EGUsphere, author comment AC1
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Reply on RC1

Gabriel Nuto Nóbrega et al.

Author comment on "Masked diversity and contrasting soil processes in tropical seagrass meadows: the control of environmental settings" by Gabriel Nuto Nóbrega et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-466-AC1, 2022

Dear Editor,

Please find the responses to all questions raised by Dr. Antisari about our manuscript entitled "Masked diversity and contrasting soil processes in tropical seagrass meadows: the control of environmental settings", referenced as egusphere-2022-466.
In this final response, we performed the adjustment and corrections suggested by Dr. Antisari, which considerably improved the quality and rigor of our manuscript. We would like to thank Dr. Livia Vittori Antisari who conducted an incredibly detailed and careful examination, including insightful comments for improving the manuscript.

Reviewer #1. Dr. Livia Vittori Antisari

#1.1. Subaqueous soils profiles (two for each places) have been studied in three different parts of Brasil: a) NE under Tropical wet and dry or savanna climate (Aw) and Hot semiarid climate, seagrass meadows Halodule, and Miocene-Pliocene extensive sedimentary deposit (fine to coarse sand, conglomerate and kaolinite matrix) b) SE Monsoon influenced subtropical climate seagrass meadows Halodule, and granitic/gneiss formation c) Humid subtropical climate, seagrass meadows Rupia and Sandy deposits. The test of paper is very clearly written: the introduction is exhaustive; the design setting is complete of important information to know the three study areas and also the analytical methods both filed, and lab was very well written.

The authors are thankful for the insightful comments on our manuscript. Considering the length of the introduction, we genuinely believe that a four-paragraph introduction is adequate and concise, whereas a more extended introduction may lead to exhaustion.

However, as suggested, some sentences from the introduction section were rewritten, and some redundant references were removed to avoid exhaustion. Besides, we believe that further introduction shortening may compromise the readability of the text.

#1.2. Generally, I think that the description of horizons sequence according to

McVey et al. (2012), and, consequently, the USDA classification were better than FAO description, used in this paper. I'm going to accept it yet.

We kindly disagree with the Reviewer, as she considers that USDA classification would be better than FAO methods. As we look for a broader impact of our study, we decided to use FAO guidelines for morphological description and classification to stimulate international description protocols for subaqueous soils. Additionally, despite the protocol for soil description, the results and interpretations in the study would be the same.

#1.3. In particular in field analysis is very important to determine the presence of monosulfides were observed though the color response of the matrix after adding some drops of 3% H2O2 and by recording the odor description of each soil horizon (Fanning and Fanning, 1989, Fanning et al., 2002). Furthermore, I suggest that pseudototal elements (i.e. S, Ca and K, besides of course Fe) could be analysed; S is important to understand the dynamic processes with organic C.

We are thankful for the detailed comments performed by Dr Livia. Unfortunately, despite the prominent sulfide odour, its presence was not correctly described. On the other hand, AVS fraction was quantified, and since the results were very low and not important for characterizing pedogenesis, the authors decided not to include it in this study, but include it in a further study regarding the characterization of microbial communities and Fe-S-C dynamics.

Additionally, we believe that total S content may improve the comprehension of the sulfidization process, which influences C dynamics. However, it was impossible to quantify total S due to technical limitations.

In our study, the comprehension of the sulfidization process was assessed by the sulfate concentration in the water column and the content of pyrite (e.g., Fe-pyrite), which is a major S fraction compared to AVS (Rickard and Morse, 2005; Otero and Macías, 2002). The use of Fe-pyrite content to assess the intensity of sulfidization processes and improve the comprehension of C dynamics in coastal wetland soils have been successfully used in a huge number of studies (for example, please see Jimenez et al., 2022; Ferreira et al., 2022; 2015; Queiroz et al., 2022; Jimenez et al., 2021; Cabral et al., 2020; Nóbrega et al., 2016).

In this sense, the lack of total S and AVS content would not affect our study's interpretations.

Cabral, R. L. et al. How Do Plants and Climatic Conditions Control Soil Properties in Hypersaline Tidal Flats? Appl. Sci. 10, 7624 (2020).

Ferreira, T. O. et al. Pyrite as a proxy for the identification of former coastal lagoons in semiarid NE Brazil. Geo-Marine Lett. 35, 355–366 (2015).

Ferreira, T. O. et al. Litho-climatic characteristics and its control over mangrove soil geochemistry: A macro-scale approach. Sci. Total Environ. 811, 152152 (2022).

Jimenez, L. C. Z., Queiroz, H. M., Otero, X. L., Nóbrega, G. N. & Ferreira, T. O. Soil organic matter responses to mangrove restoration: A replanting experience in Northeast Brazil. Int. J. Environ. Res. Public Health 18, 1–11 (2021).

Jimenez, L. C. Z. et al. Recovery of Soil Processes in Replanted Mangroves: Implications for Soil Functions. Forests 13, 422 (2022).

Nóbrega, G. N. et al. Edaphic factors controlling summer (rainy season) greenhouse gas emissions (CO2 and CH4) from semiarid mangrove soils (NE-Brazil). Sci. Total Environ. 542, 685–693 (2016).

Otero, X. L. & Macias, F. Spatial variation in pyritization of trace metals in salt-marsh soils. Biogeochemistry2 62, 59–86 (2003).

Rickard, D. & Morse, J. W. Acid volatile sulfide (AVS). Mar. Chem. 97, 141–197 (2005).

Queiroz, H. M. et al. Changes in soil iron biogeochemistry in response to mangrove dieback. Biogeochemistry 158, 357–372 (2022).

#1.4. The total carbonate amount lacks in the paper.

Unfortunately, there was a mistake in the interpretation made by the reviewer. In fact, the total carbonate contents results are present in the text, referenced as calcium carbonate equivalent (CCE) since both calcite, dolomite, and other carbonates are quantified through a titration method, as recommended in the procedures for soil analysis (van Reeuwijk, 2002).

The method for CCE quantification is presented in L. 147, whereas the results can be seen in table 2 and section 3.3 (Please see L. 259-261)

Van Reeuwijk, L. P. Procedures for soil analysis. (ISRIC - World Soil Information, 2002).

#1.5. Other information to add, if possible, to the sites' description was the height of the water column at the time of sampling and the tidal movements of the water at the three study sites.

As requested, further information regarding the water column height and tidal phase during the samplings was included in the text. The authors are thankful for such a detailed revision, which considerably improved the rigour of our study.

#1.6. How long were the underwater soils studied in contact with the air?

The soils are permanently submerged throughout the year at the three studied sites, as described in the M&M section. For morphological description, the samples were removed from tubes and described within one to two hours to minimize atmospheric interference.

To improve the comprehension of the text and avoid misinterpretation, further information was included in the text.

#1.7. Does the height of the tides affect the amount of water present in the water column above the soils? Did it vary in the time (seasons/year)?

As answered in question #1.5, further information regarding water column height during sampling was included in the text. Besides, there is no evidence of seasonal variation in water column height for the studied sites.

#1.8. Table 1: Where the table is interrupted for the page change it is necessary to re-enter the name of the variables.

The authors are thankful for such detailed revision and suggestions performed by the Reviewer. Due to a large amount of data, there was a layout problem with the table, resulting in an interruption during the diagramming.

However, as suggested, the columns' titles were inserted on the table's second page to improve the text's comprehension.

#1.9. I see that the structure of soils was classified as massive or single grained, farther the massive structure was classified even in all A horizons rich in organic C (NE). What do the authors think it is due to?

In our opinion, for the studied soils, the difference between the occurrence of single-grained or massive horizons is mostly related to grain size composition. In fact, soil horizons with coarser grain-size composition (e.g., higher sand content, coarse sand or shell fragments) presented single-grain structure, whereas soils with higher clay content presented massive structure.

Thus, further information was included in the text to improve comprehension.

#1.10. Table 2 is unreadable. To make it readable I suggest you also see the chapters of the results to which it is possible to match the data, thus breaking down the large table into three smaller ones.

As mentioned in answer to question #1.8, there was a layout problem with the table, resulting in an interruption during the diagramming. The editorial board alerted the authors that the tables should be submitted in a vertical page layout, which resulted in the poor quality of the Table.

However, as suggested, Table 2 was divided into two tables. The "new Table 2" presents physicochemical conditions and attributes related to redox processes (e.g., Eh, pH, EC, and Fe partitioning), whereas "new Table 3" presents soil cation exchange capacities.

Additionally, to improve the readability and fluidity of the text, the section regarding Fe partitioning was moved up, following the sequence of variables on the tables. We believe that these changes improved the quality of the manuscript. The authors are thankful for the insightful suggestions performed by the Reviewer.

#1.11. The chapter Soil physicochemical conditions:

- **Soils redox and pH conditions**
- **CEC, salinity and sodicity of investigated soils**
- **Organic C and iron partitioning**

These could be the paragraphs to link the data to, all solutions are welcome.

As mentioned in answers to questions #1.8 and #1.10, there was a diagramming problem with Table 2.

Thus, to improve the quality of the manuscript, the table was divided into two tables, presenting physicochemical conditions and cation exchange capacities, respectively. Additionally, following the changes in the table, the section regarding Fe partitioning was moved up to improve the readability.

The authors are thankful for the suggestions, which considerably improved the manuscript's quality.

#1.12. Before the line 260: Organic C results. In NE both soils an organic C accumulation was detected, Also from Table 1. In NE1 Crz2 at 37-56 cm an increase of OC was observed. Is it possible that this could be a buried horizon? The change of lithology would make this. Also in NE2 3Cmz1 at 47-91 cm, the lithological discontinuity is marked, but can it a buried A horizons? Different pedogenetic cycles could be develop these soils?

The authors are thankful for such detailed comments performed by Reviewer #1. In fact, it was observed an increase in TOC contents in deeper soil horizons from the NE coast, which may suggest a polygenetic soil formation. Changes in soil grain size composition and the presence of lithological discontinuities reinforce this hypothesis. Thus, further information was included in the text to include these relevant aspects.

#1.13. In this late case 3Cmz1 horizon changed the CCE and Na exchangeable amount.

As suggested, further information was included in the text regarding an increase in the depth of the CCE contents in NE soils. The authors are thankful for the insightful suggestion.

#1.14. In table 2 what does V represent? I have not seen the explanation in the materials and methods.

In fact, there was a mistake in the M&M section, where no information was included regarding the base saturation (V%). Since Table 2 was divided, it was possible to include the complete column title for base saturation.

Additionally, further information was included in the main text to prevent misinterpretation and improve the rigour of the text.

#1.15. I do not think it is correct to call the sum of the two forms of Fe extracted pseudo totals.

Unfortunately, we kindly disagree with the Reviewer regarding the term "pseudo-total". In our study, the term "pseudo-total" refers to the sum of the two distinct fractions, which do not include Fe associated with organic matter, and silicates; Thus, we truly believe that the term "total content" is imprecise. Therefore, the authors decided to maintain the term "pseudo-total" throughout the text.

Additionally, the term pseudo-total is widely used in studies referring to the sum of fractions obtained by Fe partitioning methods, which would represent the same sum of the fractions used in our study (Araújo Júnior et al., 2016; Lacal et al., 2003; Nemati et al., 2011; Pereira et al., 2020; Santos et al., 2010), since reducible Fe oxy-hydroxides and pyrite are the main Fe fractions in coastal wetlands soils.

Araújo Júnior, J.M. de C., Ferreira, T.O., Suarez-Abelenda, M., Nóbrega, G.N., Albuquerque, A.G.B.M., Bezerra, A. de C., Otero, X.L., 2016. The role of bioturbation by Ucides cordatus crab in the fractionation and bioavailability of trace metals in tropical semiarid mangroves. Mar. Pollut. Bull. 111, 194–202. https://doi.org/10.1016/j.marpolbul.2016.07.011

Lacal, J., da Silva, M.P., Garćia, R., Sevilla, M.T., Procopio, J.R., Hernández, L., 2003. Study of fractionation and potential mobility of metal in sludge from pyrite mining and affected river sediments: changes in mobility over time and use of artificial ageing as a tool in environmental impact assessment. Environ. Pollut. 124, 291–305. https://doi.org/10.1016/S0269-7491(02)00461-X

Nemati, K., Abu Bakar, N.K., Bin Abas, M.R., Sobhanzadeh, E., Low, K.H., 2011. Comparison of unmodified and modified BCR sequential extraction schemes for the fractionation of heavy metals in shrimp aquaculture sludge from Selangor, Malaysia. Environ. Monit. Assess. 176, 313–320. https://doi.org/10.1007/s10661-010-1584-3

Santos, S., Costa, C.A.E., Duarte, A.C., Scherer, H.W., Schneider, R.J., Esteves, V.I., Santos, E.B.H., 2010. Influence of different organic amendments on the potential availability of metals from soil: A study on metal fractionation and extraction kinetics by EDTA. Chemosphere 78, 389–396. https://doi.org/10.1016/j.chemosphere.2009.11.008

Pereira, W.V. da S., Teixeira, R.A., Souza, E.S. de, Moraes, A.L.F. de, Campos, W.E.O., Amarante, C.B. do, Martins, G.C., Fernandes, A.R., 2020. Chemical fractionation and bioaccessibility of potentially toxic elements in area of artisanal gold mining in the Amazon. J. Environ. Manage. 267, 110644. https://doi.org/10.1016/j.jenvman.2020.110644

#1.16. The literature is not very clear on the DOP parameter, but it brings it back to the sum and it is not called pseudototal.

To improve comprehension regarding the degree of pyritization (DOP), further information was included in the text. Additionally, as stated in answer to question #1.16, the authors decided to maintain the term pseudototal referring to the sum of Fe-oxyhydroxides and Fe associated with pyrite.