We thank the anonymous referee for the time they have taken to carefully and thoroughly review our manuscript. Their comments have helped to clarify our thoughts and have assisted us to improve the manuscript.

Our specific responses to each of the referee's comments are detailed below. The referee's original comments have been copied below, and our responses inserted in bold text. Page and line numbers refer to the original manuscript.

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 13C-labeled HCO3- 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, 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 13C fixed by MPB, ~ 70% was lost from the sediments via resuspension. Of the remaining ^{13}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.

We thank the referee for these positive comments.

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?

This is a good point, but unfortunately the suggested additional information is not available for our system. We have noted the potential for scouring to limit longer-term carbon storage, however (section 4.4, end of first paragraph and section 4.3, end of third paragraph).

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.

Incubation of cores delayed collection of sediment samples by >5h. We therefore collected additional cores for immediate sampling of sediment compartments at the earliest sampling periods (0.5 and 1 d), when we expected to see the most rapid changes in ^{13}C distribution among sediment compartments. This is now described in the first paragraph of section 2.3.

–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.

Following exposed incubation, sediment was allowed to slide back down within the core sleeve in preparation for inundated incubations. This has now been clarified in section 2.3.

Section 2.5 Calculations

–Here, the authors describe how natural abundance $\delta^{13}C$ 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?

We have added this detail to the figure caption, as suggested.

–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?

We have now included a description of how biomass estimates of bacteria and MPB change with likely variations in PLFA composition (section 2.5).

–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?

This was done because we were interested in how much ^{13}C remained in the sediment, i.e. the loss of ^{13}C from total sediment organic carbon (measured at 0-10cm). This has been clarified in section 2.5, paragraph 4).

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.

We have corrected an error in the text. However, the peak occurred on day 11, not day 2. Figure 3 (now figure 2 in the revised manuscript) shows the % of ^{13}C in bacteria as a proportion of the ^{13}C that was initially fixed by MPB, whereas this sentence refers to the proportion of the ^{13}C remaining within sediment OC that was within bacteria at this time. We have changed the wording here to read "30.5% of the ^{13}C *remaining* within sediment OC", which is the same terminology used elsewhere in the manuscript, to make this more clear. The inclusion of additional plots in figure 3 (now fig. 2 in revised manuscript) to show supporting data (^{13}C within compartments as a proportion of the ^{13}C remaining in sediment OC) should make this clearer.

–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.

We have included additional plots on figure 3 (now fig. 2 in revised manuscript) that show the same data as a % of the ^{13}C remaining within sediment OC at each time. We have referred to this figure, as necessary, to support the text. This should make the results section easier to follow as there is now a clear visual distinction between these sets of data (% remaining ^{13}C vs % initially incorporated ^{13}C).

–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 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.
Discussion of this has been added (pg 19793, ln 26-30).

p. 19790, line 8 Separate the phrase "or 13C-depleted" either by commas or parentheses.
This has been corrected.

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?
This has been clarified (bacteria and/or DOC).

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?
We have outlined possible causes of this difference in resuspension on pg 19792 ln22.

Table 1 Include number of replicates represented by the mean and standard errors.
This is now included in the figure caption.

Fig. 3
–Include number of replicates represented by the mean and standard errors.
This is now included in the figure caption.

–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.
The times in the figures are times of sample collection, allowing for incubation. We have altered the figure captions to explain this, and the text of the manuscript has been checked and corrected.

Fig. 4 Include number of replicates represented by the mean and standard errors.
This is now included in the figure caption.