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

**Anonymous Referee #2**

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**General Comments** In this study, the authors aimed to follow the fate of carbon fixed by microphytobenthos (MPB) in subtropical, intertidal sediments. MPB are widespread and very productive in coastal sediments. Understanding the fate of that production has been the focus of numerous studies over recent years, but quantifying the importance of specific processing pathways remains a challenge. The authors in this study used an elegant but proven method of spraying  $^{13}\text{C}$ -labeled  $\text{HCO}_3^-$  directly to the sediment surface in the field and then following uptake of the label by MPB and subsequent transfer of that label to bacteria, DOC, DIC, and burial deeper > 2 cm over 30 days,

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which is a longer time period than many similar studies. The authors found MPB to be the major pool of organic carbon in the surface sediments. Of the  $^{13}\text{C}$  fixed by MPB, ~70% was lost from the sediments via resuspension. Of the remaining  $^{13}\text{C}$  fixed, a portion was transferred to bacteria, some into respiration products, and some was transferred to deeper sediments (2-10 cm), suggesting the potential for long-term retention of MPB-sourced carbon in unvegetated sediments.

I think that this paper does indeed advance our knowledge of the fate of MPB carbon in coastal systems. This is a very well designed study, and the authors present intriguing data. The authors were able to simultaneously quantify as many potential fates as possible, which is a particular challenge in field-based studies. Following the longer-term fate (> 3-4 days) of MPB carbon is a unique feature of this study, and their results support the potential for these unvegetated sediments to contribute to so-called “blue carbon” burial in coastal systems. This importance of this habitat as a carbon burial site will have to be investigated over longer time scales (>1 month), but this study provides strong evidence that it warrants further investigation. The only point I recommend the authors address more completely is the potential for these unvegetated sediments to be sites of blue carbon storage in light of how susceptible they may be to resuspension because they are unvegetated. Certainly this will be system-specific, but, for example, in their system, how deep do typical resuspension or scouring events disturb the sediments? I am very comfortable with their conclusions and support publication of this manuscript with minor edits, as detailed below.

### Specific Comments

Section 2.3 Sample Collection –Why were extra cores taken on Days 0.5 and 1? I don’t think it is described later. –From the sampling description, it seems like the cores have to be incubated under inundated conditions first, before being incubated under exposed conditions. I assume once the sediments are pushed up for the exposed conditions they cannot be pushed back down for an inundated incubation? Please clarify.

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**BGD**

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Section 2.5 Calculations –Here, the authors describe how natural abundance d13C values for bacteria and MPB are estimated from d13C values of PLFAs specific to each group. But in the caption of Table 1, they describe the d13C values as being representative of “whole cells.” This makes it sound like they ran the d13C analysis on whole bacteria and MPB cells, which I don’t think is the case. Perhaps the table caption can be clarified to include the fact that these are PLFA-derived estimates of d13C? –It would also be helpful in the calculation section (or in the discussion) to mention some of the uncertainty associated with using PLFAs to estimate bacteria and BMA biomass. The authors reference Oakes et al. 2010 for the method, but a short comment on the method is warranted here. For example, how variable are the concentrations of those specific PLFAs in an individual cell? –Why did the authors use the 2-G model for the entire 0-10cm section instead of, for example, just the 0-2 cm section?

Section 3.4 13C incorporation, burial, and transfer –The authors describe here that bacteria account for a peak of 30.5% of the 13C within sediment OC at 20d after label addition. But, it looks from Fig. 3 that bacteria peak on days 2-11, not 20. Please clarify. –I like Fig. 3, and I think it very clearly shows the patterns of 13C uptake over the course of the experiment. However, at times, I find the text describing Fig. 3 a bit difficult to follow. For example, the authors mention that bacteria account for a peak of 30.5% of the 13C within sediment OC, but that this corresponds to 13.8% of the fixed 13C. Fig. 3 shows the % of fixed 13C, but there is not currently a figure or table describing the % of the 13C within sediment OC. I realize that these numbers can be estimated by comparing separate bars on Fig. 3, but I think these numbers would be helpful to have in an easy-to-see format. I do not think a similar table is necessarily needed for Fig. 4, as it is very easy to compare the components of the C budget for each time point. –The inclusion of the results of the 2-G modeling in Fig. 4 is very interesting. It looks like the loss of 13C due to resuspension was substantial as a result of the high-flow event on day 9. Comparison between 13C in sediments on Day 4 and 11 indicates a loss relative to what would have been expected simply based on the model. I think mentioning this in the results and discussion is warranted, as it shows

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clearly how important these scouring/storm events are in this system. I also think this is important to consider when the authors discuss the “blue carbon” potential for these bare sediments.

p. 19790, line 8 Separate the phrase “or  $^{13}\text{C}$ -depleted” either by commas or parentheses.

p. 19791, line 26 in the text “. . .had been transferred from MPB within 12h,” does this mean transferred to bacteria or to the DOC (EPS) pool?

p. 19792, line 21-24 Is there a particular reason why resuspension was likely higher for this study than the Oakes et al. 2012 study? Is this a site difference, a subtidal vs. intertidal difference, perhaps a result of the big freshwater flow event that occurred during the current experiment?

Table 1 Include number of replicates represented by the mean and standard errors.

Fig. 3 –Include number of replicates represented by the mean and standard errors. –The time points do not match up exactly with what was described in the methods and elsewhere in the text. For example, day 11 in the figure presumably corresponds to day 10, which was mentioned in the methods and on p. 19791, line 4.

Fig. 4 Include number of replicates represented by the mean and standard errors.

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