

Interactive comment on “Small scale variability of geomorphological settings influences mangrove-derived organic matter export in a tropical bay” by Geraldina Signa et al.

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General comments:

This paper deals with the fate of the mangrove derived organic matter in relation with local geomorphological differences. Using elemental, isotopic and Fatty acids markers, the authors emphasize the combined role of the tide and the riverine water runoff in the distribution of the Mangrove organic matter. This paper is the latest of a long series of studies that characterised OM in Gazi Bay. The “plus” of this paper is the recording of FAs data and the fact that two seasons were sampled. Therefore, the main finding of this paper is the seasonal differences in term of export, which help to better understand the OM dynamic in the Bay; the combined control of tide and runoff is not something

C1

exceptional as this is a general feature of estuarine mangrove and this must have been anticipated. The paper is well written and organised with however overstretching use of the fatty acid method. I have several concern, some I share with the other reviewer, I already read the comments, such as the statistical issues.

My main concern is on the manner how fatty acids were ascribed to sources is this study. The Fatty acid markers method, have evolved quiet a lot in the recent years. The use of FAs in a qualitative, at best semi-quantitative, manner (%) need some precautions when it comes to comparing them in living tissues and to extrapolate these relative contributions to “non living” matter in the environment. The conservative feature of these markers do not apply in sediment or POM and most of the fatty acids, at least most of those used in this study, cannot be ascribed solely to one particular source. It is probably not necessary to analyse microorganisms such as Bacteria but it would have been suitable to look at the composition of microalgae and terrestrial sources that can be brought by water draining. Indeed, relating 20:5w3 to red algae is not a good assumption when this FA is readily present in diatoms and/or other brown algae who seem to be dominant in this bay. The question is the big amount of brown algae (+ diatoms) with low % can be of less impact than red macroalgae with high % of 20:5w3 but low biomass? Also, 18:3w3 is also found in large amounts if terrestrial leaves and is more labile than LCFAs in sediments. 18:2w6 is very common in wastes and agriculture waters and we don't have indication about these possibilities in the Method's section. Also there in no indication about the seasonal changes that may affect the composition of sources which certainly can moderate here or there their relative contributions at the surface sediments as well as in the SPOM. A better knowledge on available sources and how their productions are impacted by seasonal patterns would have render this spatially restricted study to be less speculative in term on fatty acids evidences.

Other comments

Introduction P4L10 : typo : approaches P4L2 : it is important de say if it is a qualitative

C2

or quantitative contribution

M&M : P 6 L18: Here and the entire paper, including tables and figures; the terminology of saturated Fatty acids is not properly defined : the is one "0" to much 23:0 instead of 23:00 and so on for all the paper.

P7 L4: I am puzzled by the transformation arcsine square root because % data are "transformed " (total 100%) which means that they have to be used as it is.

P7 L10 : using SIMPER to identify potential FAs is somehow wrong , SIMPER give you what are the FA that contribute the most to the similarity . A small contribution of a "specific" FA, say a Branched one for Bacteria, would be a enough to trace the OM and still, will not show up in the best five of the primer analysis. This practice adds confusion on the data that are % but discussed in a quantitative manner.

Results : P7 L15to L21: all comparisons need to be tested statistically P8 L5 the Bayesian model (SIAR) is may be not needed to see the contributions of the sources since there is no fractionation to correct.

P9 : L 25: Using 18:2w6+18:3w3 as tracers of seagrasses in zone full of mangrove is very risky.

Discussion :

P10 L26 : In this paragraph it will be useful to discuss possible ocean inputs (seaward station) P13 L4-L5 : Speculative. P4L15 : 16:1w7/16:0 is certainly not an indicator of dino/diatoms and , 20:5w3/22ww3 is a diatom/dino marker (not the opposite) . Another reason to not ascribed 20:5w3 to red algae. P14 L25 : it is very speculative to relate a relative increase of bacterial FA (compare to what ?) to an increase bacterial activity , at best it may show an increase in biomass but only if to compare the same site, for instance between season.

Figure 1 : the limit of the mangrove forest is not obvious in the map, please write Mkurumunji RIVER Fig 7 and 8 : besides all my comments on the use of markers, here

C3

I would like to emphasize that Branched and 18:1w7 are surely tracers of bacteria, but one should complete the other and must not any more be added as it was done 20 years ago . there are many papers that show discrepancy between these two type of markers.

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C4