

Interactive comment on “Seagrass community-level controls over organic carbon storage are constrained by geophysical attributes within meadows of Zanzibar, Tanzania” by E. Fay Belshe et al.

Anonymous Referee #2

Received and published: 26 January 2018

Overall comments

The manuscript by Belshe et al. attempts to provide insights into blue carbon storage capacity in seagrass areas off Zanzibar Island. While I view the study a welcome addition to the increasing global and regional focus on blue carbon sequestration, the style of argument/discussion places the authors' findings in a negative light rather than a substantial progress in this field. I have elaborated on this matter below, and other suggestions that will improve the manuscript. I look forward to reading a revised version of the manuscript in the near future.

[Printer-friendly version](#)

[Discussion paper](#)



Specific comments

1) One of the shortfalls in Fourqurean et al. 2012's paper is the limited number of African meadows considered in that paper's meta-analysis. This current study complements those already done off the African continent and would therefore allow more robust regional estimates in OC sequestration capacity. The authors, however, reported and emphasized low OC stocks in their study sites. This is not novel, in my opinion, since the authors' 33.9 Mg C ha⁻¹ estimates: 1) still fall in the global range of 9-628 Mg Corg ha⁻¹ in Fourqurean et al. 2012; 2) is just slightly higher than Fourqurean et al. 2012's estimate for the Indo-pacific region of 23.6 Mg Corg ha⁻¹; and 3) not that much different to those estimates done in SE Asia, which is in the same bioregion as this study (see below on this, and please also refer to OC stock estimates in Miyajima et al. 2015; Gilis et al. 2016, Quak et al. 2016; Rozaimi et al. 2017).

2) There is a fixation by this study as well as others already published on trying to predict OC storage capacity by biological and or physical drivers. It has already been suggested in Lavery et al. 2013 that variability can be expected and therefore I don't find it surprising Gullstrom et al. 2017 had different results compared to this study. Furthermore, many studies, e.g. Serrano et al. 2016a, already connected sediment grain size as negatively correlated with OC stocks. I don't see the logic, therefore, to persist looking into such "geophysical constraints" whereupon low OC stocks are to be expected. As it stands, this is the angle that the authors communicated, and therefore I view this study's findings not very interesting. BG is rarely seen as a journal of negative results and I recommend the authors portray their findings in a different light. What I do find interesting is that high seagrass biomass/density does not necessarily translate to high sediment OC stocks, especially in reference to the study's findings in Community B. Indeed, this is in stark comparison to e.g. Macreadie et al. 2012&2015's, and Serrano et al. 2014&2016's Posidonia studies, where such correlation is expected. I believe this angle can pique the interests of BG readers more than how it is now.

3) As it stands, using %N as a predictor variable is a particularly weak approach.

BGD

Interactive
comment

Printer-friendly version

Discussion paper



Seagrasses are naturally N limited and therefore I don't see its justification in this study. I note the authors attempted to relate N content/decomposition to CNP stoichiometry (Pg 2 L 29-31) but I need further convincing before agreeing this as a viable approach.

4) I am not comfortable with the authors' way in presenting OC cycling as the alternative explanation for their findings of low OC stocks. The context linking sequestration capacity and OC cycling is too broad in the absence of sufficient evidence, which in turn made it a rather unconvincing discussion. I suggest the authors discuss the findings along the lines that the studied meadows have low capacity to sequester autochthonous inputs. Such approach would still be in the bigger auspices of OC cycling and will not stray too far from the body of evidence already presented.

5) There is an underlying initial assumption by the authors that seagrass tissues are buried in the sediment and therefore sequestration occurred. It is unfortunate that OC provenances did not fall into the scope of this study. I do not insist this be done, but nonetheless, it is reasonable to infer from the results in Miyajima et al. 2015; Gilis et al. 2016, Quak et al. 2016 and Rozaimi et al. 2017 on seagrass endmember contributions to OC sediment sequestration. The seagrass meadows in these four studies (with the exception of particular sub-tropical and temperate meadows in Miyajima et al. 2015) and those in this study, are in the same bioregion (after Fourqurean et al. 2012). The studies I quote reported low seagrass contributions and also low OC stocks to the sediment. I also refer the authors to Bouillon et al. 2004 for a data set on OC provenances that were obtained closer to their sites. These papers may assist the authors in arguing their case succinctly.

6) I am particularly uncomfortable with the supposition laid in Pg 9 L25-28 on the historical colonization of seagrasses in the study area vis-à-vis carbon deposition and seagrass community structure. It is not supported by historical data and/or organic matter provenances (see above). The authors should have considered that the period of carbon accumulation (re Serrano et al. 2016b) is an important aspect of blue carbon accounting. A stronger case is needed before readers would agree to the assumption

[Printer-friendly version](#)[Discussion paper](#)

posed by the authors.

7) I recommend adding a Figure or Table summarizing the OC density (g OC cm⁻³) and/or sediment dry bulk density data to complement Figs 6 and 7.

Methods and design

8) General comments: a) specify water column depths of the sampled sites; b) specify if epiphytes were removed before weighing biomass samples for above-ground plant parts; c) clarify sediment acidification protocols; d) specify the use of CN ratio calculations in methods.

9) Coring methodology: a) I can accept that core compaction during sampling can be assumed negligible for short cores but longer cores require core length corrections. Please refer to Howard et al. 2014; b) I find it perplexing that the authors included extremely short cores in analysis when in fact it was possible to get longer cores within the same community site (i.e. A, B, C and F). The only logic I fathom is the insistence on a replication/ecological approach, which is not particularly essential in these types of biogeochemical/biogeophysical studies. Such inclusions of short and long cores as “replications” invites greater variability and more questions are thus raised on the robustness of the findings.

10) General writing clarity Pg 3 L16-18: “Yet, the ... Gullstrom et al. 2017) - Does not fit in Methods. Either move or remove to the appropriate section. Pg 9 L10-12: “...indicating the... Hessen et al. 2004).” Does not fit in Results. Either move or remove to the appropriate section. Pg 10 L5-8: Break this into two sentences Pg 10 L30-31: “Some of ... biomass” - Unclear sentence structure. Please edit. Pg 11 L15: ...and/or sediment <what?> - Sentence appears hanging. Pg 12 L29-30: A general rule I follow is to avoid references in conclusions, which would otherwise infer weak arguments for a study. Figure captions: Please consider truncating the captions to relaying the most relevant information only.

BGD

Interactive
comment

Printer-friendly version

Discussion paper



11) Minor technical edits Pg 1 L13: is sediment -> in sediment Pg 2 L25: determinates -> determinants Pg 4 L7: Sedimentary samples -> Sediment samples Pg 6 L20: g OC per dry weight <sediment?> Pg 7 L13: granumetrical -> granulometric Pg 13 to 20: Please relook at the reference list thoroughly: a) spacing between words, italicizations of species and genus names that are lacking should be edited; b) edit Serrano et al 2016 to Serrano et al. 2016a and 2016b, and the latter was already accepted as BG and should not be a BGD citation; c) you should cite the more recent Costanza et al. 2014 paper, rather than the 1997 paper; d) consider updating/revisiting the reference list as suggested in this review:

Bouillon et al. 2004. et al. Biogeosciences 1, 71–78. Costanza et al. 2014. Glob. Environ. Chang. 26, 152–158. Fourqurean et al. 2012. Nat. Geosci. 5, 505–509. Gillis et al. J. Sea Res. 120, 35–40. Gullström et al. 2017. Ecosystems 1–16. <https://doi.org/10.1007/s10021-017-0170-8> Howard et al. 2014. Coastal Blue Carbon: Methods for assessing carbon stocks and emissions factors in mangroves, tidal salt marshes, and seagrass meadows. Macreadie et al. 2012. Glob. Chang. Biol. 18, 891–901. Macreadie et al. 2015. Proc. R. Soc. B Biol. Sci. 282. Miyajima et al. 2015. Global Biogeochem. Cycles 29, 397–415. Quak et al. 2016. Estuar. Coast. Shelf Sci. 182, 136–145. Rozaimi et al. 2017. Mar. Pollut. Bull. 119. 253–260. Serrano et al. 2014. Global Biogeochem. Cycles 28, 950–961. Serrano et al. 2016a. Biogeosciences 13, 4915–4926. Serrano et al. 2016b. Biogeosciences 13, 4581–4594.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-474>, 2017.

Printer-friendly version

Discussion paper

