Reply to Comment on bg-2021-60

Referee #1

The manuscript by Escolano-Moltó et al. presents a synthesis of seagrass metabolic data from previously published work and/or datasets in the Mediterranean relative to two seagrass species (*Posidonia oceanica* and *Cymodocea nodosa*) using two methodologies (benthic chambers and multiparametric sensors). This is a very relevant topic in the current context of climate change in relation to carbon sequestration in coastal areas, and the work presented has a considerable amount of data and results that fit within the scope of Biogeosciences. While the seagrass metabolic data is not particularly novel, the comparisons among methods, species, and regions (Mediterranean basin) are very important. However, there is a major flaw in the statistical approach used and how this is used to pooling datasets. As presented in the manuscript, the ANOVA analysis is not considering the lack of independence in the data from the same season, site, or region and should be reviewed. Depth should also be considered as a covariate as it is most likely related to the metabolic rates due to the light availability. Increasing the accuracy in the statistics presented is essential for the interpretation of the results presented here, especially because datasets are pooled based on those analyses and then further analyses are done. Therefore, the results presented are built over potentially incorrect statistical analysis, and, right now, it is not possible to evaluate the accuracy of the entire set of results presented. If ANOVA assumptions cannot be met, consider using a different statistical approach (e.g. mixed models) and present the results accordingly. Especially critical is the pooling of datasets, if possible, this should be avoided and instead, grouping factors or separate analysis should be considered. Additionally, the main text structure needs revision (see specific comments below). In particular, there is a lot of information on the methods section that is missing in the Results (e.g. habitat traits measured, logistic regressions between abiotic and biotic parameters, pH data). Also, there are Results (including stats) presented in the Discussion section. Throughout the text, there are several typos and constant misuse of species names, which appear sometimes complete and others shortened, and many times italics are not used. I believe the work presents interesting data, and so, the analyses could be revised to improve the way results are presented and discussed in the manuscript. Hopefully, my suggestions help to improve the manuscript. All my comments are made with this purpose.

Before detailing our replies to the reviewer I would like to indicate that through the revision of our data for the revised manuscript we have made the hard decision to exclude sensor data with positive oxygen signals during the night time. As we already mentioned in methods, results and discussion sections in the last version, the sensor data has the disadvantage of picking up oxygen

concentrations from water volumes drifting past by lateral advection. A positive signal during the night time is a clear indication of this problem and thus we have excluded data where we suspected a big influence of lateral advection. Currents are not usually intense in the Mediterranean and in our opinion this is not a common problem with the dataset and does not invalidate sensor results. However, we have wanted to be on the cautious side and only present data we absolutely confide in. Therefore the database has decreased somewhat in size and this has also meant all statistics and figures have been re-done and some results have changed. We apologize for this, and also the delay it has caused in our revision. The extensive changes made in the manuscript have also caused us to reconsider the order of authors as you may have noticed. However we think the extensive reworking of the manuscript has vastly improved the quality of the analyses and the conclusions are much more robust.

Reply: We thank the referee for the helpful comments, we have restructured the text as suggested, and taken all the specific comments into account. We understand the concerns about the ANOVA analyses and have redone the analyses using mixed models (package lme in R) and included depth as a factor. See replies to the specific comments below.

Abstract:

L14. I would recommend replacing ": "Through their metabolic activity, they ..." with "Seagrasses". As it is written now, the statement neglects the fact that carbon stored in sediments can come from external sources and that the buffer of low pH can also occur due to other processes not related to the seagrass aerobic metabolism.

Reply: Thank you for the suggestion, we have modified the text accordingly. The sentence now reads: "Seagrasses can act as carbon sinks; buffer lowering pH values during the day and store carbon in the sediment underneath their meadows."

L15. This is a long sentence that could be re-written to increase clarity. For instance: In this study, we analysed published and own (unpublished?) data on seagrass community metabolism to evaluate trends through time of these two species comparing two methodologies: benthic chambers and multiparametric sensors.

Reply: Thank you for the suggestion. The modification has been included in the manuscript. The sentence now reads: "In this study, we analysed published and previously unpublished own data on seagrass community

metabolism to evaluate trends through time of these two species comparing two methodologies: benthic chambers and multiparametric sensors."

L19. remove "with no significant results despite the clear visual trends."

Reply: Modified in the text.

L21. Add a comma before whereas

Reply: Added to the text.

L23. add "the" before highest or replace by higher

Reply: Added to the text.

L23 - L24. write the complete species name in italics and remove the genus (i.e. P.oceanica, C. nodosa)

Reply: This was modified in the text.

Introduction

General comment: The introduction is long, there is a lot of information and it is difficult to follow the flow of ideas. This is especially the case around the importance of seagrass aerobic metabolism related to (1) carbon burial in sediments and (2) buffering of low pH. Both processes are related to primary productivity, however, there are differences among them that right now are unclear in the text. I would recommend reviewing the text, try to shorten it, and present idea by idea avoiding redundancy and unnecessary information. The first paragraph in particular is hard to read and it is very long (L30 to L84). See detailed comments below:

Reply: We have shortened and modified the introduction as suggested, and hope the first paragraph is easier to read now.

L30. Please consider rewriting this sentence to increase the accuracy of the statement. For instance: Organic carbon buried in sediments underneath marine vegetation.

Reply: Thank you for your suggestion. The sentence has been modified in order to improve the accuracy in the final manuscript. The first sentences of the first paragraph now read: "A fifth of the global carbon sequestration in marine sediments (Duarte et al., 2004; Kennedy et al., 2010) can be attributed to seagrass meadows, despite the fact that they cover only a 0.1% of the ocean surface. This "blue carbon", which is defined as organic carbon buried in sediments underneath marine vegetation (Duarte et al., 2004; Kennedy et al., 2010; Mcleod et al., 2011; Greiner et al., 2013) is the result of the combination of intense metabolic activity of the vegetation, high trapping capacity of allochthonous matter and an effective carbon preservation in sediments underneath meadows (Cebrian, 1999).."

L33. remove dot before the references.

Reply: Removed.

L34. add "an" before intense.

Reply: Added to the text.

L34. Remove "together with excess production". I believe the authors meant high productivity rates, but the word excess is a subjective assessment that can lead to confusion

Reply: Thank you for the suggestion, part of the sentence has been removed in the text.

L34. Remove "in seagrass meadows" because it is obvious

Reply: Removed in the text.

L35. Increased compare to what? Consider replacing “increased” by “high”

Reply: Thank you for the suggestion, “increased” changed by “high” in the text.

L35-L40. This statement is redundant with the one before (“high trapping capacity of allochthonous matter in seagrass meadows”.

Reply: We have clarified the sentence removing the redundancy, see the revised first paragraph above.

L40. Consider removing: “elements such as”

Reply: Thank you for the suggestion, “elements such as” has been removed in the text.

L39. this last sentence hangs alone in the text and it is difficult to understand what it refers to. Please review: “together with in situ production due to their primary production (Greiner et al., 2013).”

Reply: Thank you for the remark, we have modified it in the text.

L43. The species names should always be in italics

Reply: We apologize for the format error. Format changed in the text.

L50. Unclear what it means “consistent estimates”. Does it refer to methodology?

Reply: Indeed, we referred to methodologies. The statement has been modified in the text for clarity.

L56. Consider replacing “human processes” with “human activities”.

Reply: Thank you for the suggestion. The recommended change has been added to the text.

L56. I believe this refers to the dynamics of the carbonate system but needs clarification.

Reply: Clarification added to the text.

L60. Two dots in a row, remove one.

Reply: We apologize for the format error. Dot removed in the text.

L85. Consider replacing “which are located in” to “from”, as *C. nodosa* can also be found outside the Mediterranean.

Reply: Thank you for the suggestion. Change added to the text.

L96. Consider replacing “as ranging” to “to range”

Reply: Replacement added to the text.

L102. Add space between “Mediterranean meadows”

Reply: Space added to the text.

L124. Consider replacing “by the use of” to “using”.

Reply: Change added to the text.

L132. Consider replacing “large” with “larger”

Reply: Word replaced in the text.

L136. Remove “the” before “two” as there are more seagrass species in the Mediterranean.

Reply: Thank you for the suggestion. “the” removed in the text.

L139 Remove “including the two species in the Mediterranean Sea”.

Reply: Removed from the text.

Methods

General comment: In the abstract, it says that part of the data analysed in the study is its own data. But in the methods, it states that data is from published literature or published datasets. Does it mean the “own data” comes from previously published work? Is there any data collected in the field for the purpose of this study? All this needs clarification. Based on the information in the abstract I was hoping to see an assessment of how seagrass metabolism has changed through the years (authors have data since 1982) as a function of changes in the CO2 atmospheric concentrations "In this study we analyse the metabolism synthesized from published data on seagrass community metabolism and from own results to evaluate trends through time". If possible, it would be really interesting to include this.

Reply: We thank the reviewer for pointing this out. By “own data” the authors referred to all the data collected by the IMEDEA Global Change department (some of the data was published and some is unpublished). We have clarified the text, as the wording was confusing. We acknowledge that this might not have been fully clear in the text as the wording was confusing. This study brings in 3 unpublished data sets, 1 from Mallorca (W Med) but more importantly 2 from the Eastern basin, from Crete and Cyprus, and therefore expands the current knowledge of metabolic rates in the Eastern basin considerably. We did analyse the data for trends over time for changes in metabolism, but we did not find any significant results for the productivity (GPP, NCP) data collected with sensors. This could be due to the fact sensors are picking up a highly “composed” signal, as water column mixing makes it difficult to attribute measured metabolism to a single habitat. We did, however, find a difference over time (Year) for respiration (CR) for sensor data, with increasing values over the years (Figure A6). We also found a trend (not significant) over time for CR and GPP for the benthic chambers with the new

analyses, but not NCP, both CR as well as GPP decreasing, which is in contrast with the sensor data for CR and in contrast with expectations. As the dataset includes different methodologies, regions, highly variable sites and measurements done mostly in summer, it was difficult to get robust results for an unbalanced design, specifically evaluating the effect of season. Although theoretically there are seasonal trends, our results did not shown these trends due to the bias of the data set with more data available during summer compared to other seasons.

The paragraph now reads: “All data for benthic chamber deployments was extracted from the literature (published or submitted), while part of the sensor data for the metabolic parameters was extracted from the literature (published or submitted) while another part was obtained from unpublished data in the Western- but also more importantly Eastern Mediterranean Basin (Crete, Cyprus; Table 1). Data available as oxygen concentration over time was processed and analysed to obtain the metabolic parameters, when this was not available we used reported values for metabolism.”

L146. Site description: The way is written suggests that field data was specifically collected for this study (see comment above). If this is not the case, consider re-writing this part avoiding the use of terms like “sampling campaigns” or “sampling sites” and/or specifying that all this information comes from previous work. Furthermore, there is a high level of detail on the site description that (in my opinion) is unnecessary for a scientific paper. In case it is necessary for discussion, consider moving that info (such as the different status of protection of each site: SPA, Birds directive, ZEPA, LIC, ZEPIM, etc.) to the discussion section.

Reply: Thank you for the suggestions. The terms “sampling campaigns”/” sampling sites” have been replaced in the main text, except for the locations that were specifically collected for this study. We think the details of the site description are useful to have an environmental context about the sites where the data was specifically collected as this information is not available by referring to published literature. However, we agree the description of the other sites is too detailed and have re-arrange the section to highlight the information of the “new” sites and put them in the context of the type of existing sample locations.

As the manuscript text has changed so substantially we do not highlight a specific paragraph in this reply, but refer to the new methodological section.

L156. Add space after “Souda,”

Reply: Space added in the text.

Fig 1. Add north arrow and latitude and longitude degrees in the axes. Missing reference for GEBCO 2020 in the reference section.

Reply: We have completely changed Figure 1 and made a new figure in Matlab with the location of the sample sites and latitude, longitude, north arrow and depth isobars.

L188. Add “traits” after “habitat”

Reply: Added to the text.

L183. Data analysis: Please add the accuracy (± SD) of the multiparametric sensors for each of the parameters used, especially for DO and pH. This is crucial for further interpretation.

Reply: We apologise for the lack of information in the text and included the accuracy of each sensor.

L187 - L189. Need to add methods for the habitat data.

Reply: we followed the procedure described in Hendriks et al. 2014 and added this information to the text.

Hendriks, I. E., Olsen, Y. S., Ramajo, L., Basso, L., Steckbauer, A., Moore, T. S., Duarte, C. M. (2014). Photosynthetic activity buffers ocean acidification in seagrass meadows. *Biogeosciences, 11*(2), 333-346. doi:10.5194/bg-11-333-2014

Table 1. Not sure what is the date format required by Biogeosciences but consider using MM/DD/YYYY.

Reply: Thank you for the concern, we have checked the date format required by Biogeosciences and it is DD/MM/YYYY. Therefore we have left the format as it is.

L211. Salinity is unitless. Remove units here and in Table 1

Reply: Thank you for the remark, salinity unites were removed from the text and in Table 1.

L223-L225. In the k and k660 calculations, what is the effect of the higher salinity found on each of the sites?

Reply: We appreciate your concern. In this study K and k600 calculations were chosen from the work published by Kihm and Körtzinger in 2010 and by Cole and Caraco in 1998, as they were the most suitable for coastal areas. These authors did not reflect on the effect of high salinities, specifying that the stronger dependence in the parameterizations is caused by elevated wind speeds, which is not our case. However, we do believe this is an aspect that should be included in future specific studies of the air-sea gas transference in high salinity areas.

L277. How were the 12 publications selected? Is this the total number of published works for P.oceanica and C.nodosa in the Mediterranean? If not, it will really help to include more data from seasons and regions understudied (for instance: studies with spring, fall, or winter data from the Eastern basin).

Reply: The 12 publications were selected after a thorough search and to the best of our knowledge they reflect the total number of published works with metabolic data for *P. oceanica* and *C. nodosa* in the Mediterranean Sea. We would greatly appreciate receiving information on additional studies if the reviewer noticed they´re not included at present.

This paragraph now reads: “Data for the metabolic parameters was extracted from the literature, through a literature search on SCOPUS and the Web of Science using the keywords “Posidonia”, OR “Cymodocea”, OR “Seagrass”, AND “Productivity”, OR “Metabolism” and manually screened for data on metabolism in the Mediterranean basin. This database was extended with submitted data and data from dedicated sampling campaigns in 2016 in Mallorca (Western Mediterranean) and 2017 in the Eastern basin (Crete and Cyprus, see Table 1, Fig. 1). We also compiled data from multiparametric sensors collected during different periods ranging from 2011 to 2019 (for details see Table 1). While data using the benthic chambers methodology had a higher number of literature studies, with a total of 12 publications with data for *P. oceanica* and/or *C. nodosa* meadows (for details see Table 2), and a wider temporal cover with studies carried out from 1982 to 2019. Importantly, this study adds new data on Mediterranean seagrasses metabolism in the Eastern Mediterranean Basin (Crete, Cyprus; Table 1), where little data has been

published before. Data available as oxygen concentration over time was processed and analysed to obtain the metabolic parameters, when this was not available, we used the reported metabolic rates."

L278. Add space before 12

Reply: Space added in the text.

L281. "In this work we add benthic chambers data to the body of literature," suggests that field data was collected, but no other explanation is given. See the comments above about clarifying this.

Reply: We agree this sentence was confusing, we added more details on the benthic chamber's methodology to the text. In fact, no unpublished data was used for the benthic chambers, only published literature, either from the IMEDEA Global Change group or outside.

L282-L285. I believe this sentence corresponds to a data analysis section, not to data compilation. Please add information on how the ANOVA assumptions were tested, especially the lack of independence from the time series data and data from the same site/season/region when comparing metabolic rates. Was any random factor considered? If not, the statistical analysis for the comparison of metabolic rates should be reviewed. For all statistical analyses done, please add information on how the residuals looked and if those met the assumptions of the correspondent analysis.

Reply: We moved the sentence to the Data analysis section. Furthermore, we revised all statistical analyses and used a more appropriate design as suggested. We used mixed models, through the lme4 package in R with random factors. For instance when we evaluate the difference between species for the sensor data we used "Sites" as a random factor as some sites had data for 1 species and some for both. We could not use mixed models with random factor for all the data due to unbalanced number of measurements and therefore used general linear models instead when not assigning random factors. We have added more information on the statistic outcome to the text as well (t values, degrees of freedom).

The phrase in the data analysis section now reads: "We used mixed linear models with package lme4 in the R environment (R core team, 2021) to evaluate differences between methods, regions and species. To reflect the variability between study approaches and sampling procedures and therefore

variability in the precision of outcome of each study, we used a linear model where publication was included as random effect unless specified differently. We also analysed abiotic (wind, pH, depth) parameters related to sensor data as there was more additional data associated to these measurements. As the data was not normally distributed according to the Shapiro-Wilk test, we log transformed data for GPP, and CR before analysis. NCP could not be log transformed due to negative values."

L284. Are density and shoots the same measurement? How were all these parameters measured? See the comment above about the need to add methods for the habitat data.

Reply: Thank you for the remark, the notation has been erroneous and, in fact, it should have been "shoot density". As all benthic chamber data comes from published data, the details for biotic parameters were extracted from the papers as well. In this revised manuscript we have left out this analysis on the base that we have too little data to evaluate the effect of this parameter properly. Furthermore, the manuscript contains a lot of information already and including this analysis does not add enough new information while it distracts from the main message.

Table 2. Two decimals are enough for temperature, salinity, and depth. Also, remove units in salinity. Consider adding here or in the text the characteristics of the chambers (i.e. flexibility and material).

Reply: Thank you for the remark. Superfluous decimals and salinity units were removed from the text. As the benthic chamber data is published, we have added some sentences on the general construction of benthic chambers and referred for specifics to the respective papers.

Results:

General comment: There are methods written in the Results section. It would be better to move that to the methods section. I have serious doubts about the use of non-significant results in one-way ANOVAs to pooling datasets in data that is (for what I can see in the methods section) not independent. The results on habitat traits and abiotic parameters used (pH for instance) and many of the logistic regressions (temperature, shoot density, etc.) are missing and should

be added. Finally, I would suggest, in order to gain clarity, to summarize section 3.1 in a Table and keep consistency on the use of written numbers.

Reply: we agree summarizing section 3.1 improves the readability of the paper and have moved that information to Table A2 in the appendixes. We apologize for not including the linear regression mentioned in the earlier version but we believe that as this information was not significant, it was therefore with little relevance for the paper and would only add confusion. In this revised version we have therefore decided not to include this analysis as we think we do not have enough data for a good assessment. We have concentrated on time and temperature, and have included this figure in the appendix (Figure A4). The reason to exclude pH is also the fact that due to the metabolic activity of the plants this parameter was highly variable during the measurements (day-night) and the correlation with an average value would not really contribute information on the studied processes.

The results section with the revised statistics and justification for pooling the species-specific data for the sensor part now reads: "Sensor data were collected in the water column, with lateral movement between habitats of water masses, and there were no significant differences, in GPP ($t_{df=31.75}$=-0.16, p=0.87), CR ($t_{df=32.46}$=0.91 p=0.37) and NCP ($t_{df=32.30}$=0.21, p=0.84), between the two species (*P. oceanica* and *C. nodosa*), tested in a mixed model with "Site" as random factor, including depth, region and seasons. Therefore, we didn´t divide the sensor data for the two species."

We have also corrected the abstract and discussion to reflect for instance the fact that, due to using "Site" as random factor in the region analysis, with the high variability, there are no regional differences observed for metabolic rates.

L295-L298. All this info can be removed or moved to the Methods section. If the data is available, please add the correspondent link.

Reply: Information removed and summarized in table A2, in the appendixes. The final database will be available through the repository with the correspondent link upon acceptance of the paper.

L310. In the stats analysis, please provide more details: degrees of freedom, F-values, Sum or Mean of Squares for ANOVA, etc. This information can go in a Table into supplementary materials.

Reply: We have revised the statistics, and updated the results section including t-values (for linear regression models) and χ^2-values (for mixed

models) with accompanying degrees of freedom. We think providing an additional table in the appendix might be confusing as there are many analyses and thus there would have to be several tables or composed tables. We are willing to include these though if the reviewer thinks this would improve the clarity of the paper.

L310. See my general comment above about merging datasets based on simple one-way ANOVAs.

Reply: We agree simple ANOVAs are maybe not the best way to analyse the data. However, we do think that merging some data, is justified. For instance, in the case of the sensor data from the two species. Even though the underlying idea was to capture species-specific metabolic rates, In practice this has proven to be extremely difficult due to lateral movement of water masses. Even in large sandy areas in Posidonia meadows the metabolic signal of the meadow is noticeable (data not shown, personal experience of the authors) and it is difficult to separate the components (species specific productivity) contributing to the ecosystem productivity measured in the water column. So, in this case merging this data has a biologically sound reason, backed up by the statistical test. We did, however, revise the statistics as we do agree the previous analyses were too simple. Nested ANOVAs or ANCOVAs as well as mixed models are far more appropriate. We have decided to use mixed models to be able to include random factors.

L321. See my general comment above about merging datasets based on simple one-way ANOVAs.

Reply: see comments above

L328. Replace “didn´t” by “did not”.

Reply: Replaced in the text.

L330-L333. If possible, I would suggest moving the methods and results related to temperature from the appendix to the main manuscript. The finding of temperature not affecting metabolic parameters in the Western basin is very relevant to the work done and is very interesting.

Reply: We agree with the reviewer that the lack of correlation between metabolic parameters for the benthic chamber data and temperature is interesting and definitely unexpected for us. However, we fear this is due to the unbalance of the data over the seasons, with a range of temperatures within different seasons and their corresponding biological activities of the seagrass.

With the revised database we have found relationships with temperature for sensor data, however opposite as expected, with increasing NCP and decreasing CR. Again this could be due to the fact this signal is composed, with many organisms contributing to the measured signal. Also, as for benthic chamber data, the unbalance between seasonal data, with plants in a different growth stage could have influenced the results.

L329. Remove capital letter from “Addition”

Reply: Capital letter removed in the text.

L331. Replace “none” with “any”

Reply: Replaced in the text.

L346. I would suggest removing “and act as carbon sinks” as this was not studied.

Reply: Removed from the text.

L365. Replace “didn´t” by “did not”

Reply: Replaced in the text.

L369. “Except for the summer” hangs alone and it is difficult to know what it means.

Reply: Thank you for the remark, removed in the text.

L373. See my general comment above about merging datasets based on simple one-way ANOVAs.

Reply: as commented above

L375. Keep consistency on the number of decimals used for each parameter.

Reply: Corrected in the text.

Discussion

General comment: There are results (I believe from the logistic regressions) written in the Discussion section that should be moved to the Results. Also, it

would help the readers to have a first paragraph on the discussion with the take-home message**.**

Reply: We appreciate your comment. Some of the results presented in the discussion section have been added in the results section. In addition, a first paragraph in the discussion with the take-home message have been added.

L413. Replace “didn´t” by “did not”

Reply: Replaced in the text.

L417. This statement about the 10m distance among seagrass meadows is very confusing. From Table 1, only two sites presented both species. Please clarify what do you mean here.

Reply: Thank you for the remark, clarification added in the text.

L423. Replace “didn´t” by “did not”

Reply: Replaced in the text.

L430. I would suggest removing “and act as carbon sinks”.

Reply: Removed in the text.

L432. Keep consistency in the use of acronyms.

Reply: We appreciate your comment and revised all the acronyms in the text.

L439. These results are not presented anywhere.

Reply: Thank you for pointing this out. Now pertinent results have been included in the Results section.

L440 – L447. These results need to be presented in the Results section

Reply: We appreciate your comment. These results have been included in the Results section.

L441. Add space after comma, and remove dot before comma

Reply: Thank you for the remark, corrected in the text.

L446. The results of the biotic parameters related to metabolism are really surprising and it would be interesting to discuss them further.

Reply: As mentioned in the text, biotic parameters like shoot density and biomass were not determinant for GPP, CR nor NCP ($p>0.1$), which underlines the effect of lateral advection and mixing of water masses influencing the net signal measured by the multiparametric probes. Also, we firmly believe that we lack sufficient data to provide a solid estimate. Therefore we have not included this analysis in the current version of the manuscript. However, we appreciate your comment and believe that this should be included in future studies with more available biotic data.

L454 Replace “wasn´t” by “was not”

Reply: Replaced in the text.

L455. See my comment in methods about the bibliographic research. Does this mean that no benthic chambers have ever been used in C. nodosa in the Eastern basin? If this is the case, the results presented in this work are even more important and this should be highlighted as one of the outcomes.

Reply: We found data on benthic chambers for *Posidonia oceanica* used in the Eastern basin in the publication by Apostolaki et al., 2010, we included the reference below. On the other hand, we did not find published data in the Eastern basin with sensors neither for *P.oceanica* or *C.nodosa.* We have updated the paragraph on the bibliographic search and clarified the method section.

Apostolaki, E. T., Holmer, M., Marbà, N., & Karakassis, I. (2010). Metabolic imbalance in coastal vegetated (*Posidonia oceanica*) and unvegetated benthic ecosystems. *Ecosystems, 13*(3), 459-471.

L458. Replace dot by comma

Reply: Replaced in the text.

L459. Avoid repeating results in the discussion section.

Reply: Removed in the text.

L471. Please cite the correspondent literature.

Reply: Clarified in the text.

L486. Remove dot after column

Reply: Dot removed from the text.

L515. Replace “didn´t” by “did not”

Reply: Replaced in the text.

L518. Remove “a” before “more”.

Reply: Removed from the text.

L544. Remove “prevention”

Reply: Removed from the text.

Appendices

Appendix B is really scattered and the results of the higher GPP with depth seem to be driven by only 1 depth (15m). Is this only driven by one site?

Reply: Thank you for the remark. We agree there is a high variability in the data. To clarify, the GPP values at 15m depth are measured at the same site for 11 consecutive days and we considered them relatively robust. The

significant relationship of GPP with depth is not present in the new statistical analysis, based on our sanitised database, which is why we have decided to delete the figure.

Appendix D. remove capital letter from oceanica.

Reply: Thank you for the remark, format changed in the text.

Reply to Comment on bg-2021-60

Referee #2

Comparing the methodology for assessment of GPP using benthic chambers and the potentiometric probes is so apt and need of the hour especially while highlighting the role of marine macrophytes to combat climate change impacts. The aim and objective of the article is genuine and well achieved.

Before detailing our replies to the reviewer I would like to indicate that through the revision of our data for the revised manuscript we have made the hard decision to exclude sensor data with positive oxygen signals during the night time. As we already mentioned in methods, results and discussion sections in the last version, the sensor data has the disadvantage of picking up oxygen concentrations from water volumes drifting past by lateral advection. A positive signal during the night time is a clear indication of this problem and thus we have excluded data where we suspected a big influence of lateral advection. Currents are not usually intense in the Mediterranean and in our opinion this is not a common problem with the dataset and does not invalidate sensor results. However, we have wanted to be on the cautious side and only present data we absolutely confide in. Therefore the database has decreased somewhat in size and this has also meant all statistics and figures have been re-done and some results have changed. We apologize for this, and also the delay it has caused in our revision. The extensive changes made in the manuscript have also caused us to reconsider the order of authors as you may have noticed. However we think the extensive reworking of the manuscript has vastly improved the quality of the analyses and the conclusions are much more robust.

Reply: Thank you very much for your comment. The authors really appreciate it.

Metabolic rates of seagrasses may vary with the temperature, salinity, pH, dissolved oxygen levels etc of ambient water as well as photoperiod and PAR reaching the canopy (depth). If the authors have taken care of these factors prevailed during the long observation period (2000 to 2019 while drawing inference, this preprint assumes more merit of publication in the Biogeo Sciences Journal.

Reply: The authors strongly appreciate your comments. The factors mentioned have been taken in account in this study, although not the same amount of associated data was available for all variables and we have not included analyses for which we judged insufficient data was available.

Except for a few typographical errors (page no 2, line 43 name not in italics, Page 1 line 23 Easter or Eastern basin?) and grammer in a few pages (page 11 line 281 tense), the manuscript has been well constructed with bold presentation of results.

Reply: Thank you for the remarks. In order to improve the quality, corrections have been made in the main text.