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Interactive comment on "Benthic C fixation and cycling in diffuse hydrothermal and background sediments in the Bransfield Strait, Antarctica" *by* Clare Woulds et al.

Anonymous Referee #2

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The authors evaluated importance of two C sources to benthic community at sedimented hydrothermal vents; one as photosynthetic C and the others as chemosynthetic C. They conducted 13C-incubation experiments onboard using cores obtained from 3 different stations from the active venting site to the reference (normal sediment) site. The results somewhat contradict to the author's (and readers) expectation, that the chemosynthetic OM production was extremely low at the active venting sites but higher at non-vent site. Vent characteristic polychaetes Siboglinid including symbionts also did not exhibit any sign of OM production thorough 13C-bicarbonate.

As the authors also mentioned in the text, they carried out the experiment at bottom

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water temperature, while the sedimented venting sites should have high temperature at the subsurface sediments. Furthermore, more importantly, chemoautotrophic activities at the sedimented venting site must be supported by the reducing compounds contained in the venting fluid. However, the incubation experiments did not have this supply from the deeper part of the sediments. I indeed agree that it is extremely difficult to simulate the venting fluid from the bottom in the laboratory, but without that supply, the measured chemoautotrophy should have decreased dramatically because chemoautotrophic C production at the vent is supported by both oxic seawater and reducing venting (or seepage) fluid. I therefore think the measured C production rates using bicarbonate did not reflect that of in situ.

I think the manuscript should focus only on the importance of phytodetritus C consumption at normal and sedimented hydrothermal vent sites. Even though there were only 2 replicate cores at each site (and have high variations among a site, making it difficult for proper interpretations), it may be worth to report because there is no such experiment performed at hydrothermal vent area.

I noted some specific comments below.

Line 57 to 62 It is better to mention about the time scale, because C uptake by fauna will also be respired into CO2 in longer time scale.

Line 93 Middle sister is not described in Fig 1 (Three Sisters is described). Off-vent is also not described in Fig 1, but Off-Axis control is described. Please be consistent through the text and the figures.

Table 1 The authors listed the water temperature of each site, but do you have a data for characterizing each site in terms of venting activities, such as heat flow value, H+ or CH4 or Cl concentration of pore water? Can you list up some from Aquilina et al. 2013??

Line 115 (If the authors decided not to delete Chemoautotrophic C production results)

To give better idea how much 13C- and 15N were dosed into existing DIC or ammonium, it is needed to indicate them uM. In the line 158, the authors mentioned that the added 13C-DIC account for 22%, but this must differ between sites because venting fluid contains high DIC (5-100mM) than bottom water (∼2.1mM).

Line 151 It is totally unclear which depths did authors use for each analysis. For PUFA, all sediment layers were used or not? For faunal, the authors examined 10 cm or deeper?

Line 173 More specifically, how much 13C-labeling was determined as detection limits considering natural variations in d13C values? This must be written in M&M.

Line 188 Again, how much 13C-enrichments were regarded as 13C uptake? "Measurable uptake" sounds like even 1 per mil of d13C differences from background are regarded as uptake.

Line 206 Please describe this for more detail. Not only analytical precision, but also variations in background samples in replicate (if available) must be considered, which sometimes shows ∼5 per mil of variation.

Line 213 If you measured 13C of PLFAs in different layers, it is worth to put vertical trends in the graphs.

Lines 250 and 252 Probably, "mg" is missing.

Line 273 It is odd that describing "standard deviation" on samples with 2 replicates.

Figure 7 (If the authors decided not to delete Chemoautotrophic C production results) It is a bit confusing to show these graphs together; one with "respiration" but one without. I understand that the respiration of B cannot be measured because of 13C-DIC addition, but you need to mention that clearly in the caption.

Line 325 The differences in microbial biomass were less than twice, while those in respiration rates differed ∼7 times. So, the microbial biomass is not the only reason.

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The authors need to discuss about these fact more carefully.

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