

GENERAL COMMENTS:

The submitted manuscript of Kindeberg et al. under review and discussion for the journal Biogeosciences presents summer benthic community metabolism and composition measurements over a “chrono-sequence” from bare sediments to different restored seagrass meadow development stages in a high temperate marine embayment in Sweden. Different flux techniques (aquatic Eddy Covariance, benthic chamber measurements) along with sediment and benthic fauna/flora characterization and associated computations (O_2 :DIC ratio, PQ and RQ, LUE, PI curves, etc.) were done to particularly resolve the links between carbon cycling and biodiversity in this restored seagrass meadow area.

The study presents very interesting in situ measurements, analysis and computations through a significant, detailed and well written manuscript. The latter is of particular importance as it identifies mechanisms and links between benthic carbon processes/fluxes and fauna/flora diversity over a restored seagrass meadow system and such coastal carbon research studies need to be done increasingly in the future; thus, I congratulate the authors for their work that is well suited for Biogeosciences journal.

I have two main general comments on the submitted manuscript (i) the first one concerns the lack of certain information especially on benthic flux measurements and computations described in the M&M section that could be given to help the readers to better follow in situ deployments done during the study. ii) my second and major concern is with regards to the chrono-sequence methodology and associated assumptions on which results and discussions relied. Indeed, as authors said, abiotic conditions during the four site measurements have to be not significantly different to truly endorse the four restored seagrass meadow development stage influence only on corresponding measured benthic carbon fluxes. Linear mixed effects models and associated statistical approaches have been used to address this fundamental purpose to validate the approach but associated results are not clear enough or even given in the manuscript as it stands. For instance, flow velocity (i.e. between bare/3years and 7 years/natural sites) as well as salinity and water temperatures during and between this summer week deployments experienced important and significant variations as rightly noticed by authors, that may have influenced benthic metabolism besides meadow habitat development itself. All these aspects need to be better ruled and discussed in the manuscript.

In this way, please see the specific comments below to help in the revision of the different sections and the overall manuscript.

SPECIFIC COMMENTS:

Abstract

- 1.32: not clear what are these values? CR? NCP?
- 1.33: what about heterotrophic biomass?

1. Introduction

This section is very good as it stands.

2. Material and methods

- 1.130: 1-4 m depth, is the studied zone subtidal? what about hydrodynamics, horizontal advection and influence of downstream and upstream systems? Please give general information on it.

- 1.136: please give the exact distance between the four sites.

- 1.139-143: indeed, these aspects need to be addressed (see general comment above); also, please refer to Table S1 and Fig. S1 here.

- 1.147-155: it is very important to refer here to Fig. S1, if not, we don't have any information on EC deployment beginnings and ends (days, dates, numbers, hours, durations at each site), these information have to be given in the text or at least in the Fig. S1 caption. A photo of the EC frame in situ deployed with habitats could be nice in the supplementary material as well.

- 1.157-163: similarly, information according to benthic chamber incubations are lacking and must be given in this sub-section: the number of incubations at each site, the order of incubations between clear and dark chambers, the durations of each incubation, the dates of beginning and ending of each incubation, the correspondence with EC deployment (corresponding positions and times?), were they deployed simultaneously with EC measurements? A table with all these EC and BC information could be added in the MS (supplementary material besides the Table S1).

- 1.193-194: why O₂ concentrations were not measured continuously inside the chamber during each incubation and only at the beginning and at the end of it? With regards to the laboratory experience testing the assumption concentrations change linearly with time, why could authors not test it in situ?

- 1.207: the 2.3.4 Chlorophyll a subsection could be displaced right after the 2.3.1 Macrophytes one as we wonder here if microalgae (microphytobenthos) have been measured as well along with macroalgae at each station.

- 1.227: how authors are sure the OM versus POC linear relationship they obtained for the top 0-2 cm sediment layer in the 12 samples is the same or is well suited for the other core slices? Is there no variability according to sediment depth for sure?

- 1.246: why authors used this flux formulation instead of the one taking into account surface and volume chambers and continuous O₂ concentration measurements? (see previous comment above)

- 1.249: authors computed salinity normalized TA and DIC fluxes, could they give here the range of salinity they measured at each site during the incubations please?

- 1.260: that is why information previously asked in comments above are important to clearly understand what was in situ done in the study.

- 1.315-326: Statistical (linear mixed-effects) models used to test the assumptions of similar or non-significant differences in environmental conditions during the 4 stations deployments, measurements are well described here, the presentation of the associated results in the manuscript is another story (see general comment above and other specific comments below). Authors could also better or in a clearer way present in the result section, their statistical tests

and results to show if significant differences in environmental parameters (water temperature, turbidity, current, salinity) existed between each site.

- 1.331-332: what about microalgae, was it taken into account here in the carbon budget formulation (sigma algae)?

3. Results

- 1.342-347: salinity variations from 24.7 to 28.9 are important and could have an (indirect) influence on benthic carbon metabolism at each sampled station. If rain events were minor as author said, could they give explanations on these salinity variations (hydrodynamics?) during this summer week please? Salinity remained constant during each individual deployment, at least between bare and 3 years sites and between 7 years and natural sites according to Fig. S1 (please complete the caption, insufficient information about colors, year, idem Fig. S2), however it is not clear, are there significant variation in salinity values among the four deployments? Please give the same results for all the other abiotic measured parameters (flow velocity, water temperature, turbidity, oxygen concentrations, aquatic PAR, nutrients, TA, DIC etc.) summarized in a table to clearly rule these important considerations according to the author chrono-sequence assumption and possible interference with it in the associated results and discussion. In which sense have flow velocities varied (0,9-21 cm s⁻¹)? Yet, with these important flow velocity variations, I doubt flow velocity didn't influence at all measured benthic fluxes and did not partly explain associated flux differences observed between sampled stations besides (meadow development) habitats?

- 1.348-351: here again, authors have to be clearer, did they measure significant differences in salinity, TA, DIC and DIN between the four sites and between which sites, or not? Please better do the link between these parameter variations and the hydrodynamic of the site. Values given 1.351 do not correspond to Table S1 for bare sediment?

- 1.362-369: authors clearly computed significant relationships between benthic fluxes and flow velocities, moreover their linear mixed effect models indicated PAR and flow velocity explain a large portion of the variation in O₂ fluxes, along with other parameters not included in the model. All these considerations and results should be addressed in a clearer way and discussed after in the discussion section. Is it possible to include in the model parameters such as salinity, temperature for instance?

- 1.443 and Fig. 4: only fauna in the sediment (infauna) was encountered at the bare site?

- 1.453 and 1.459: how authors explain that large within site POC variability?

- 1.484 and Table 3: interesting modelling tested by authors, have you tried to test these models with other parameters than the meadow age?

4. Discussion

This section is good and well-written. However, with regards to my general and specific comments, it is important to add a sub-section or clear elements on the limitation of the chrono-sequence approach used here due to environmental condition difference observed during this summer week measurement between the four sampled sites.

- 1.514-515: please specify here, as the link between both contributions from macrophytes diversity and benthic fauna communities to benthic carbon metabolism is hard to dissociate.
- 1.516: it could also be interesting to discuss and compare expected results that could arise at other seasons than summer?
- 1.521-522: values could be given here in the discussion as a reminder.
- 1.548-549: the sentence is not clear, please specify; if both GPP and CR increased, it does not always imply an autotrophic diversity increase?
- 1.578-580: please specify these other biogeochemical processes or delete this sentence since as authors said it is speculative at this stage.
- 1.585-588: please specify.

TECHNICAL CORRECTIONS:

- 1.44: delete “crisis” and “the” and replace “crisis” by “crise”
- 1.126-128 and Fig.1: where are a) and b)
- 1.371: the weakest instead of “lowest”
- Fig. 3 caption: delete “of” before oxygen
- 1.375: -21 mmol m⁻² d⁻¹
- 1.452: in “the” natural meadow core instead of “a”