

Interactive comment on “Foraminiferal survival after long term experimentally induced anoxia” by D. Langlet et al.

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Dear editor, Dear referee, we hereby provide you with our detailed answers to your remarks on our manuscript. Our answers to your remarks can be found below.

#Anonymous Referee 2 - This paper offers very valuable results about the long term adaptability of benthic foraminifera to anoxia. The field and laboratory methodologies in use are all best practice (*arte legis*), and the working teams involved range among the very experienced specialists. Tables and figures are clearly presented, captions are adequate (perhaps the abbreviation OTU (fig. 4) might be written out for clarity).

Authors' response: Figure 4 caption will be changed to mention the meaning of the abbreviation OTU (Operational Taxonomy Unit).

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#AR2 - The results leave no doubt that benthic foraminifera may adapt to anoxic conditions much longer than formerly assumed, that anoxia and sulfidic conditions do not rise an impassable environmental threshold at all, and that such long term conditions even may provide beneficial conditions for a number of species. This is hooked to a second paper already accepted by Biogeosciences. All in all, this structures highly interesting results for meio-benthologists as well as for micro-paleontologists, especially in the light of rising ocean surface temperatures and expanding oxygen minimum zones. Scientific significance and quality are excellent. Suggestions for improvements remain meager, therefore. However, some parts of the discussions only range from good to fair. It should have been mentioned that specific benthic foraminifera were found living even under constant anoxia.

Authors' response: the fact that numerous individuals of *Virgulina fragilis* were found Rose Bengal stained in the anoxic mud belt off Namibia (work of Leiter and Altenbach) will be added to the introduction and to the discussion.

#AR2 - In addition, a number of indications could have been mentioned about specific intracellular organelles, kleptoplasts and prokaryotic symbionts which all may be reasonable for the results presented (mainly works of Joan Bernhard).

Authors' response: Several indications on the role of intracellular organelles and symbionts on the foraminiferal metabolism will be added to the paragraph 4.3.2

#AR2 - Rose Bengal staining or ATP - measurements are neither 'right' nor 'wrong', nor good or bad, they fail if applied inappropriate or if their interpretation follows erratic suppositions; this is hardly made visible for a reader within three or four sentences of discussion.

Authors' response: We do not fully agree with this remark. Protoplasm may be preserved for a long time in anoxic sediments and consequently, foraminifera which have been dead for a long time will still stain. This is not the case for the CTG method. However, we do agree with the reviewer that even the CTG method may have some caveats

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(e.g., bacterial takeover), and that both methods have to be applied very critically in order to obtain the best possible results. However, we are convinced that these “optimal” results are much more accurate for CTG than for RB. RB also fails in anoxic conditions when applied appropriately. We changed some passages in the text accordingly.

#AR2 - It seems worthwhile and much more appropriate to quote possible key features for their anoxic survival within the discussion, and to omit assessments which can not be covered in detail. Another fact to wonder about is the more or less stable numbers when modeling the standing stocks. At comparable water depth, long term in situ observations along the British Coast (John Murray) and in the Baltic Sea (Lutze/Wefer) recover clear ups and downs of standing stocks, sometimes by orders of magnitude within weeks, mainly committed to seasonal influences, primary productivity, and reproduction. Might be helpful to define the more stable situation at the Adriatic Sea.

Authors' response: Since the samples of the 4 periods were taken in different chambers, the densities cannot be compared without considering the possibility of substantial spatial patchiness. Therefore, in our interpretation, only major changes, of about 1 order of magnitude, were considered as due to the experimental treatment. The reviewer appears surprised by the small differences between the densities of some of the treatments. Nevertheless, our 1 month chamber shows a very clear increased density, which cannot be due to natural environmental changes (which, except for temperature have been ruled out in our experimental setup). Like in the areas indicated by the reviewer, also in the Northern Adriatic, the natural faunas show important seasonal variability, as we indicated in paragraph 4.4.1. However, as we indicated as well, natural phenomena cannot explain the changes (or lack of variability) observed in our benthic chambers.

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