

## ***Interactive comment on “Uptake of phytodetritus by benthic foraminifera under oxygen depletion at the Indian Margin (Arabian Sea)” by A. J. Enge et al.***

**Anonymous Referee #3**

Received and published: 9 December 2013

The authors describe a foraminifera community from a push core collected in the center of an OMZ along the Indian margin, and assess the uptake of fresh labeled algae into foraminifera cytoplasm. This kind of work is very interesting from a C-cycling and N-cycling point of view, but not necessarily that novel in the sense that similar studies along the Pakistan margin, and studies on the role of metazoan macrofauna in C and N-cycling in this area have already been carried out. While this is not necessarily a reason for rejecting the paper, I feel that the study is very limited in that only a single 7cm core was collected and all of the results come from a 1cm slice from this core. For this reason, and the fact that I think the authors have over interpreted most of the findings and made large generalizations from this single core (e.g., first line of the con-

C6693

clusions paragraph), I cannot accept this paper in its present form. I commend the authors for attempting this type of study as deep-sea experiments are notoriously difficult to carry out, and the amount of work that has gone into sorting and identifying the foraminifera is admirable. However, I would have liked to see at least all 3 of the cores sampled and processed, with maybe less impetus placed on identifying every single protist. I think that one way to overcome the enormous limitation of the experimental design would be to start putting the experiment into context. You have an n of 1 and this 1 sample was only sampled to 1cm depth. This must first be acknowledged and the authors should discuss the pitfalls and limitations of this. In no way should the authors start making generalizations that these results show that foraminifera are strongly involved in the cycling of C in the core of the Indian margin OMZ. While I agree that this may be the case, the results only suggest this (at best). I found it strange that no mention was made of the potential artifacts that can result when fresh algal material is added. Would it not have been better to have added material that was aged in some way? I completely understand that OMZ's are carbon maximum zones and the seafloor will be receiving less degraded material than seafloor environments under oxygenated water bodies, but what they do receive will be aged to some extent. This may be a cause behind some of the strange responses exhibited by some of the forams with a high biomass (e.g., *Bolivina* aff. *B. dilata*) that appeared to not play a dominant role in C-uptake. The authors state often that “relaxed predation pressure and food competition through the absence of macrofauna, as well as metabolic adaptations to anoxia allow foraminiferal species to take up fresh phytodetritus in amounts larger than at the Pakistan margin OMZ sites”. I found these statements difficult to follow as none of the experiments carried out in this study showed relaxed predation pressure and food competition ultimately results in more uptake of material. If the authors really want to show and discuss the above, I would collect all of the available data from foraminifera C-cycling experiments and run a meta-analysis using macrofauna biomass and bottom O<sub>2</sub> concentration as dependent factors to test this. Without this and with the limited sample size reported here, this can only be hypothesized. One other thing in the paper

C6694

that confused me was the repetition in the discussion. I think much of this could be removed, which would ultimately lead to a shorter paper. I believe this would be more appropriate for the type of data presented. In conclusion, I cannot accept this paper in its present form. To be able to accept the paper I would like to see that authors run a meta-analysis using their data and that of others to show how the role of foraminifera in nutrient cycling changes as a function of faunal biomass and O<sub>2</sub> concentration. I really believe that this would strengthen the paper significantly. Moreover, the limitations of the data should be highlighted and all generalizations that cannot be made with the resulting dataset should be removed.

---

Interactive comment on Biogeosciences Discuss., 10, 15305, 2013.

C6695