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Interactive comment on “Transformation and fate of microphytobenthos carbon in subtropical, intertidal sediments: long-term carbon retention revealed by ^{13}C -labeling” by J. M. Oakes and B. D. Eyre

Anonymous Referee #1

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This manuscript presents an assessment of the fate of microphytobenthos (MPB) production in a subtropical intertidal sediment. In this thorough study looking at yet another type of benthic environment and at yet another latitude, the authors deliver yet another set of data on the relative fractions and contributions of the different auto- and heterotrophic compartments of a sandy sediment. With respect to the data set produced, this study is interesting and legitimate. However, with respect to the take-home message the authors try to deliver, the study fails. This work offers no evidence that a significant MPB biomass could be stored in the sediment to be qualified as “blue car-

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bon". The novel idea presented by this study is interesting (MPB carbon storage), the topic is "sexy" (Blue Carbon) and I understand the rationale behind the work, but trying to derive some carbon storage potential from this study would be hasty.

The potential significance of MPB storage suggested by this work is weak: 1) 50% of the production is lost (likely by resuspension) and 30% remains in the sediment. The rest is assimilated and respired. This is nothing out of the ordinary for this system and probably others (next point). 2) This is expected as the experiment was done after a flood event and one more flood event happened during the study. As noted by the authors on p. 19794, "episodic freshwater flows are typical of any (sub) tropical system. . .". Unfortunately the authors use a contradictory statement to support the potential for C storage in the following sentence of the same paragraph, and rather boldly in the conclusive section p. 19797: "...there could be a substantial contribution of MPB to long-term carbon sequestration in unvegetated sediments. This is particularly remarkable given that the burial and retention of MPB-derived carbon in the current study was observed under conditions of high flow, when resuspension would be enhanced." 3) Macrofauna was absent and only foraminifera were sampled and analyzed, showing a negligible contribution to C processing. The absence of grazing is likely having a high impact on the MPB standing stock and recycling on the sediment organic matter. The absence of fauna is probably related to the highly dynamic characteristics of this benthic environment: high flow, resuspension of sediment and MPB, variation of salinity (1-24) will not allow macrofauna to thrive. Once again, should these episodic flood events stop, larvae would settle, biofilm and fauna would flourish and ultimately, the recycling of organic matter would be very different. . . 4) The authors compare their study site to very different (mainly temperate) systems from other studies that were much bigger. This study was done in a much smaller estuary that seems to show the chemical and morphological characteristics of a river. It would have been interesting to show the relevance of this type of system at the global scale, but the study would need to be replicated. 5) Also, considering the higher stable isotope values found deeper in the sediment, I wouldn't be surprised if seagrass burial was more relevant to C burial

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than MPB.

Because the data and discussion don't support the hypothesis tested, I will reject this manuscript.

However, the data could be published elsewhere after major revisions. The following points (and other comments in the attached document) will need to be considered and carefully addressed: 1) The title is misleading and needs to be more specific. This is one study in one system, therefore it should not be generalized: "Transformation... in a subtropical intertidal sediment..." (no plural) 2) Despite the paper published earlier (Oakes et al. 2010b), the use of the reference to Bellinger et al. (2009) paper was erroneous and PLFA 16:1(n-7) is not MPB-specific. The biomass and uptake calculations need to be redone. Please see the attached document for more details and supportive references. 3) It will be very important to present the PLFA data and the methods you have used to calculate the different compartment biomasses and uptakes. It is also necessary to present the approach used to calculate the stable isotope signatures (natural abundance) of the different compartments, as well as the conversion of ^{13}C -PLFA to ^{13}C -microorganism. Despite your referring to your previous papers, a lot of the information is in the background and the reader is left to believe and trust. However, my previous point shows that mistakes can be carried over. 4) You need to take into account the dilution of label for bacteria (and any other heterotroph): while for MPB the DIC pool was 100% labelled, bacteria rely on fresh ^{13}C labelled MPB and MPB-byproducts within a pool of unlabelled labile organic material. You have assessed the standing stock of the different compartments at the start of your experiment. You should use these to estimate the relative contributions based on these dilutions. This also applies to respiration.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/10/C8537/2014/bgd-10-C8537-2014-supplement.pdf>

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