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Interactive comment on "Living (Rose Bengal stained) benthic foraminiferal faunas along a strong bottom-water oxygen gradient on the Indian margin (Arabian Sea)" *by* C. Caulle et al.

Anonymous Referee #3

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The paper by Caulle et al presents data on living (Rose Bengal stained) benthic foraminifera across an OMZ bottom water gradient on the Indian Margin. The paper is generally well constructed and data is well organized. However, I do not agree with the main conclusions that the authors make here. The authors suggest (abstract lines 14-15 and conlusion) that the foraminiferal assemblage and the dominance of agglutinated foraminifera are linked to relatively low surface water productivity and associate lower Corg flux at the area. The authors base this argument on satellite productivity estimates, which indeed show lower productivity in the study area in comparison to the other OMZ areas in the Arabian Sea. Yet, they have no actual flux data and the sedimentary Corg content and amino acid index seems to suggest otherwise, showing

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relatively high values in the OMZ. In addition, in the paper Cowie et al (2014), where organic geochemistry of the study region is discussed in detail, shows that the organic matter distribution at the Indian margin is tightly coupled to hydrodynamic processes and oxygen availability. Therefore, I suspect that the local hydrodynamic region coupled to BWO, and/or alternatively the carbonate chemistry and associated lower pH may restrict the distribution of larger calcareous foraminifera and favoring agglutinated taxa. The carbonate chemistry was also suggested to be a factor in Murray Ridge where agglutinated foraminifera were abundant in >125 um fraction (Caulle et al. BGD 10, 15257–15304, 2013). All statements about organic fluxes should be deleted, as there is no data to support them.

The study is also based on surficial sediments (0-1 cm) and >150 um fraction, although sediment was sampled down to 10 cm depth. I wonder why the deeper intervals were not investigated? Or alternatively, why did the authors did not investigate the smaller size fraction, if only the surficial sediments were studied? In the paper the authors state that the small fraction was not studied as it is very time consuming, yet this study has 5 stations, where only surficial sediments (10 samples in total) where studied. Furthermore, as authors state in section 4.1, this could cause a bias, as especially in low oxygen setting foraminifera are generally smaller. Furthermore, this size dependence seems to be especially the case for calcareous foraminifera as shown in the study of Caulle et al (2013) in the Arabian Sea OMZ. Similarly in the study of Schumacher et al (2007) agglutinated foraminifera are abundant at deeper sites (at similar water depths to this study) and calcareous foraminifera are only abundant in the small size class. I also do not agree that the small size class only consists of juveniles as authors suggest in section 4.1 (lines 28-29). Especially in low O2 settings, adults may not just grow as large due to environmental reasons. I would think this study would improve significantly if small size class would be examined as well. This could then potentially provide confidence to the current arguments, or alternatively lead to new different outcomes.

The abundance of agglutinated taxa in low oxygen setting is indeed interesting but I

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do not think this is totally new. The study of Pina-Ochoa et al. (2010) showed that at least some species of agglutinated foraminifera also collect nitrate. For Reophax no nitrate pool was measured but only very limited number of specimens was measured (4 in total). Thus further investigation is required. These parts of the discussion should be rewritten and work of Pina-Ochoa et al cited.

Other notes Census data should be presented as an appendix, and counts of all species shown even if not discussed in detail.

Authors also comment on the extreme low-oxygen content of the core of the Indian margin OMZ in context of species diversity and compare it to other studies in the region (this study 0.3 uM, around 2 uM Murray Ridge, around 4 uM Pakistan Margin; Section 4.2 lines 12-15; and Section 4.5). I indeed agree that the high diversity may be related to careful taxonomy of this study, and hence I would not place this much emphasis on the topic. Firstly, as the authors note in the start of section 2.1. core of OMZ is where O2 content is <2uM, this would then leave out the Pakistan margin complete. Secondly the position of the core of the OMZ is slightly different in different regions, for example, at Murray Ridge the shallowest OMZ core sample comes from over 800 m water depth unlike in this study where it is from 535m depth. Thirdly it may be relevant to take into account differences in the measuring techniques and what their O2 detection limits and errors may be. For example, sensors are continually developing. The unisense O2 detection limit now is 0.3 uM, although in the past there has been a problem especially at very low concentrations. Further other studies may have used other approaches like Winkler titration to measure bottom water O2 content.

p. 3250 lines 12 add weight % or wt. % in front of Corg content. Check everywhere for this as units are missing in other places too. p. 3250 line 6 you mean lower boundary of the OMZ core? p.3255 line 26 Caulle et al 2014 does show some infaunal foraminifera. For example at the OMZ core site highest abundances where at 1-2 cm depth in sediment. Also at the transition towards more oxygenated conditions foraminifera are present at relatively high numbers from 1-3 cm depth and at more oxygenated sites M.

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barleeanus is also found. p.3259 Lines 16-18. Why only mention dormancy? What about denitrification. Do authors have any data on bottom and pore water nitrate concentarions? p. 3261 line 24 you mean the OMZ core? Add OMZ for clarity. p. 3264 line 16. No E. trigona and C. oolina in Fig 7. Please check Figure is ok and contains all appropriate data.

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