

Interactive
Comment

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

Anonymous Referee #1

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Assessment “bg-2013-560” spatiotemporal variability of sedimentary organic matter. . .” by Bayraktarov & Wild.

Overall, this is a nice well-written manuscript relating seasonal input of organic material and large scale sedimentation patterns to the benthic turn over in sediments around a coral reef. The manuscript present “circumstantial evidence” for the general observation that permeable sand works as biocatalytic filters and efficiently degrade deposited labile material. This has been shown before – and generally there are little novel findings in the manuscript that in my opinion mainly has local interest. On the

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other hand the study does add to the steady growing global database on turn-over rate in permeable (tropical) settings. It is an editorial decision if this merits publication in Biogeosciences.

Beside these general comments I do have some concerns about the presentation and extent the presented data supports the conclusions. I cannot fully evaluate the statistical procedures and recommend that an expert in this assess this point. the Major concerns include.

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?

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.

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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?

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 conclusions 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.

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.

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.

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

Materials & methods: Page 19899 line 24: Are these “exemplary sites” the same as the dots indicated for the hydrographic survey? Please specify. Page 19900 line 13: How did you check that the gas production ceased? 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. 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. 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.

Results: Figs –indicate in the legend what the color zonation’s indicate. “major dry;

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“minor rainy” etc. In the text (19903 line 12-15) you define 4 periods, why not maintain those in your figure? 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”. 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. Is it really required to resolve the different pigment classes for the overall conclusion? 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.

Discussion Page 19910 line 11-12: What should be the mechanism driving a C:N ratio below 6.6? 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 tarping etc) – this could be discussed

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