\delta^{13}C\text{-}CH_4$ and dark CO_2 flux (inserted below), contrary to the random forest model, which suggested it had the highest predictive power. Our interpretation of this, is that there is a complex relationship between $\delta^{13}C\text{-}CH_4$ and dark CO_2 flux but agree with the reviewer on the importance of methanogenesis and have added to the discussion of this.

		Action taken: Statistical analysis of the $\delta^{13}C$ signature and season has been added to the results, along with the variables significantly correlated with $\delta^{13}C$. The correlation matrix has been added as supplementary material. It now reads: "*The $\delta^{13}C$ signature of CH_4 and CO_2 did not change significantly across seasons. However, significant correlations ($p > 0.05$) were observed between core replicates and inorganic carbon (C_{inorg}-3cm) with $\delta^{13}C$-CO_2, and well as between electrical conductivity ($EC_{1:5}$) and $\delta^{13}C$-CH_4."* Moreover, we have added the following text to the discussion to reflect the non-significant relationship found in the correlation matrix analysis. Further, a few sentences have been added to acknowledge the importance of acetoclastic and hydrogenotrophic methanogenesis and the potential for further study to improve understanding of the CH_4 flux from mangrove soils. It now reads: *"Notably, there was no statistically significant correlation between $\delta^{13}C$-CH_4 and dark CO_2 flux, contrary to the random forest model, which suggested $\delta^{13}C$-CH_4 had the highest predictive power for dark CO_2 flux. This finding may be a result of overfitting from the random forest modeling or there may be more complex non-linear relationships uncovered by machine learning which are not detected by simple correlation."* Moreover, we have added the following subsequent text: *"To better understand the origin and fate of CH4 from mangrove soils, methanogenesis should be studied directly through the determination of δ13C of the methyl group of acetate (Goevert et al., 2009) or isotope mass-balance approach Sánchez-Carrillo et al., 2021)."*
	Figs 2, 3: I have the impression that most high CO2 and CH4 fluxes occur between March and May, in both years. Is this mirrored by the d13C signature of the fluxes? Is this the season of highest plant growth?	There is no consistent month for highest plant growth, although there is generally peak flowering and propagule development in November and January (Almahasheer et al., 2016). The highest growth for *A. marina* between March and May is unlikely as this is when temperatures are increasing, there is little to no rain, and tidal inundation becomes less frequent. It is more likely GHG flux correlates with environmental conditions favoring enhanced microbial metabolism. For example, from the correlation matrix, light CH_4 flux significantly correlated with temperature ($p=0.007$) and water content ($p=0.009$). Dark CH_4 flux correlated with water content ($p=0.043$) and electrical conductivity ($p=0.018$). CO_2 correlated with water volume (light conditions, $p=0.008$; dark conditions, $p=0.032$). The ^{13}C signature of the fluxes do not have a significant relationship with the magnitude of the flux, except between light CH_4 flux and $\delta^{13}C$-CH_4 ($p=0.005$)

		Action taken: The significant correlations with GHG fluxes have been added to the results section 'Drivers of flux variation'. It now reads: *"There were several significant correlations relating to environmental and soil properties with GHG flux. Light CH_4 flux significantly correlated with temperature (p=0.007) and water content (p=0.009), dark CH_4 flux correlated with water content (p=0.043) and electrical conductivity (p=0.018), and CO_2 correlated with water volume (light conditions, p=0.008; dark conditions, p=0.032)."* Additionally, these results have been used to strengthen the 'Drivers of flux variation' section of the discussion, blending this with the discussion on the random forest analysis.
	Figs 5, 6: The predictive power of the year of sampling is interesting. This should be discussed some more.	The growth and flowering cycles of *A. marina* mangroves in the Red Sea are not annual (Almahasheer et al., 2016). Potentially, the growth cycle of the studied mangrove stand may have changed across the multi-year duration of the study, thus giving the year a high predictive power in the random forest modeling, but this cannot be verified. Additionally, water was present during 4 of the 5 months sampled in 2021, whereas other years were dominated by dry sampling conditions, this may be artificially inflating the predictive power of the year. There were also climatic variables and extreme weather patterns for the region, particularly in 2023 which may explain the predictive power of the year (Van Dijk et al., 2023). It now reads: *"In both light and dark models, the year was the second most important predictor for CO_2 flux. The growth and flowering cycles of A. marina mangroves in the Red Sea are not annual (Almahasheer et al., 2016). In theory, increased growth over a given year may result in increased soil carbon pools for microbial respiration, directly impacting GHG flux. However, this cannot be tested as mangrove growth was not measured in the present study. Alternatively, the importance of the year of sampling may be artificially inflated in our models due to the presence of water during 4 of the 5 months sampled in 2021 while subsequent years were dominated by dry sampling conditions. However, there were also climatic variables and extreme weather patterns for the region across the 3-year period. 2023 had widespread greening due to higher-than-average rainfall (Van Dijk et al., 2023), potentially also facilitating mangrove growth. It is likely that a combination of these 3 factors explain the predictive importance of the sampling year, and emphasise the importance of long-term flux measurements to capture variations resulting from climatic changes, and perennial life-cycles."*


Correlation Matrix with P-Value using Function and Spearman
Year
Month
Core
Water (mL)
Light CO₂ (µmol m⁻² d⁻¹)
Dark CO₂ (µmol m⁻² d⁻¹)
Light CH₄ (µmol m⁻² d⁻¹)
Dark CH₄ (µmol m⁻² d⁻¹)
Temperature (°C)
δ¹³C-CO₂ (‰)
δ¹³C-CH₄ (‰)
TN (%)
TOC (%)
TC (%)
TIC (%)
C:N (mol)
Corg-3cm (g C cm⁻³)
Corg-10cm (g C cm⁻³)
Cinorg-3cm (g C cm⁻³)
BD 10cm (g cm⁻³)
WC 10cm (%)
BD 3cm (g cm⁻³)
WC 3cm (%)
Soil volume (cm⁻³)
EC 1:5 (mS/cm)
Sediment ¹³C (‰)
1.00
0.75
0.50
0.25
0.00
−0.25
−0.50
−0.75

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