

## ***Interactive comment on “Patterns of carbon processing at the seafloor: the role of faunal and microbial communities in moderating carbon flows” by C. Woulds et al.***

**Anonymous Referee #2**

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Woulds et al. present data and discussion on two pulse-chase  $^{13}\text{C}$ -labeled organic matter experiments conducted on sediments cores taken from two estuarine sediment types in Scotland. The goal of the experiments was to investigate the flow of freshly deposited organic matter through the benthic food web. These experiments follow in the footsteps of a limited number of similar pulse-chase experiments that have been conducted throughout the last decade or so. It should be clear that the two experiments described here in this study, while similar in nature, are not directly comparable (e.g. differing coring diameters, different seasons, etc..). Nevertheless, the comparison of two types of estuarine benthic environments yields insights into the “processing” of freshly deposited organic matter.

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A major strength of this manuscript is the comparison made between the sites investigated in this study and previous studies of “biological C processing”. As the authors point out, there are all sorts of caveats to be attached to such experiments, and reproducibility is certainly bound to be one of the problems. Only through repeated experimentation with a broadly similar approach does a general picture begin to emerge. Overall, this manuscript represents a good contribution to the steadily expanding range of data and results from such experiments.

The first half of the manuscript through the Results section is generally well-written. Some issues need to be addressed, and these are noted below. In my opinion, however, the Discussion could be easily halved. For instance, the discussion of BGE (lines 520 to 545) far exceeds the data supporting any conclusions. Or see my comments below on Section 4.2 and the paragraph starting at Line 493. Overall, a more focused discussion would greatly benefit the overall impact of the manuscript.

Specific comments:

1. Line 73: It might be worth pointing out what does biological C processing not cover. Is there non-biological C processing in these systems? It might be worth pointing out the differences.
2. Line 76: A quibble: Stable isotope tracer experiments are an excellent tool, but not ideal. For instance, radiotracer  $^{14}\text{C}$  incubations are far more sensitive and do not depend on sorting out mass of naturally occurring background tracer distribution.
3. Line 117 and following: Independent of the food-web tracer studies, it would be nice to have some information on the relative benthic biomasses for these two sediment types, e.g. muddy and sandy bottoms. I would be surprised if muddy bottoms actually supported more faunal biomass.
4. With the exception of the respiration measurements, these are single endpoint experiments. Dynamics between the pools are not necessarily accessible.

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5. Line 124: "Recent findings" is relative; dynamic biogeochemical cycling in low OC permeable sediments has been extensively documented over the last two decades.
6. Line 171: Please describe more carefully the labeled phytodetritus in more detail. Was it composed of a single species and what? Was it prepared in the same fashion for both sites? What was it composed of? How fresh was it? Was it added as fresh or freeze-dried material.
7. Does the difference between the labeling percentages (ca. 25% and 34%) for the two sites reflect different batch preparations, or differing compositions of phytodetritus?
8. Methods: It's not entirely clear to me that total bulk  $^{13}\text{C}$  of the sediment was determined (i.e. total Corg  $^{13}\text{C}$ ). This must have been done in order to calculate the recoveries of tracers shown in Figure 2.
9. Is there a time zero sample, i.e. samples taken from one core immediately after the addition of the  $^{13}\text{C}$ -labeled phytodetritus?
10. Line 244 and following: It is not really clear to me why the authors work with the  $\delta$  notation for these type of experiments. There is also no obvious connection from how they go from Equation 2 to Equation 3, the latter of which is the more relevant for this manuscript.
11. Calculations with exceedingly large enrichments, for instance as seen in the macrofaunal biomass (lines 290 and following), become inaccurate.
12. Line 280: ...or as dissolved organic carbon.
13. Section 3.1: It might be helpful for the reader to plot the remineralization data over the time course of the experiment.
14. Section 4.2: This whole discussion is rather contradictory. On one hand the authors claim that the temperature and organic C loading are similar (line 384), but then suggest that temperature plays a larger role than biomass or organic C (line 395) does

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not make sense. Furthermore, there are no proper controls for assessing any of these factors. I would drop this whole discussion (see further discussion about curtailing discussion) and the conclusions regarding temperature (line 571). This was not the point of the study, it was not properly assessed, nor is it supported by the data.

15. Line 494: "This hypothesis. . ." Which hypothesis? From this paper or Wouldts et al. 2009? Actually I find the whole discussion of hypotheses, both here and earlier in the manuscript (line 134 and following) a bit specious. I think that it is enough for the authors to state that they are comparing two types of sites that are thus far lacking from the overall range of sites on which such experiments have been performed.

16. Figure 2: I assume that "Total" stand for the sum of respiration, bacterial uptake, etc.. in that case, there is also no Total for the % of Biologically Processed C. It might also be interesting to add a third panel to include total initial pool size of each of the separate pools (i.e. how much C is in each pool originally).

17. Figures 5 and 6: These figures are quite compelling, although I think that they could be combined. It is not entirely obvious why there are two separate figures.

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