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Comment

***Interactive comment on “Uptake of algal carbon and the synthesis of an “essential” fatty acid by *Uvigerina* ex. gr. *semiornata* (Foraminifera) within the Pakistan margin oxygen minimum zone: evidence from fatty acid biomarker and  $^{13}\text{C}$  tracer experiments” by K. E. Larkin et al.***

**K. E. Larkin et al.**

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Dear Reviewer. Thank you for your positive remarks and for your constructive comments, which we have responded to below.

An interesting manuscript on the role of *Uvigerina* ex. gr. *semiornata* in the cycling of fresh POC at the seafloor under low oxygen, and potential de novo EFA synthesis in this species.

C2004

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The ms confirms the central role foraminifera can play in OM cycling at the OMZ impacted sea floor. This in itself is not new, but data are still sparse and therefore well worth publishing.

The introduction is written well and gives a clear and concise overview of the state of the art and research question. At the same time, I felt some parts of the manuscript might benefit from modification/ clarification.

Comment 1. In particular, I feel the authors are perhaps a bit overenthusiastic in their choice of a manuscript title, that claims to present evidence for the de novo synthesis of EFA by the foraminifera under study. While the data presented support this hypothesis, I do not believe they present conclusive evidence and in the discussion the authors themselves acknowledge this and tread far more carefully.

Response 1. Given the low levels of 20:4(n-6) in the diatoms, yet high levels in the foraminifera at the end of the incubations, we feel it is highly likely that the foraminifera are indeed synthesising this essential fatty acid. However, we agree that the evidence is not conclusive. To address this concern, we have made the following changes. 1) The word 'likely' is inserted into the title ('likely synthesis'), 2) The end of the abstract is modified as follows - 'In addition, levels of 20:4(n-6), a PUFA only present in low amounts in the diatom food, increased dramatically in the foraminifera during both the in situ and shipboard experiments, possibly because it was synthesised de novo. This 'essential fatty acid' is often abundant in benthic fauna yet its origins and function have remained unclear. If *U. ex. gr. semiornata* is capable of de novo synthesis of 20:4(n-6), then it represents a potentially major source of this dietary nutrient in benthic food webs.' 3) The word 'possibly' is included in the Conclusions - 'Evidence presented here suggests that foraminifera could possibly be a major source of 20:4(n-6) in benthic ecosystems...' (line 490). We feel that the Discussion already treads fairly cautiously - 'Given the scale of the 10-fold increase in the amount of 20:4(n-6) observed in our shipboard experiments, we believe that this (i.e. de novo synthesis) is the most likely explanation' (lines 460-462). .

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Comment 2. It appears that different amounts of  $^{13}\text{C}$  labelled detritus (C per  $\text{m}^2$ ) were added in the ex situ and in situ experiments. What was the rationale for this ? It seems to unnecessarily complicate comparison between the two sets of experiments ?

Response 2. Unfortunately, the rationale for the unequal density of detritus (in terms of carbon per  $\text{m}^2$ ) added to the shipboard and lander experiments, which were conducted more than a decade ago, is no longer clear to us. The absolute amounts of detritus added to the lander chamber were obviously scaled up to compensate for the much greater surface area of the lander chamber ( $900 \text{ cm}^2$ ) compared to the megacores ( $78.5 \text{ cm}^2$ ). However, as the reviewer correctly points out, the density of added carbon was higher in the cores than in the lander chamber ( $685 \text{ vs } 361 \text{ mg C m}^{-2}$ ). We believe that the most likely explanation for this is that limited supplies of detritus available on the ship meant that there was not enough to add to the lander in order to match the density in the much smaller megacores.

Comment 3. With regard to the single in situ incubation performed, the term 'time series' does not seem adequate and should be deleted.

Response 3. Agreed, Time series removed.

Comment 4. Also, with only one chamber experiment performed, where did the 2nd in situ replicate come from (I15 p 260) ? Where both cores taken from the same experimental chamber ? If so this should be clearly stated.

Response 4. Both cores came from the same experimental chamber and the text has been amended to explain this (line 230).

Comment 5. At such low oxygen concentrations, an accurate maintenance of DO is central to the experimental design. It needs to be explained how oxygen concentration was maintained in the in situ experiment – at such low DO, the lander chamber would have become anoxic quickly without regulation, and maintenance of ambient DO with a passive, gradient-driven system is not easy due to the low ambient DO and hence

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shallow gradient. It is thus particularly important to either include or refer to the O<sub>2</sub> time series readings from the lander chamber (the paper cited in this context is a broad overview paper that does not give any details).

Response 5. We've expanded the description of the system slightly (lines 227-229) and referred to the Woulds et al. (2007) paper, rather than Cowie and Levin (2009),

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Interactive comment on Biogeosciences Discuss., 11, 251, 2014.

## BGD

11, C2004–C2007, 2014

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