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Interactive comment on "Spatiotemporal variability of sedimentary organic matter supply and recycling processes in coral reefs of Tayrona National Natural Park, Colombian Caribbean" *by* E. Bayraktarov and C. Wild

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We would like to thank Anonymous Referee #1 for taking the time to comment on the present manuscript and would like to address the following points (**):

** I cannot fully evaluate the statistical procedures and recommend that an expert in this assess this point.

The statistical analyses applied within the present manuscript satisfy the requirements necessary for multivariate non-parametric environmental data. Permutation analyses

of variances based on Euclidean (dis)similarity distances are often used for data on environmental monitoring (please see Anderson et al. 2008 and Anderson et al. 2001 for a detailed description of the statistical methods). The design and statistical analyses of the present study are consistent with methods used in long-term environmental monitoring and assessments, e.g. on water quality (Wear & Tanner 2007; Bayraktarov et al. 2014).

** 1) It is very poorly documented how the hydrographic data (or other data) are used to define the different annual stages: Major upwelling/dry season (December-April); minor non-upwelling/minor rainy season (Maj-June); minor upwelling/dry season (July-August); major non-upwelling/rainy season (September- November). Some older general references are given to justify this. But surely there must be a large inter-annual variation in this pattern. This becomes very important as you integrate data for these different periods. How well do your hydrographic surveys justify such very distinct patterns linked to specific calendar months during your investigations?

We agree with the reviewer and clarified the definition of seasons as follows "Seasonal time intervals of the TNNP used for statistical analyses of sedimentary parameters were: major upwelling season (major dry season, December – April), minor non-upwelling season (minor rainy season, May – June), minor upwelling season (minor dry season, July – August), and major non-upwelling season (major rainy season, September – November). This is in accordance with the long-term hydrographical survey by Bayraktarov et al., (2014) and further supported by Salzwedel and Müller, (1983); Bula-Meyer, (1990); Diaz-Pulido and Garzón-Ferreira, (2002); and Andrade and Barton, (2005)." This information has now been given on page 9.

** 2) I cannot understand the procedure used to quantify the benthic turn-over rates (I would like to add that I consider myself to be an expert on this – having performed hundreds of such measurements with a wide range of techniques in a wide range of environments). There is very little detail provided about this and if I have understood this correct (see below) I cannot see how this can provide an quantitative assessment of the benthic carbon turnover rates in these sediments. This is essential as these a key data set for the main conclusion of the manuscript. Maybe better explanation solve this issue (see specifics below) – if not I think the dataset is very questionable. I have not consulted Wild et al 2010, which is referenced in the Method sections describing the applied procedure. But in my opinion it should be possible for the reader to understand the applied procedures, without consul additional literature – at least when the data sets are essential for the conclusion.

The C turnover rates of the present study were calculated by relating the supplied particulate organic carbon (POC; as derived from the sediment trap deployments) to the sedimentary oxygen demand (SOD; as derived from the incubation experiments) assuming that 1 mol of supplied organic C to the sediments is mineralized to CO2 by 1 mol of consumed O2. However, responding to another comment of the reviewer (please see below), we decided that we take out the C turnover calculations from the revised manuscript.

** 3) I have a hard time understanding that the turnover rates not are confounded by benthic primary production? This both relates to the pigment levels and the O2 turnover rates measured in darkness (local PP could provide a carbon source turned over in darkness) – this point is hardly discussed, but could potentially compromise the calculations you perform. – A side point any suspension and re deposition in the area that could affect your calculations and extrapolations? in the area?

These points are now addressed in the discussion on page 17 of the revised manuscript. We agree that microphytobenthos may have contribute to sedimentary oxygen consumption, but do not assume that this is the main reason for the observed spatial differences. This argument was supported by comparing our results to a study on primary production at the same sampling locations showing no temporal or spatial differences between net and gross photosynthesis in marine sand samples (Eidens et al. 2014): "The observed spatial differences in turnover rates could not be explained by the contribution of sand inhabiting microphytobenthos to the consumption of oxy-

gen since no spatial or seasonal differences were observed in a study on primary production between an exposed (net primary production 2.3 mmol O2 m-2 during non-upwelling and 0.1 mmol O2 m-2 during upwelling) and sheltered (2.3 and 0.1 mmol O2 m-2, respectively) site (Eidens et al. 2014)." We cannot exclude resuspension via wave influence, but because of the relatively deep sampling locations (10 m water depth), we consider this factor as negligible. This info has now also been given on page 5 of the revised manuscript.

** 4) I do not fully understand why you include the very many pigment analysis (and associated statistical treatments) – in the end you do really use then for very much – Are they required for the key concluisons and "punch.-lines"? In my opinion not and they could be eliminated (see specifics below) Below I have add more specific comment in the order of appearance.

We agree with the reviewer that data on pigment analysis could be reduced to chl a only without any loss of important information which is required for the study conclusions. This has now been carried out in the revised manuscript.

** Abstract: I miss any consideration/comments on local benthic primary production (I suppose nutrient enrichment could stimulate local benthic PP and thereby dark-time SOC? And that local benthic PP could contribute to the supply of labile Org C) Line 19 : 4.4 % h-1 is a bit of a strange unit, why not provide the numbers in mol C d-1. This would give the reader a direct opportunity for relating to the absolute values.

We were interested to evaluate how much of the supplied POC was mineralized how fast in the sediments. However, we agree with the critical comment of the reviewer and therefore excluded all C turnover calculations from the revised manuscripts. Consequently, the manuscript conclusions on recycling processes within the sediments are now based on sedimentary SOD rates only.

** Introduction: P19896, Line 25 : "They cover over 70% of the worlds: : :" - What is "they", marine sediments? So what is the rest bare rock? Please specify what you mean. P19896 Line 26: “83% of all remineralization…” that cannot be correct! Firstly please specify what you mean by “shelf sediments” – give a depth range. Secondly please update the values with some more recent number (there are more recent compilations based on the available global data base). P19897 Line 3: This is a bit confusing. You are right that aerobic respiration can account for a significant proportion of total carbon turn-over (Again you use a very old ref for this – more updated values have been provide in the last 25 years). But why do you provide this information – if you do not discuss the other contributors. I think the important point– in the present context – is to emphasize that the O2 uptake account for 100% of the turn-over (integrating aerobic and anaerobic turn-over when measuring SOC) as it include the rexodation processes.

We revised the related text passages and clarified the function of shelf sediments for the degradation of organic matter and the features of the oxic layer. We addressed more recent compilations of data as follows: ”Marine shelf sediments are the major sites for mineralization and nutrient regeneration of organic matter derived from pelagic primary production and terrestrial input (reviewed by Arndt et al. 2013). A fraction of 25 – 50 % of the organic matter derived from coastal primary production is deposited to the sediments (Nixon, 1981, Wollast, 1991, Jørgensen, 1996). The oxic surface layer of marine shelf sediments is restricted to only a few millimeters to one cm deep at the sediment surface (Rasmussen and Jørgensen, 1992; Kühl and Jørgensen, 1994; Arnd et al. 2013) but can account for more than half of the total organic carbon mineralization (Jørgensen and Revsbech, 1989; Köster et al., 2000). The remaining organic carbon is degraded by anaerobic processes such as denitrification, manganese, iron and sulfate reduction, followed by methanogenesis and/or fermentation (Henrichs and Reeburgh, 1987). However, much of the sediment oxygen uptake in fine-grained coastal sediments is used to reoxidize the reduced products of anaerobic respiration (Canfield, 1993).The biogeochemical processes in the oxic sediment layer play a particularly important role for highly permeable, carbonate sediments in coral reefs (Boucher et al., 1994; Alongi et al., 1996; Werner et al., 2006; Huettel et al. 2014).”

** Page 19898 line 5: do you here mean benthic PP or coral associated PP.

We mean benthic primary production of coral reef organisms and changed the text passage accordingly: “reef sediments are crucial for the functioning of coral ecosystems and help to maintain the typically high biomass and primary productivity of coral reefs”

** Page 19898 line 8: I do not see the argument for “why it is particular important” I guess you could also argue that it is particular important in extreme oligotrophic settings, where the relative importance of the benthic processes are quantitatively more important for reef functioning. I suggest rewording.

The investigation of sedimentary properties and processes in coral reefs under oligotrophic settings showed that recycling of organic matter and release of inorganic nutrients are important for ecosystem functioning. However, the importance of reef sediments in regions with seasonal upwelling which leads to a surplus of nutrients has not been investigated to our knowledge. We provided an example why sedimentary processes are especially important for regions affected by pulses of organic matter on Page 19897 line 29 – Page 19898 line 2. However, we delete the word “particularly”.

** Page 19898 line 13-17: I was wondering if seasonal dynamic in discharge mattered when I was reading the Abstract – it was not mentioned. But now you mention here that this effect “highly influence” PP – maybe ensure a bit better consistency in your wording.

We agree and deleted the word “highly”.

** Page 19899 line 15: Just for consistency and since you mentioned that aerobic respiration accounted for “more than half” of the carbon turnover (see above) I would suggest writing “ a proxy for TOTAL sedimentary organic matter recycling”.

We agree and insert the suggested change in the updated version of the manuscript.

** Materials & methods: Page 19899 line 24: Are these “exemplary sites” the same as the dots indicated for the hydrographic survey? Please specify.

The hydrographic surveys were performed in parallel and at the same sampling sites, but these data are presented in a different study. We added an explanation and reference to the hydrographic study on Page 19899, line 21: "The sampling sites of the present study are identical with the sites where a hydrographic survey was performed (Bayraktarov et al. 2014)."

** Page 19900 line 13: How did you check that the gas production ceased?

Gas production after acid addition was assessed visually and acoustically. This information has now been inserted in the method section of page 6 of the revised manuscript.

** Page 19902 line 16-ff: You have to provide more details for the core incubation procedure. The reader do not want to consult Wild et al 2010 in order to find out how you have done these are very central measurements for the manuscript.

The incubation procedure is described between Page 19902 line 15 and Page 19903 line 8. In brief, sediment was sampled with mini corers and incubated in sealed, non-mixed glass containers filled with in situ seawater for 8 h and under exclusion of light. SOD was calculated by subtracting the O2 concentration prior to incubation from the O2 value after incubation. The difference in O2 concentration was divided by the time of incubation. Respiration from seawater control samples was subtracted from sediment respiration in order to exclude the contribution of water column microbial respiration.

** Do I understand this correctly that you only sample sediment cores down to 1cm depth? Why? Then the incubation values will hardly reflect the integrated benthic response towards carbon enrichment? You want to ensure that you integrate the sediment section that is enriched by carbon and that will be many cm given advection and bioturbation – typically you take sediment cores of 10-15 cm lengths for these kind of measurements. Maybe I have misunderstood but then please clarify.

We sampled the first cm of the sediment only, because that is the place where the

sedimented POC arrives and is usually rapidly turned over by microbes via oxic respiration. We however agree that advection and bioturbation may transport organic matter deeper in permeable sediments, but we decided against deeper core samplings, because we did not want to measure chemical reoxidation of reduced electron acceptors originating from older organic matter degradation processes, but rather remineralization of freshly supplied organic matter in the sediment surface layer. At the same time, test measurements revealed that no bioturbating meiofauna was present in the sediment samples, indicating that bioturbation was very low. This information has now been given on pages 7-8 of the revised manuscript.

** What is the core area and enclosed volume? How is the water mixed/circulated during the incubation? How much did the O2 decline (in %) during incubation – can you assume a linear decline? Any hints to how porewater during incubations resembled in situ conditions? These are very important points! You need to give the reader trust in the applied procedure.

The core area was 6.61 cm^2 which results in an enclosed volume of 6.61 cm^3 taking into account the first cm of the sediment. The incubations of 6.61 cm^3 sediment sample were performed in closed glass containers filled with 80 ml seawater in order to measure diffusive oxygen fluxes. Only at the end of the incubations, water was thoroughly stirred in order to destroy concentration gradients. Oxygen concentrations during dark incubations decreased by 21 %(mean) to 47 % (max) during the incubations of 8 h duration. Final oxygen concentrations never reached below 50 % saturation so that we can assume a linear decline. This information has now been given in the methods section on page 8 of the revised manuscript.

** Results: Figs –indicate in the legend what the color zonation's indicate. "major dry; "minor rainy" etc. In the text (19903 line 12-15) you define 4 periods, why not maintain those in your figure?

We decided to highlight the major dry season in blue since water temperature dramatically decreases during this period triggered by the major upwelling events. The legends of Fig. 2 and Fig. 3 have been modified accordingly.

** Page 19906 line 2-5: To me it seems more natural to wait with all these relations until all data have been presented – ie to me it would be logical to move this to "Discussion".

We agree with the reviewer and present all relationships between the study variables within the discussion.

** Page 19907-19908: repeating all of the statistical results of the respective tables over two pages is not very inspiring. The readers can read the Table on their own. Rather explain the overall patterns and focus on the punch lines in words selling/presenting the overall idea/concept.

We have modified the presentation of results according to the suggestion by the reviewer.

** Is it really required to resolve the different pigment classes for the overall conclusion?

We agree to remove the analysis of different pigment classes and only to present data on chlorophyll a.

** Page 19908 line 19-21: You need to convince the reader that the differences in SOD not is related to the way the incubations were performed – see above. I would really appreciate to see the individual SOD values presented seasonally - as Fig 2 &3: Page 19908 line 23-24: Here you ought to explicitly state how you derive the POC turn over rates.

The figures representing the SOD rates are available as Fig. S3 within the supplementary material of the present manuscript. The POC turnover rates were excluded from the updated manuscript version.

** Discussion Page 19910 line 11-12: What should be the mechanism driving a C:N ratio below 6.6?

In the updated manuscript version, we introduce five mechanisms explaining regional variation in C:N ratios and offer a suggestion which of the mechanisms are likely present for the Tayrona Park: "Only during January, C:N ratio of organic matter supplied to the sediments decreased below 6.6 indicating a depletion of organic carbon and a relative enrichment in N, supported by observations from cold, upwelling rich regions which are governed by high turbidity and nutrient availability (Martiny et al., 2013a). Typically during non-upwelling, C:N ratios exceeded 8 which goes along with results from warm oligotrophic regions where inorganic nutrients are depleted (Martiny et al., 2013a).The C:N ratio links the carbon and nitrogen cycles and yet the systematic regional variations in C:N are not fully understood. Some authors suggest that variations in C:N ratio are attributed to regional differences in environmental conditions and phytoplankton diversity while at least five C:N ratio-driving mechanisms exist: (1) Content of cellular N declines when cells are N-limited leading to an increased C:N ratio (Vrede et al., 2002); (2) Lower cellular C:N ratio is present for phytoplankton growing under low light irradiance and high nutrient availability (Cronin and Lodge, 2003); (3) Negative relationship between C:N and growth rate (Chalup and Law, 1990); (4) Changes in phytoplankton community composition lead to variation in C:N ratio (Martiny et al., 2013b); and (5) Influence of detritus on C:N ratio (Martiny et al., 2013b). While mechanisms (3), (4) and (5) cannot be excluded, the C:N ratios of the present study show a typical pattern characterized by light (typically decreased during upwelling due to higher turbidity; Bayraktarov et al., 2014) and nutrient availability (increased during upwelling; Bayraktarov et al., 2013, 2014)."

** Page 19911 line 18-27: Are these indicative observation on pigmentations really important? They are at best only indicative. If you do have benthic primary production – it would in my opinion compromise your direct budgets on POC turnover rates (which I understand is derived by POC sedimentation divided by the SOD). This needs to be discussed Any arguments that the why you quantify the sedimentation rate – ant are affected by procedure itself (resuspension, trapping effects, fauna tarppingetc) – this could be discussed

We reduced the presentation of pigment analysis and only used chlorophyll a data in the revised version of the manuscript. We also added the following additional information to the description of the trap results in the results section: "Because of the design of the sediment traps, its short deployment duration of 48 h, and visual inspection of the trap contents, we can largely exclude resuspension and trapping of benthic fauna. Our results therefore closely reflect the sedimented POM from the water column.", added on page 14 in the discussion of the updated manuscript version.

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