Answers to the reviewers comments of the manuscript egusphere-2024-1320

Here we show the answers to the reviewers comments of our manuscript (egusphere-2024-1320). We would like to thank both reviewers for the dedication of their time to review our work, we believe that their constructive comments and suggestions have contributed to improve our manuscript. The comments of the reviewers are marked in black, the answer to the comments in blue and the changes that we have made in green. Line numbers refer to the revised manuscript where the changes are not marked

Comments Reviewer #1:

This is a promising study of carbon emissions and soil mineralization potential from non-tidal salt marshes which offers unique data with which to improve understanding of relationships between gas fluxes, plant physiology, and salinity. Marshes in the region are relatively understudied and the non-tidal system is also poorly known in terms of methane dynamics. The measurement of CO2 exchange from woody plant tissues reveals new insights about the capacity for carbon uptake throughout the plant body in the salt marsh species studied here.

Thank you very much for the feedback on the value of the study.

One major finding is that the salt meadow and halophilous scrub had lower soil mineralization potential but higher soil CO2 emissions than the glasswort sward. This result seems counter-intuitive and I would like to see more discussion to potentially explain how longer term carbon could be sequestered despite the shorter term CO2 fluxes from soils.

We agree that, at first glance, this result may seem counter-intuitive, but it is important to remember that the mineralization quotient is calculated as the ratio between carbon emitted and carbon stored as soil organic carbon (SOC) (Pinzari et al., 1999). In a parallel study, we found significantly higher amounts of carbon in the soils of the halophilous scrub and the salt meadow than in the glassword sward, in accordance with a much higher aboveground, belowground and litter biomass in the former two habitats (Carrasco-Barea et al. 2023). Therefore, despite the higher soil CO_2 emissions in the halophilous scrub and the salt meadow, they showed lower mineralization quotients due to their much greater amount of SOC.

We have clarified this in the discussion in lines 404-409 *("Despite the halophilous scrub and the salt meadow had higher soil CO_2 emissions than the glasswort sward, they showed lower mineralization quotients due to a much greater amount of SOC (Table S3), which was in accordance with a much higher aboveground, belowground and litter biomass (Carrasco-Barea et al., 2023). Hence, our results would indicate that soils of the halophilous scrub and the salt meadow would have a higher carbon sequestration potential, despite their higher soil carbon emissions".)*.

Authors meanwhile found higher CO2 uptake from woody tissues of the plants in the former two habitats. I wonder whether this might contribute to or be related to the higher carbon sequestration rates in those soils?

As commented above, soil carbon sequestration capacity is related to soil CO_2 emissions and soil organic carbon content, which is directly linked to the amount of organic matter that arrives to the soil surface, ready to be decomposed and integrated into the soil. The CO_2 fluxes from woody stems measure the net CO_2 exchange between the woody living surface of plants and the atmosphere, and thus are not directly related to what happens at the soil compartment. Nevertheless, we could speculate that higher CO_2 uptake by woody tissues might lead to increased woody tissue biomass, potentially resulting in greater incorporation of more recalcitrant organic matter into the soil. However, this remains speculative, and we have decided not to include it in the discussion.

Another highlight from the study is that relatively large methane emissions were observed despite the salinity of the marshes. Authors partially attribute these high methane fluxes to the influence of low salinity groundwater, which is logical. However, I am concerned that the value of the methane emissions may be over-estimated due to the long duration of chamber closure (24h). Taking only 2 samples (initial and final) over this 24 period limits the precision of the methane fluxes as well. Authors should take care to interpret their emissions in relative terms (comparison between habitats) rather than drawing comparisons with literature, unless they find studies that have employed similarly long chamber deployments.

We agree that using different methodologies is a handicap when comparing results from various studies.

We have included a sentence indicating which of the mentioned studies collected samples after 24h of chamber closure as we did, in lines 453-455 *("although it is worth mentioning that only Hirota et al. (2007) took samples after 24h of chamber closure, as it was performed in the present study").*

The data presentation does need to be improved. Please use different symbols or colors to distinguish the marsh habitats or plant species in all data figures. With the current version (all gray), one cannot discern these groups.

Thank you for this suggestion. You are right.

Following your suggestion, we have improved data presentation using different types of lines to distinguish the different habitats and species in all the figures.

Additional detailed suggestions are attached.

Review for 2024-1320

Abstract

Line 14: Clarify that H. portulacoides and E. atherica are part of the same habitat (similar to line 85)

Done.

Introduction

Consider also methylotrophic methanogens which persist in saline environments.

We have included a sentence about the predominance of methylotrophic methanogens in saline environments in lines 64-67 *("Specifically, aceticlastic and hydrogenotrophic methanogens, with their lower energetic yields, are more susceptible to increasing salinity than methylotrophic methanogens, which explains the predominance of* *methylotrophic methanogens like Methanohalophilus spp. in hypersaline environments (Mcgenity and Sorokin, 2018).”*

Line 68- How extensive are non-tidal marshes in the Mediterranean? Elsewhere?

This information has been added to the text in lines 72-74 *(“Hence, considering the extensive coverage of non-tidal salt marshes in the Mediterranean Basin, which has been estimated in approximately 19 million hectares (around 2.5% of the total area of the 27 Mediterranean countries and 1 to 2% of wetlands in the world; Geijzendorffer et al., 2018)”).*

Line 85-90: Which plant species are C3 vs C4?

It is specified in lines 91-92 that all species are C3 plants.

Line 93: Specify the temperature and salinity ranges typical of this region. Are there any salinity differences in the marsh soils between habitats or seasons?

We have included the lowest and the highest mean temperatures in the text (line 98) and we have added a Figure (supplementary material, Figure S2) showing monthly mean daily temperatures and total rainfall for the previous 10 years and for the study year (2017). Besides, we have included the groundwater salinity in section 2.1 (lines 103-105). Soil salinity differences between habitats and seasons (represented as variations in soil electrical conductivity) are shown in section 3.2.1 (Figure 3).

Section 2.2

How does severing the plant stems and leaves affect the CO2 fluxes? How long were they stored in the refrigerator? (Were any measurements done on live, intact plants in the field?)

All measures of net CO_2 fluxes from vegetation were conducted in the field using intact and attached plant tissues. After measures were performed, we collected the measured plant fractions, and we stored them in a fridge until the sampled leaf area was determined (within the next 24h).

We have clarified this in the text in lines 108-109 *(“Measurements were performed in the field, using attached living green and woody plant tissues”).* We have included the time that samples remained in the refrigerator in line 119.

Line 110-111: How was stomatal conductance measured?

We measured stomatal conductance with the same infrared gas analyser (IRGA) used for net CO_2 exchange measures (CIRAS-II, PPsystems USA).

We have clarified this in the text (line 107).

Line 126-130: Specify make/model of the gas chromatograph- was it using flame ionization detector?

We have specified the model of the gas chromatograph used (Agilent 7890A, Agilent Technologies USA) and that it was connected to a thermal conductivity detector (lines 143-144).

Two methods for soil respiration rates are reported: The soda lime method was used when soils were not flooded. A gas chromatography based method was used when soils were flooded. How was “flooded” defined?
How many measurements were made with each method? (This information will help readers to understand whether flooding frequent or infrequent).

We considered a soil as flooded when it was covered by water. The number of measures performed with each method are detailed in Table S2.

We have included a brief definition of what we considered flooded soils in line 140. Besides, A sentence has been added to inform readers about Table S2 (lines 154-155).

I am not familiar with the soda lime method. Authors should better support their statement that gas chromatography underestimates CO2 fluxes relative to the soda lime method. With only an initial and final time point, over 24 hours, the fluxes are not very precise. They may be affected by artifacts such as accumulation of pressure or altered temperatures, both of which could influence the gas fluxes measured.

We agree that the statement about gas chromatography has not enough support.

Thus, we have decided to remove the sentence “it has been observed that gas chromatography can underestimate CO_2 emission rates by up to 45% in comparison with the soda-lime method (Lou and Zhou, 2006)”. Instead, we have focused on explaining why we chose the soda lime method, rather than gas chromatography, in order to have an integrative measure of soil CO_2 fluxes throughout the whole day (day and night), see lines 151-154 *(“Gas chromatography analyses were not used to estimate soil respiration when the soil was not flooded because temperature and humidity variations throughout the day and night could affect the concentration of gas components in the sample (Rochette and Hutchinson, 2005), not being this a problem by using the soda-lime method, which can integrate soil CO_2 fluxes over long periods, such as 24h (Keith and Wong, 2006)”).* We have also added a sentence clarifying that previous studies have demonstrated that soda lime is a reliable method for estimating soil CO_2 fluxes, see lines 128-129 *(“This method gives a reliable and integrative measurement of soil CO_2 fluxes throughout the whole day (Keith and Wong, 2006)”).*

Results

Figures: Colors or symbols are needed to distinguish the species represented by each line.

We have improved the data presentation by using different types of lines to distinguish habitats and species in the figures.

Figure 4a: Since these are different methods used to measure CO2 fluxes from flooded vs unflooded soils, the study should not make claims about differences in soil respiration between flooded and exposed conditions. Likewise, authors should omit the flooded data points from Figure 4a to avoid direct comparison with the non-flooded data.

We understand your concern, but we believe this comparison is interesting despite the different methodological approaches applied.

For this reason, we have decided to keep these data points in Figure 4a, but, in order to clarify that measurements were performed using two different methods, we have added a sentence in the figure caption and we have highlighted this limitation in the discussion in lines 427-428 *(“However, since different methods were used to measure soil* *respiration in flooded and non-flooded soils, this comparison should be interpreted with caution.")*.

Discussion

Could the higher photosynthetic rates be related to C4 metabolism in E. atherica (in addition to structural difference in stomata?) Which species are C3 vs C4?

As commented before, all the studied species are C3 plants (lines 91-92).

Line 327: Listing the species in consistent order of water use efficiency would be clearer for the reader

Thank you for your suggestion.

We have ordered the species from high to low WUE values (Lines 344-345).

Line 397-399: Avoid direct comparison of flooded and unflooded CO2 fluxes (as mentioned above) due to differences in methods. Authors might rather consider that flooding waters are a known physical barrier to gas exchange.

As commented above, we have highlighted this limitation in the discussion (lines 427-428) and explained the effect that flooding has on the diffusion of CO_2 molecules in lines 426-427 *("A reduction of soil CO_2 emission to the atmosphere during flooding conditions can be explained by the fact that CO_2 molecules diffuse 10000 times slower in water than in air (Kathilankal et al., 2008).")*

Table 1: Which methods were used in the studies on this table? Are they comparable to those in this study?

The most common method used in these studies is gas chromatography for both CH_4 and CO_2 measures, with the infrared gas analyser being also used to determine CO_2 fluxes in one study.

To highlight these methodological differences, we have included this information in Table 1.

Despite the time period in which the chamber was closed was generally shorter than in our study, the study by Hirota et al. (2007) also kept the chamber closed during 24h.

We have added some comments about this to the discussion in lines 420-423 ("*Nevertheless, it should also be noted that the methodology used to determine soil CO_2 and CH_4 fluxes differs from that generally employed in the studies listed in Table 1, since most of them used gas chromatography for both CH_4 and CO_2 measurements. Thus, an effect caused by these methodological differences cannot be excluded*"), in lines 427-428 ("*However, since different methods were used to measure soil respiration in flooded and non-flooded soils, this comparison should be interpreted with caution*.") and in lines 453-455 ("*although it is worth mentioning that only Hirota et al. (2007) took samples after 24h of chamber closure, as it was performed in the present study*.").

Discussion of methane lines 401-414: Most of the methane fluxes were positive, and so authors should not mislead the reader by first discussing negative fluxes (indicating consumption). Similarly, the methane emissions did not differ statistically between habitats. Discussion in this section should therefore focus on what might have been similar between habitats and/or how the general magnitude of fluxes falls within the range reported in other marshes.

Thank you for the comment.

This part of the discussion has been rewritten following your suggestion (lines 429-455).

Line 425-435: This paragraph about salinity relationships to methane emissions is useful for readers to place this study site and its findings in context. This information about the site salinity should be incorporated into the methods/ site description.

We have included this information within section 2.1 (lines 103-105).

Conclusions

Authors should discuss the possible relationship of the high CO2 uptake of woody tissues with the high carbon sequestration potential (as reflected by low mineralization quotients) for the salt meadow and halophylic scrub. Can this help to reconcile the finding of lower soil mineralization quotients despite the high respiration and methane emissions observed?

As we mentioned above, soil carbon sequestration capacity is related to soil CO_2 emissions and soil organic carbon content, while CO_2 fluxes from woody stems measure the net CO_2 exchange between the living woody surfaces of plants and the atmosphere and are therefore not directly related to the soil compartment. Hence, although we could speculate that higher CO_2 uptake by woody tissues might lead to increased woody biomass and potentially more organic matter being incorporated into the soil, this remains purely speculative. Therefore, we have decided not to include this speculation in the discussion.

CO2 emissions may be higher in this study than in other previous studies due to the long period of chamber closure (24h) and associated artifacts discussed above.

The soda-lime method has been proved to be a reliable way to integrate daily soil CO_2 fluxes (as commented above, see for instance Keith and Wong, 2006). Previous studies have also used 24h of chamber closure, as we have now highlighted in the discussion (lines 453-455) (Hirota et al., 2007).

In response to your comments, we have also included several sentences to inform the reader about the different methodologies used in the studies cited throughout the manuscript (lines 420-423 and 427-428).

Comments Reviewer #2:

General comments:

In a world dealing with climate change, there is a need to better understand all ecosystems. Studies like this one, investigating GHG exchange in understudied ecosystems like non-tidal salt marches are relevant and important. The combination of in-situ CO2 – CH4 soil fluxes with CO2 vegetation fluxes in the different habitats results in interesting insights and a valuable addition to the laboratory studies with controlled conditions previously carried out. The study highlights the seasonal variability and the differences between species well.

Thank you for your positive feedback on the value of the study.

The Materials and Methods sections could be more detailed. Many elements are not mentioned here such as soil information, salinity, more specific climate data, the amount of data points taken and details about calculations are also left out. Possible additions and suggestions are mentioned in the attached file.

We have included information about salinity and climatic data in section 2.1. We have also added more specific climate data (Figure S2), and a table showing mean soil SOC, TN and bulk density parameters obtained for the three studied habitats (Table S3). Detailed information about the samplings performed are shown in the Supplementary Material tables, while further explanation about the mineralization quotient calculations has also been added. Lines where this information has been included are specified in the responses to the specific comments (see below).

The Carbon mineralization quotient is not entirely clearly explained for me in the method section and not much explanation is given in the result and conclusion section. I think more explanation is needed around the mineralization quotient calculation and some discussion is needed around the carbon sequestration potential of the habitats as this result is interesting but not well supported.

The mineralization quotient has been more thoroughly explained in the Material and Methods section and commented in detail in the Discussion section. The lines where this information has been included are specified in the responses to the corresponding specific comments (see below).

The authors mention large discrepancies between measurement methods (GC, soda lime) and between in situ and laboratory experiments, therefore this information should be added when comparing to literature. Especially for the soil fluxes it should be mentioned which method and closure time is used in the literature you compare to. In this study, the closure time of the chamber for the GC method is very long, with only 2 data points (before and at the end of the closure time), so this will seriously influence the fluxes.

We have always compared our results with previous studies of soil carbon fluxes conducted under field conditions.

This has been clarified in the caption of Table 1. We have also added information about the methodology used in these previous studies (Table 1), emphasizing the limitations of comparing data obtained by means of different methodologies (lines 420-423 and 427-428). Moreover, we have specified which previous studies used the same chamber closure time (lines 453-455) and we have highlighted the reliability of the soda lime method to estimate integrated soil CO_2 fluxes over long time periods, such as the one used in the present study (lines 128-129 and 151-154).

Data presentation can be improved by the inclusion of a table with soil parameters, a table with the main results, a map of the study region and graph of climate data (either in supplementary or in the main text).

We have included all this information as Supplementary Material.

More comments and also some technical corrections and suggestions for readability are included in the attached document.

Specific comments:

Introduction

Line 68: How extensive are the salt marshes worldwide/in this region and what is the proportion of tital vs non-tidal in this region?

This information has been added to the text in lines 72-74 *("Hence, considering the extensive coverage of non-tidal salt marshes in the Mediterranean Basin, which has been estimated in approximately 19 million hectares (around 2.5% of the total area of the 27 Mediterranean countries and 1 to 2% of wetlands in the world; Geijzendorffer et al., 2018)").*

Materials and Methods

Line 79: Authors could add a map of the region

Following your suggestion, we have added a map of the study zone in the Supplementary Material (Fig. S1).

Line 92: Authors could add some climate data of the region both in numbers and a graph. Annually average rainfall, mean temperatures, … also add some soil data if this is available.

We have included the lowest and the highest mean temperatures in the text (line 98) and we have added a Figure (supplementary material, Figure S2) showing monthly mean daily temperatures and total rainfall for the previous 10 years and for the study year (2017). Besides, we have included the groundwater salinity in section 2.1 (lines 103-105). Soil salinity differences between habitats and seasons (represented as variations in soil electrical conductivity) are shown in section 3.2.1(Figure 3).

How many months is the soil flooded on average? Is this different between the different habitats?

We have included a sentence providing information on the flooding duration of each habitat in lines 101-103 *("The duration of flooding varies among habitats, with the shortest duration in the salt meadow (a few days at most), an intermediate duration in the halophilous scrub (several weeks) and the longest duration in the glasswort sward (ranging from several weeks to several months) (Pascual and Martinoy, 2017)").*

Be sure to add the bulk density, SOC, C and N from the three habitats as these are important in the discussion

We have added a table to the Supplementary Material (Table S3) showing mean SOC, TN and bulk density values for the three studied habitats.

Section 2.2: Is the NER measured in situ with the leaves and tissues attached to the plants?

All measures of net CO_2 fluxes from vegetation were conducted in the field using intact and attached plant tissues.

We have clarified this in the text in lines 108-109 *("Measurements were performed in the field, using living attached green and woody plant tissues").*

How many leaves and tissues were measured per plant and how many plants were measured per species per session and in total?

The number of plants measured per species and the frequency of samplings, considering the time of the day, are detailed in Table S1.

A sentence has been added to inform readers about this Table (lines 116-117).

Section 2.3: I'm don't know the soda lime method very well, but was the amount of soda lime needed for the chambers tested to be sure of complete absorption of the efflux or was this based this on previous literature?

The amount of soda lime required for this study was previously tested by one of the coauthors in earlier works (unpublished data).

I also think the statement starting in line 137 should be more nuanced or better supported, as in the same reference (Lou and Zhou, 2006), there is also indicated "The method (soda lime) tends to overestimate soil CO2 efflux in its low range and underestimate it in its high range compared-with dynamic methods (Yim *et al.* 2002). The technique can potentially underestimate soil surface CO2 effluxes by 10 to 100% (Norman *et al.* 1992, rochette *et al.* 1992, Haynes and Gower 1995, Nay *et al.* 1994)."

The sentence used in this article is based on the sentence from (Lou and Zhou, 2006) "The GC method can potentially underestimate the rate of soil CO2 fluxes in comparison with other methods by up to 45% (Knoepp and Vose 2002)." But if you continue to Knoepp and Vose 2002, it seems to me that the SODA method used in this study is not that good either compared to the other methods and underestimates the CO2 emissions even more than the GC method.

We agree that the statement about gas chromatography has not enough support.

For this reason, we have decided to remove the sentence "it has been observed that gas chromatography can underestimate CO_2 emission rates by up to 45% in comparison with the soda-lime method (Lou and Zhou, 2006)". Instead, we have explained why we chose to use the soda-lime method, rather than gas chromatography, to measure soil CO_2 fluxes when the soil was not flooded, see lines 151-154 *("Gas chromatography analyses were not used to estimate soil respiration when the soil was not flooded because temperature and humidity variations throughout the day and night could affect the concentration of gas components in the sample, not being this a problem by using the soda-lime method, which can integrate soil CO_2 fluxes over long periods, such as 24h (Keith and Wong, 2006)").*

In this study serious underestimation is however a possible issue as the closure time is 24h. This is a long time in which saturation in the headspace can occur. The accumulation of the gas inside the chamber can limit the further emission. However this underestimation is not linked to the GC method but rather to the closure time of the chamber.

Previous studies (see for instance Keith and Wong, 2006) support the reliability of the soda-lime method for measuring soil CO_2 emission after long periods of chamber closure (such as 24h). One study cited in the text also kept the chamber closed for 24h (Hirota et al., 2007), although measurements were not performed using the soda lime methodology.

Mention how many measurements were taken in flooded state and how many in non-flooded state and how this is different for different habitats.

The number of measurements performed in flooded and non-flooded soils for each habitat is detailed in Table S2.

We have added a sentence to let readers know (lines 154-155).

Wf and *Wi* were estimated from volumetric concentration (%) considering the air volume inside the chamber in each sampling date. Is it meant here inclusion of temperature and pressure measurement from the chamber on the sampling date to transform the ppm/ppb measurements from the GC to g CO2/CH4? If yes, which temperature and pressure is used?

No, we did not include temperature and pressure in the calculations. We converted the volumetric concentration (ml CO_2/100 ml air) obtained from the GC results to mg CO_2 using the chamber volume and CO_2 density. We estimated the amount of CO_2 (in millilitres) within the chamber and then we converted these ml to g CO_2 by multiplying by the CO_2 density.

Line146: very small comment but the unit of SMF is g CH4 m-2 d-1 here but later on the unit mg CH4 m-2 d-1 is consistently used.

Thank you.

We changed the units in the CH_4 flux equation to mg CH_4 m^{-2} d^{-1}.

Line 164: The bulk density of soil is never mentioned before, so authors could add the values. Also mention the SOC values used (see also previous comment on line 92).

As previously mentioned, we have added Table S3 in Supplementary Material, which provides information on SOC, TN and bulk density for the same plots where we performed the carbon flux measurements.

I'm not familiar with the carbon mineralization quotient. How were the C_CO2 and C_CH4 values calculated?

The carbon content of CO_2 (C_CO_2) and CH_4 (C_CH_4) was calculated using the atomic and molecular weights of these molecules. For example, to calculate C_CO_2: from X grams of CO_2, we estimated the amount of carbon in these X grams by considering that 44 g of CO_2 have 12 g of C. Thus, we multiplied the grams of CO_2 emitted by 12 and we divided by 44. C_CH4 values were calculated similarly.

How was the transformation from the unit of "g CH4 m-2 d-1" and "g CO2 m-2 d-1" to "mg C g soil-1 d-1"?

The calculation of mg C is explained in the previous response. To convert emissions per unit area to emissions per grams of soil, the volume of soil under the chamber was first estimated by multiplying the area of the chamber by 20 cm, which was the soil depth considered. Then, using the estimated soil volume and bulk density (g soil/cm^{-3}), we calculated the grams of soil under the chamber (i.e. the soil from which the carbon emission measured comes from).

We added information about these calculations in lines 179-185 (*"C_CO_2 and C_CH_4 were calculated multiplying the amount of CO_2 and CH_4 emitted by 12/44 and 12/16, respectively, being 12 the molecular weight of carbon, 44 the molecular weight of CO_2, and 16 the molecular weight of CH_4. SOC values were taken from previous measurements performed in July 2015 and 2016 in the same experiment (Table S3), after observing that these values exhibited stability and remained constant over the studied years (Carrasco-Barea et al., 2023). To convert emissions per unit area to emissions per grams of soil, we estimated the volume of soil beneath the chamber by* *multiplying the chamber area by the considered soil depth (20 cm), and then multiplying this volume by soil bulk density (g soil cm^{-3}).")*

Results

Concerning the correlation between SR and SOC and TN found in July: there is one value of SOC and TN for each habitat, so three in total? Or is there a value for each plot?

To avoid pseudoreplication, we used the mean of SOC and TOC for each habitat (n=5 per habitat), with each habitat having a single representative value.

Discussion

The authors could add a table (maybe in supplementary) with mean/min/max values of water use efficiency, photosynthetic rates, …

Following your suggestion, we have added a table to the Supplementary Material with mean/min/max values of instantaneous net CO_2 exchange rates (NER) of vegetation (Table S4), another with stomatal conductances and intrinsic water use efficiencies (Table S5) and a third one with carbon fluxes and mineralization quotients of non-flooded soils (Table S6).

Section 5.2.2.

The soil C and N content is put forward as possible explanation in line 372 and 377 for the higher SR in HS and SM than in GS, so it think the values of SOC and TN should be added in the paper (maybe in supplementary).

As commented before, we have added a table with the mean values of SOC, TN and bulk density to the Supplementary Material (Table S3).

Line 375-378: Accordingly, a positive correlation was found between July SR and SOC or TN content at the halophilous scrub and the salt meadow of La Pletera salt marsh, since these two habitats had higher content of SOC and TN than the glasswort sward (Carrasco-Barea et al., 2023).

Was there only a positive correlation found between SR and SOC and TN for HS and SW because in the results this is not specified (see my one remark in the section Results above). Also if you have one value of SOC or TN per habitat, then how is a correlation per habitat found, which brings me back to my previous question in Result section?

Maybe this needs to be rephrased, framing that the positive correlation found between July SR and SOC or TN content across the habitats together with the fact that HS and SM have higher SOC an TN underpin the statement that the differences in SR might be related to the soil C and/or N content.

Thank you for your comment, since there was an error in the redaction of these sentence. As you noticed, it seems that we did a correlation per each habitat, when, in fact, we did a correlation considering one value (the mean) per habitat.

We rephrased the sentence in order to clarify this as follows (lines 393-396): *"In our study, a positive correlation was found between July SR and SOC or TN content reinforcing the idea that differences in SR among habitats would be related with the higher SOC and TN content found in the halophilous scrub and the salt meadow compared to the glasswort sward (Table S3)".*

Line 394: can the "occasional tide" be more specific. Are we talking about flooding during several weeks in specific months or also small occasional floods once every week?

We have clarified the meaning of "occasional tide", including the frequency with which this salt marsh is typically flooded and the shortest and longest flood durations across the different habitats in lines 417-418 *("In tidal salt marshes, flooding occurs once or twice every day, while it is occasional at La Pletera (1-2 times per year, remaining the soil flooded from some days in the salt meadow to several weeks or even months in the glasswort sward) (Pascual and Martinoy, 2017).").* This information has also been added to the Materials and Methods section (lines 101-103).

Line 406-413: The way this section is written makes it seem like there is mainly absorption of methane and then some sudden high emission peaks, while it is actually mainly emissions that are measured. Maybe a percentage of negative fluxes to total fluxes can be given.

This part of the discussion has been rewritten. We have now focused the discussion on the seasonal changes and the comparison between habitats and with previous studies performed in other marshes (lines 429-455).

Line 411-413: "In the glasswort sward, peaks in CH4 emissions were observed both when the soil was not flooded (110± 59 mg CH4 m-2 d-1) and when it was flooded (131 ± 45 mg CH4 m-2 d-1), highlighting that methane oxidation in the overlying water column would not be happening."

I'm not sure that emissions during both non flooded and flooded states prove that there is no oxidation in the overlying water column. It states that the methane oxidation is rather limited or actually that the methane oxidation is not a big factor as the net emission is still large.

This part of the discussion was removed following a suggestion from reviewer 1.

Line 439-442: Not much is said about this mineralization quotient. Is there an explanation for the higher sequestration potential of the HS and SM or the lower sequestration potential of GS? I assume that the SOC amount of GS is very low compared to the SOC amount of HS and SM (be sure to put these values in).

As commented above, we have added SOC values to Table S3 and discussed the results in more detail in the discussion, relating the mineralization quotient to the SOC values in lines 404-409 *("Despite the halophilous scrub and the salt meadow had higher soil CO_2 emissions than the glasswort sward, they showed lower mineralization quotients due to a much greater amount of SOC (Table S3), which was in accordance with a much higher aboveground, belowground and litter biomass (Carrasco-Barea et al., 2023). Hence, our results would indicate that soils of the halophilous scrub and the salt meadow would have a higher carbon sequestration potential, despite their higher soil carbon emissions".)*.

Table1. The authors mention large discrepancies between measurement methods and between in situ or laboratory experiments, therefore this information can maybe be added in the table.

We have added to Table 1 the methodology used in each study and we have specified in the table caption that all the studies cited were conducted *in situ*.

I would suggest to move this table to supplementary material and instead incorporate some tables with the data gathered from this study. For readers who want to quickly scan the paper, a table with the mean results of the study would be handy.

We think that Table 1 helps readers to quickly compare present data with data from previous studies and easily locate values discussed in the text. For this reason, we have decided to keep it in the main manuscript.

However, following your suggestion, we have included three tables (Tables S4, S5 and S6) with the main results, although they have been added to the Supplementary Material to avoid redundancy with the Figures in the manuscript.

Technical corrections:

Abstract

Line 19-21: Regarding the studied habitats, the halophilous scrub and the salt meadow showed higher soil CO2 emissions than the glasswort sward, ~~being these values, in general~~, and the overall emissions were higher than those previously reported for tidal salt marshes.

Done.

Introduction

Line 29: ~~in relation~~ compared to the atmospheric concentration

Done (line 31).

Line 30-32: In this context of continuous global warming, ecosystems play an important role in global climate regulation, ~~being thus~~ Therefore, it is essential to determine net emissions of greenhouse gases of ecosystems to estimate their effects on global warming.

Done (lines 31-33).

Line 34: (Laffoley & Grimsditch, 2009) is not present in the references.

We have included this citation in the reference's list.

Line 35-39: ~~Regarding net primary productivity, p~~Previous studies on the photosynthetic capacity of salt marsh halophytic species have mainly focused on the effect of salinity on photosynthetic rates, ~~being~~ and most of these studies ~~mostly~~ were performed under controlled conditions (Davy et al., 2006; Duarte et al., 2014; Kuramoto and Brest, 1979; Nieva et al., 1999; Pearcy and Ustin, 1984; Redondo-Gómez et al., 2007) and less frequently under field conditions (Drake, 1989; Maricle and Maricle, 2018; Warren and Brockelman, 1989).

Done (lines 36-40).

Line 45-47: Photosynthetic rates also depend on abiotic factors, such as light, temperature, flooding regime, salinity or nutrient availability (Drake, 1989; Huckle et al., 2000). ~~being i~~In general it is assumed that the highest plant photosynthetic activity occurs during the hours of the day with the highest solar radiation (midday).

Done (lines 45-48).

Line 55-56: In salt marshes, flooding also has a major effect on CO_2 and CH_4 emissions, since it determines which process, aerobic respiration or anaerobic metabolism, prevails.

Done (lines 56-57).

Line 60-62: Nevertheless, ~~in general~~, generally soil CH_4 emissions are negatively affected by salinity (Bartlett and Harriss, 1993; Livesley and Andrusiak, 2012; Poffenbarger et al., 2011), since in saline environments sulphate-reducing bacteria use to compete with methanogens for energy sources, and consequently disfavor and even inhibit methane production.

Done (lines 61-64).

Line 65-68: ~~However, d~~Despite the importance ~~that~~ of soil carbon fluxes ~~can potentially have~~ in climate regulation, few studies have characterized these fluxes in Mediterranean salt marshes (Wang, 2018), and, to our knowledge, not one study has been performed in non-tidal salt marshes (tides range from 0.1 to 1 m, in contrast to 1-10 m of tidal salt marshes) of the Mediterranean Basin.

Done (69-72).

Materials and Methods

Line 79-80: The study was performed at La Pletera, a coastal Mediterranean non-tidal salt marsh located in the north of the river mouth of the Ter ~~mouth~~ in the municipality of Torroella de Montgrí.

Changed to "… in the north of the mouth of the Ter river …" (lines 84-85).

Line 86-87: being all these species C3 -> , all these species being C3

Done (lines 91-92).

Line 136: the air volume inside the chamber ~~in~~ on each sampling date.

Done (lines 149-150).

Line 151: air volume inside the chamber ~~in~~ on each sampling date.

Done (line 167).

Line 157 : stored in the soil (SOC) ~~for~~ at/above a certain depth

Done (lines 173-174).

Results

Line 203: Differences among species in instantaneous ~~net CO2 exchange rates (~~NER~~)~~ from green tissues

Done (line 222).

Line 205: after sunrise ~~(~~with no significant differences with *S. fruticosa* in April ~~(~~Fig. 1a),

Done (lines 223-224).

Line 232: During most of the year, *E. atherica* showed the highest values of ~~stomatal conductance (~~gs~~)~~ at midday

Done (line 151).

Line 272: ~~Daily soil respiration (~~SR~~)~~ for non-flooded soils of the three

Done (line 291).

Line 273-274: ~~On the contrary,~~ CO_2 emissions were remarkably lower when soils were flooded.

Done (line 292).

Line 276-277: Remarkably high peaks of soil CH_4 emissions were recorded in the three habitats, ~~despite~~ but also negative values, ~~(~~indicating net CH_4 consumption~~)~~ were ~~also~~ observed (Fig. 4b).

Done (lines 295-296).

Line 277-279: In the halophilous scrub, soil CH_4 emissions were detected in April, June ~~(with high values)~~ and September, ~~with~~ and the highest CH_4 absorption ~~being~~ was observed in February, when the soil was flooded.

Done (lines 296-297).

Line 279-281: Maximum soil CH_4 emissions for the salt meadow were recorded in July for the salt meadow, and for the glasswort sward in March or June in flooded and non-flooded soils, respectively~~, for the glasswort sward~~.

Done (lines 298-299).

Line 285: SR and ~~daily soil methane fluxes (~~SMF~~)~~ were

Done (line 303).

Discussion

Line 316-319: The same ~~occurred with~~ was true for *E. atherica* and *S.patula.* ~~being~~ their maximum photosynthetic rates at La Pletera (29.1 ± 2.4 and 20.8 ± 2.9 μmol CO_2 m-2 s-1, respectively) were higher than those previously reported for *E. atherica* (18 μmol CO_2 m-2 s-1, Rozema & Diggelen 1991) and for the annual species *Salicornia ramosissima* (14 μmol CO_2 m-2 s-1, Pérez-Romero et al. 2018) grown under controlled conditions.

Done (lines 335-338).

Line 322-324: ~~Therefore, s~~Studies reporting photosynthetic rates of dominant salt marsh plant species under field conditions are scarce, and the values obtained often diverge substantially from those recorded under controlled conditions.

Done (lines 341-343).

Line 335-338: Interestingly, photosynthetic rates of the studied species at La Pletera were much lower in autumn than in spring, despite the environmental parameters, such as temperature and soil moisture, ~~were~~ also being favorable ~~to~~ for photosynthesis in autumn, ~~(~~especially in October, where maximum temperature was 21°C and soil VWC was even higher than in March and April~~;.~~(Pascual, 2022). A possible explanation might be related ~~with~~ to the high accumulation of ions and soluble carbohydrates ~~that~~ in these species ~~would present~~ after a salt stress period, ~~such as the one occurring~~ which occurs in the Mediterranean salt marshes during summer.

Done (lines 354-359).

Line 346-348: especially in March and May and before sunset, with values of photosynthesis reaching 12 µmol CO2 m-2 s-1~~,~~. These values are in agreement with data reported for Californian evergreen species (Saveyn et al., 2010) or for savannah shrubs and trees (Cernusak et al., 2006; Levy and Jarvis, 1998).

Done (lines 365-367).

Line 357-358: Regarding night respiration rates, the highest values for the four species were recorded in summer (August) and/or autumn (November), ~~being especially elevated~~ with those found for the green tissues of *S. fruticosa* and *E. atherica* during these months being especially elevated.

Done (lines 375-377).

Line 362: references are large

We have reduced the font size.

Line 364-365: In November, respiration rates were also very high despite that the minimum temperature was much colder (4.6 °C) than in August (22.2 ºC) and similar to February (5.9°C) (Pascual, 2022).

Done (lines 382-383).

Line 384-386: On the contrary, the sparse vegetation, (which is only alive during few months) of the glasswort sward and the poorly developed root system of its dominant species, *S. patula*, would make ~~negligible~~ the contribution of roots to soil respiration in this habitat negligible.

Done (lines 402-404).

Line 437: This would not be the case ~~of~~ in the salt meadow, the most distant habitat from the sea,

Done (lines 450-451).