

# ***Interactive comment on “Sink or link? The bacterial role in benthic carbon cycling in the Arabian sea oxygen minimum zone” by L. Pozzato et al.***

## **Anonymous Referee #2**

Received and published: 2 August 2013

Referee comment: Pozzato et al: Sink of link? The bacterial role in benthic carbon cycling in the Arabian sea oxygen minimum zone

This study investigates the carbon transfer between bacteria, protists and metazoans including meiofaunal and macrofaunal organisms using ex situ incubations with amendments of  $^{13}\text{C}$  labelled DOM and POM. Thereby this study addresses the question whether, as has been widely assumed, there is trophic transfer of bacterial carbon to higher trophic levels i.e. the existence of a microbial loop. Based on sediment incubations that lasted for 7 days from two different sites, one within the Arabian Sea OMZ (STOMZ) and one outside the OMZ (StoutOMZ) it was found that during incubations where  $^{13}\text{C}$  labelled DOM was offered there was no significant transfer of bacterial-

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derived carbon to metazoan consumers at both sites. In contrast during incubations with  $^{13}\text{C}$  labelled POM, this organic carbon was strongly assimilated from metazoans at STOMZ. However, at StoutOMZ  $^{13}\text{C}$  labelled POM was only to a small extent assimilated and only by bacteria.

The authors concluded that the bacterial loop at both sites is rather inefficient. Whereas DOM uptake is almost entirely restricted to bacteria, metazoans that directly consume labile particulate organic matter are supposed to compete with bacteria for phytodetritus.

I was reading this study with great interest and think it is well suited and within the scope of Biogeosciences. It is well written, clearly structured and represent the data in an appropriate way.

Beside the rather technical comments given below I discovered some aspects that should be considered and at least briefly addressed in the discussion section:

1. First of all this study is in parts very close to the study by Pozzato et al. (2013) where similar data from the same sites were presented. I guess that the cores incubated in this study originate from the same multiple corer cast as those treated by Pozzato et al. 2013. This provides the opportunity to broaden this study a bit by at least comparing the data between the two different studies – it might even be considered to include some of the data of the previous study when their origin is clearly indicated. This has not been done although treatments were almost identical in both studies. Questions could address the variability of biomass and POM tracer uptake at the two different sites. In case there would be strong variability, how would that affect the conclusions made in this study? 2. During this study the  $\delta^{13}\text{C}$  and presumably  $\delta^{15}\text{N}$  background values were determined from the different faunal groups but not shown in the results section. These measurements are very valuable and I wonder whether these data could be included in this study? Are there any trophic relationships to discern? 3. During this study as well as in the study by Pozzato et al. (2013)

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there was a major difference in the  $^{13}\text{C}$  labelled POM and DOM tracer incorporation between the station inside and outside the OMZ. This was hardly discussed and might be considered in more detail. Do the authors have any idea, why is there such a pronounced difference? 4. Another issue that I suggest to briefly discuss is the length of the incubations. Witte et al. (2003) covered three different time periods during their in situ incubations, where it became apparent that the macrobenthos immediately (2.5 days) responded to the input of labelled POM. In contrast, the response of the bacteria, which need to extra-cellularly hydrolyse particulate organic matter was retarded. This brings me to the point whether the time period of the incubation used in this study (7 days) is sufficiently long to detect the different processes involved in the carbon transfer between the different groups. This is not to question the results presented in this study but I think this issue should at least critically be considered.

The authors conclude that the DOM pathway is limited to bacterial exploitation, since no other group showed any uptake of this resource, and no transfer of DOM-derived carbon via bacterial grazing is observed, thus making this processing route a dead end. Generally, I think that the study presents interesting results to understand carbon cycling and transfer in marine sediments but nevertheless I have the feeling that the conclusions made are only valid for the experimental conditions chosen in this study hence I suggest to be careful with generalizations. There are certainly many factors affecting carbon transfer and its efficiency between the different trophic groups e.g. lability of organic matter, or what if organic matter becomes limiting.

Brief comments:

P 10400 L20: “These results, although very informative, left one question unanswered: is organic matter assimilated directly by meio- and macrofauna feeding on phytodetritus, or indirectly via ingestion of bacteria that in turn hydrolyzed particulate organic matter (POM) into dissolved organic matter (DOM) and subsequently incorporated it?” This is not entirely true as Witte et al. 2003 at least indicated that labeled organic material fastly was incorporated into macrobenthic organisms while Bacteria (PLFA)

showed a time lack for incorporation of the labelled substrate

Section 2.4 Sediment characteristics: This title is somehow misleading, one really has to read very carefully to understand that these sediments were also used for background determination of the  $\delta^{13}\text{C}$  of the fauna. I recommend to already integrate these background measurements in the title. I assume that you also measured PLFA and total lipids in these samples. If so please indicate.

P 10413 Line 28: I am not sure whether it should be generalized beyond the presented study that POM uptake proceeds via ingestion of phytodetritus rather than ingestion of bacterial biomass.

P 10414 L2: the observation that "in the POM treatment, the  $^{13}\text{C}$  values of fauna are much higher than those of bacteria (Table 2), ... further supporting that tracer incorporation occurred via direct substrate ingestion" was also made by Witte et al. 2003, see comment above.

P 10400 Line 4: please delete "aquatic" it somehow doubles pelagic

P 10401 Line 17: conditions instead of condition

P10401 Line 17: "She found that biomass and biodiversity of bacterial grazers may explain part of the differences in carbon preservation ... " for a reader who does not know this particular paper it is difficult to follow of what is meant here. Suggest to better specify and clarify this sentence.

P10401 Line 25: please specify which particular meiobenthic organisms are addressed in the studies of Nomaki and Guilini.

P10402 Line 17: "Instead, community functioning and efficiency are more likely to be key factors in determining such phenomenon." It not really clear what is meant with this sentence and should be a bit more elaborated.

P10403 Line 2: Whats about DOM uptake by nematodes?

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P10403 Line 19: It would be great if oxygen concentrations could be provided to explain the distribution of the OMZ

P10406 Line 6: please delete “manual” after “. . . standard procedures”

P10408 Line 6: Linopherus sp is also a polychaete, why did you separate here?

P10410 Line 15: Please indicate that you mean integrated biomass values over the upper 0-4 cm.

Table 1 provides the same data as have been already provided in Pozzato et al. 2013 but does not refer to this study, it is also not referred to this previous study in the results section. Please make clear to what extent data presented here were already presented in your previous study (Pozzato et al. 2013). Did the cores used for the incubations originate from the same multiple corer cast as used for the oxygen experiments described in Pozzato et al. (2013)?

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**BGD**

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