Angers, 26 December 2013

Dear Editor,

Hereby we resubmit the revised version of the manuscript "Live foraminiferal faunas (Rose Bengal stained) from the northern Arabian Sea: links with bottom-water oxygenation" by Caulle et al.

We would like to acknowledge anonymous referees #1 and #2 for their comments and suggestions which much helped to improve the manuscript. We considered the comments with care and acted on the majority of the points raised by the reviewers. In a few cases we have decided not to follow the suggested changes. Below, please find a list of our answers to referees # 1 and #2, presented in the same order used in the review.

Thank you for considering our manuscript for publication in the Biogeosciences special issue on "Current biogeochemical and ecosystem research in the Northern Indian Ocean".

Yours sincerely,

Clémence Caulle (also on behalf of the co-authors)

Interactive comment on "Live foraminiferal faunas (Rose Bengal stained) from the northern Arabian Sea: links with bottom-water oxygenation" by et al.

Anonymous Referee #1

Received and published: 28 November 2013

Dear Dr Kitazato

Many thanks for giving me the opportunity to review the work by Caulle and co-workers: "Live foraminiferal faunas (Rose Bengal stained) from the northern Arabian Sea: links with bottom-water oxygenation", submitted to Biogeosciences. The work is based on a suite of multicores recovered from a transect in the northern Arabian Sea. The authors have studied the distribution of live (stained) foraminifera together with environmental variables from 10 stations, stations located along an oxygen concentration gradient. The sampling took place during the winter monsoon in 2009 and the foraminifera have been investigated in three different size fractions. Overall the paper is well written and well structured. The paper is well worth publishing in Biogeosciences, however, there are certain aspects which needs to be addressed.

Specific comments

Referee #1: I lack a clearly defined aim.

Why have the authors performed this study and what is the problem that they want to solve? Are they foremost interested in a paleoproxy calibration approach or in the biology of benthic foraminifera in an low oxygen environment. Both approaches can be easily justified but it requires a more detailed aim from the beginning. It will increase the interest for the paper and also something to go back to in the conclusions.

Author response: Our aims are listed on page 4 lines 104-111. We tried to be more specific:

- *Our study aims at providing better insight into foraminiferal ecology of the Arabian Sea.*
- *The study therefore focuses on: 1) describing the live benthic foraminiferal distribution (Rose Bengal stained) on the Murray Ridge, and 2) identifying the main controlling parameters for benthic foraminiferal faunas in this environment characterized by a gradient in oxygenation and high organic matter fluxes.*

The ultimate objective of this study, although not specifically mentioned, is to provide new elements for paleo-proxies based on benthic foraminiferal assemblages in the Arabian Sea. A process-base understanding of relationships between environmental parameters and benthic foraminiferal assemblage is essential for paleoceanographic reconstructions.

Referee #1: The authors mention in the beginning that the study is based on rose Bengal samples. This should be made clearer throughout the text.

Author response: The information of Rose Bengal stained foraminifera were used has been added on several places in the text.

Referee #1: I also recommend a more detailed discussion about why the authors choose to work on RB instead of CTG in this particular project.

Author response: We are aware of the potential problem involving the use of Rose Bengal and which could be avoided with the CTG technique. Our reasons to choose for the Rose Bengal are explained in the manuscript on page 5, lines 144-147.

*The main reasons for favoring this technique over CTG are: on one hand, it is much easier applicability, and on the other hand, and **more importantly**, to keep our results comparable to previous studies in the area based exclusively on Rose Bengal stained foraminifera (e.g. Maas, 2000; Jannink et al., 1998; Schumacher et al., 2007; Larkin and Gooday, 2009). A sentence has been added in the text on page 5, lines 144-147 to justify this choice*

Referee #1: I'm also curios why the authors choose to work on several different size classes, however, apparently didn't work extensively on the smallest fraction (>63 um), which is probably the most important one when it comes to a low oxygen environment.

Author response: We agree that the >63 um fraction is important as well. This is the reason why we investigated it despite the fact that it is extremely time consuming. This fraction was already described and discussed in the original manuscript, pages 10 to 12 and page 14 lines 410-419. However the reasons for focusing on the > 125 μm fraction in the discussion part are:

> *1- The study of the 63-125μm fraction is extremely time-consuming. A study of this fraction in all levels (of 10) and all cores is simply not possible in a reasonable time-span.*

> *2- The investigation of the smaller fraction did not show a large difference with the larger fraction that might bias the interpretations. Both size fractions show similar patterns. In fact, the >63 μm mostly contains juveniles of the larger fraction. The additional information provided by the >63μm fraction is described in detail and discussed.*

> *3- This decision is also based on our ultimate aim of contributing to paleo-proxy calibrations, since most paleoceanographic studies focus on the >150μm or >125μm fraction.*

Referee #1: The manuscript will benefit if it was stated from the beginning what was already published and what are new data.

Author response: In the original manuscript this is stated specifically in "Material and methods" and in the figures, what has already been published and what is new. However, a new sentence has been added to clarify this even further on page 3 lines 99-101.

Referee #1: I find the discussion about the saturation stages and dissolution most interesting and I think it would benefit the paper strongly if this part could be expanded and that the unpublished Reichert data (also a co-author on this paper) could be included in the manuscript. The authors discuss the planktonic forams but they have, of course, been transported through the water column, what about the benthic ones? Do the authors see a difference between the RB stained and the recently dead ones (i.e not stained in the upper most one cm)?

Author response: The unpublished Reichart data referred to in the manuscript are included in a manuscript submitted by Koho et al. to Earth and Planetary Sciences Journal. That manuscript has a strong geochemical focus. To discuss the possibility of dissolution and transport more completely, it would have been necessary to introduce some of these data, and to add figures. We think that our manuscript is already quite extensive and contains a lot of new information. Adding more would make it difficult to keep the focus. Hence we prefer to delete this part of the discussion alltogether.

We agree with the reviewers that planktonic foraminifera could have been laterally transported through the water column. We did not observe any signs of alteration in dead benthic faunas from the upper most sediment interval, suggesting an absence of dissolution. However, to avoid misunderstanding in the discussion, all test referring to planktonic foraminifera has been removed from the manuscript. Moreover, the use of planktonic foraminifera was somewhat subjective: we did not quantify the alteration in planktonic foraminifera. It would have been more robust to discuss the alteration of specific planktonic foraminifera species known to be sensitive to dissolution. But in our opinion, this would be completely outside of the scope of the present paper.

Referee #1: Conclusions need to be more firm and exact. What new knowledge and conclusions can we draw from this study? I recommend the authors to help the reader to better understand what do we know now that we didn't know before and how can we but this knowledge in a larger perspective? This should be coupled to the aim. (even if the results are that we couldn't link the forams to the environmental parameters, a negative result is also a result).

Author response: Conclusions have been restructured and better connected to the main objectives of the study.

Referee #1: Structure: I wouldn't use the word feel in a scientific text. I would assume that the authors base their reasoning around something a bit more logical and rational than feelings. The authors use a lot of abbreviations, which makes the text unnecessarily difficult to read. Example: instead of TTS use abundance or concentration.

Author response: The word feel has been omitted. Where possible, abbreviations have been replaced by complete names.

Referee #1: Already published work should be discussed in present tense, the authors new data should be discussed in past tense.

Author response: The suggested changes have been made.

Referee #1: There is a mix between discussion and results in the result section from the PCA, any interpretation needs to be moved to the discussion section.

Author response: Interpretations from the PCA section (3.4. Foraminiferal assemblages and relation to environmental parameters: statistical analysis) have been moved to the discussion.

Referee #1: Taxonomy : I suggest the authors to look at instructions for authors for a more micropal journal to learn how to write taxonomic names (the more senior authors could perhaps also kindly inform the younger ones). Eubuliminella exilis is now new name for Bulimina exilis accordingWoRMS data base. I think the authors would find the paper by Filipsson et al (2011) in J Q Sciences helpful when it comes to the B. exilis discussion.

Author response: We verified the validity and the writing of the species names used in the manuscript. In the specific case of Bulimina exilis, we prefer to use the genus known as Bulimina. However, it is specified in the taxonomic appendix that this species is now recognized by some authors as Eubilimininella exilis.

Referee #1: Statistics: The R2 values need to be reported with significance values. Any discussion about trends needs to be statistically tested incl. significance test.

Author response: p and R^2 values are added (see page 8 lines 236; 240).

Referee #1: Again, I stress that it is a good study and it is overall nicely written and based on a substantial amount of material. A bit more work and it will be a very good paper as well.

Response to comments on the manuscript (pdf comments)

Referee #1: I would turn this around and state right from the beginning why OMZs are important and what questions you want to solve and how you have done it.

Author response: Changed accordingly. We added a few sentences and clearly defined the aim of the study at the end of the introductory part (page 4 lines 104-111).

Referee #1: I'm surprised that you decided to work with rose bengal when we know that it has large limitations especially in a low oxygen environment.

Author response: See above for response.

Referee #1: p=?, significant at which level?

Author response: Added (see page 8 lines 236; 240).

Referee #1: Please remind us here that you only work with rose bengal stained samples.

Author response: Added

Referee #1: show, show, show. I suggest to use another verb, demonstrated, displayed.....

Author response: Changed

I'm a bit surprised that you didn't focus more on the >63 fraction since it is well known that especially for low oxygen environments the 63 fraction is of large importances.

Author response: See previous comment on this topic.

Referee #1: This is interpretation and should be moved to discussion. This whole paragraph needs a careful separation between what are the results from the PCA and what are interpretations.

Author response: Changed (see pages 12-13).

Referee #1: I wouldn't use the word feel in a scientific text. I would assume you base your reasoning around something more logical and rational. This also goes back to the question to why you have done this study, if you are interested in a paleoproxy calibration approach or if you are interested in the biology of benthic foraminifera in an low oxygen environment. Both approaches can be justified but it requires a more detailed aim from the beginning.

Author response: Changed (see page 12, line 381).

Referee #1: What do you refer to with any other? Is there one variable that it is linked to or do you mean that "do not demonstrate a significant (needs testing) relationship with any of the measured variables"). If you mention trend then you also need to test the significance.

Author response: Sentences changed (see page 13, lines 396-397).

Referee #1: please change this to present tense, others data is presented as the state of knowledge.

Author response: Sentences changed (see page 13, line 413).

Referee #1: please change this to present tense, others data is presented as the state of knowledge.

Author response: Sentences changed (see page 13, line 417).

Referee #1: Or that they are more adapted to low oxygen conditions. Both size adaption and a competition advantage to smaller size species. Or a combination of both.

Author response: We have added a few sentences about the possible relationship between size and low oxygen adaptation as suggested by the reviewer (see page 13, lines 423-427).

Referee #1: What is actually relatively fresh organic matter? Typical words to use in foram studies but what do we actually mean?

Author response: See Koho et al., 2013 for an organic matter quality discussion. We think that a detailed discussion here is outside the scope of the paper.

Referee #1: I would try to vary the use of show a bit. There are other words to use, demonstrate, indicate and so on.

Author response: Sentences changed (see page 16, lines 529-530).

Referee #1: I think you will find the paper by Filipsson et al., 2011 in Journal of Q Science, quite interesting in this discussion.

Author response: We added the paper in our discussion (see page 16, lines 543-545).

Referee #1: What is the most important new knowledge that this study has generated?

Author response: The conclusion has been rewritten (see page 18, line 717-743) in this scope. Our study provides new elements for a future paleo-proxy calibration. Three groups on foraminiferal assemblages were defined, reflecting different environmental conditions, and thereby the OMZ structure. Additionally, this is one of the first studies to report a high dominance of agglutinated foraminifera in the Arabian Sea OMZ.

Interactive comment on "Live foraminiferal faunas (Rose Bengal stained) from the northern Arabian Sea: links with bottom-water oxygenation" by Caulle et al.

Anonymous Referee #2

The manuscript presents new data on the species and microhabitat composition of live benthic foraminifera across strong biogeochemical gradients of the OMZ in the Arabian Sea. Major target is to explore the relation of benthic foraminifera to bottom water oxygen concentration and sediment biogeochemistry. Recent oceanographic observations revealed a significant expansion of oxygen minimum zones in the world oceans. This observation has been attributed to a weakening of global deep-water ventilation as response to global climate warming. Quantitative reconstructions based on proxy data are required in order to evaluate this observation in face of the natural climate and OMZ variability during the Holocene and to test the significance of model studies. The present study not only contributes to a better understanding of the ecology of deep-sea benthic foraminifers in OMZ ecosystems but also provides the first step for the development of a regional foraminifer-based transfer function for quantitative oxygen reconstructions. The study addresses relevant biogeochemical processes and biological and oceanographic interactions in OMZs. It is well suited for the journal Biogeosciences.

There are, however, several issues that should be considered by the authors prior to submission of the final version. The first issue addresses the present organization of the discussion chapter that appears rather descriptive and not very suitable to highlight the relevant processes. Therefore, I recommend re-organization of the discussion chapter, based on the relevant environmental processes (e.g. oxygenation of bottom and pore-water, quality and quantity of food supply, preservation potential of the different faunas etc.). The second issue addresses the statistical investigation of species environment relationships. In PCA, dependent and independent variables should not be mixed. Instead, canonical correspondence analysis (CCA) or redundancy analysis (RDA) should be applied for quantitative evaluation of the role of different environmental parameters on the species composition. The third issue is that some of the figures contain too much of information. Specifically, it is rather difficult to differentiate between the different colors and shadings in figures 4 and 5.

Author's general response to the review:
We are grateful to the referee for the constructive and useful comments. The discussion chapter has been strongly modified and condensed. Descriptive and repetitive sections were deleted. Sections about the environmental parameters and the vertical distribution were deleted and some parts were integrated into the discussion. As referee #2 suggests, we added more discussion about diversity indices. We agree with the reviewer concerning the use of PCA analyses. Therefore, we restricted the PCA analysis to species alone, and added CCA analyses to analyze relations between

species and geochemical data. Finally, we enlarged the scales on figures 4 and 5 to make them easier to read.

My specific comments and issues are specified below:

Referee ≠2: 1. Introduction: a) Line 71: you should also mention the influence of outflow waters from the Red Sea and Persian Gulf. b) Line 88: is should read correctly "processes".

Author response: The potential influence of outflow water from the Persian Gulf and Red Sea has been added in the introduction (see page XX lines XX)
"Process" was corrected into "processes"

Referee ≠2: 2. Material and Methods 2.2. Foraminiferal analyses a) Line 126: it should read correctly "except".

Author response: correction made

Referee ≠2: b) PCA: it appears problematic to include both independent and dependent parameters in a PCA. PCA should be applied in order to group species and/or samples. For evaluation of the species-environment-relationships you should rather use Canonical Correspondence Analysis (CCA) and/or Redundancy Analysis (RDA) according to the nature of the relationships (linear or unimodal). These techniques allow proper quantitative evaluation of species-environment relations.

Author response: The new PCA analysis was only carried out using the percentage of live benthic foraminifera (>2%). The new PCA defined the same three groups as in the previously presented version. These three groups were related to environmental parameters by Canonical Correspondance Analysis (CCA) (see page XX lines XX)

Referee ≠2: 3. Results 3.2. Foraminiferal assemblages of the 0-10 cm interval (>125 _m fraction) 3.2.2. Diversity and dominance Line 244-247: when compared with diversities of faunas from other regions, H(S) values of 2.3-2.5 still appear not very low, and values of 3.1-3.2 not particularly high. This should be kept in mind for the discussion chapter.

Author response: We added this point in the discussion.

Referee ≠2: 3.2.3. Distribution patterns of individual species Line 266: "mexicana" instead of "Mexicana"

Author response: correction made

Referee ≠2: 3.3. Foraminiferal faunas of the 63-125_m fraction (0-1 cm) 3.3.2. Faunal composition of the >63µm fraction (0-1cm level) Lines 328-330: it would be interesting to know which species of Nuttallides you have lumped together here because the different species of this genus exhibit significantly different ecological preferences.

Author response: We have now detailed the species of Nuttallides, which have been lumped together (page 12 lines 533 and 535)

Referee ≠2: 3.4. Foraminiferal assemblages and relation to environmental parameters: principal component analyses (PCA) As mentioned before, PCA is useful to define species and/or sample associations. However, dependent and independent variables should not be mixed. Therefore, PCA is not a proper tool for investigation of species environment- relationships.

Author response: see above for our response to this comment.

Referee ≠2: 4. Discussion General Comments:

Referee ≠2: a) Although the ecological discussion on the species level is correct and up-to-date, I recommend re-organization of this chapter focusing on the relevant environmental processes ("oxygen versus food quality and quantity") or novel aspects of your study ("implications for the development of a proxy for BWO reconstructions"). Such a re-organization would also help to reduce the more introductory text passages and avoid repetition of results description.

Author response: The ecological part of the discussion has been completed with the remarks on the vertical distribution, to avoid repetition. Most of the subtitles have been modified, to better match the content.

Referee ≠2: b) What do the observed diversities (H(S) between 2.3 and 3.2) tell you in terms of ecosystem stability or stress? How do your values compare to benthic foraminiferal diversities in other deep sea ecosystems?

Author response: we added a chapter on foraminiferal diversity (page 14) addressing the points indicated by referee #2.

c) A short subchapter on the preservation potential of your faunas would add relevant information to your study, particularly concerning the applicability of your ideas concerning a proxy for BWO reconstructions.

Author response: we did not add a subchapter about the preservation potential of the foraminiferal faunas. To discuss the preservation of the faunas a direct comparison with the dead faunas would be required which is beyond the scope of this manuscript.

Referee ≠2: 4.1. Environmental conditions and their implications to benthic foraminifera. This subchapter appears a bit too unspecific and has an introductory character. Therefore, this information would probably be better placed in the introduction chapter.

Author response: We agree with the reviewer, and the discussion has been reorganized accordingly. Subchapter 4.1 is now deleted. Some parts of old 4.1 have been moved to the introduction, whereas others parts were deleted.

Referee ≠2: 4.3. Dominance of agglutinated species along the study transect Lines 432-434: please rephrase the two sentences for better comprehensibility

Author response: correction made (see page 16, line 466)

Referee ≠2: Line 442: please rephrase "study setting"

Author response: correction made (see page 16, line 477).

Referee ≠2: 4.4. Species zonation 4.4.1. Taxa dominating the faunas in the core of the OMZ Line 480: shouldn't it read correctly "A. cretaceus" instead of "A. cretaceous"?

Author response: correction made (see page 17, line 521).

Referee ≠2: Line 505: "species" instead of "specie"

Author response: correction made (see page 18, line 528).

Referee ≠2: 4.4.2. Dominant taxa of the lower OMZ Lines 551-567: you argue that E. trigona may be endemic to the lower part of the OMZ in the Arabian Sea. In the Abstract, you write, however, that E. trigona is a cosmopolitan species. The latter may be correct because it has been reported from a variety of oceans, including the Pacific (Jones 1994, Challenger Foraminifers, Plate 55), Atlantic (Lohmann 1978, JFR 8, 6-34; Mead 1985, Micropal. 31, 221-248; Mackensen et al. 1993, Mar.Mic. 22, 33-69) and SE Indian Ocean (Corliss 1979, Micropal. 25, 1-19). 4.4.3. Taxa dominant at more oxygenated deeper sites Lines 588-589: you may consider here that your OM measurements only reflect a snapshot situation for the time of sampling. The absence of fresh organic matter in your measurements does not exclude the presence of fresh phytodetritus during other times of the year. Your samples have been taken in January but maximum productivity and maximum phytodetritus pulses would be expected during the SW monsoon season in summer and early fall. So the observation of E. exigua may still indicate pulses of fresh phytodetritus even though samples did not contain any fresh OM at time of sampling. Maybe you should add this point to the discussion.

Author response: The discussion of E. trigona has been modified. In the earlier manuscript we used the term endemic incorrectly. With the word "endemic" we meant to say that E. trigona is a

marker species for the lower OMZ, as is not found elsewhere. This point is now clarified in the discussion (see page 20, lines 609-617).

Referee ≠2: 4.5. Vertical distribution First paragraph: this paragraph is mainly a repetition of the description of results. This could be avoided by reorganization of the discussion chapter (see above).

Author response: this section has been deleted. Part of this section has been included in the species zonation discussion (4.4. Benthic foraminiferal succession along the study tansect: oxygen versus food quantity and quality). This way we avoid repetition in results and discussion sections.

Referee ≠2: 5. Conclusions Lines 664-667: problem of methodology (see above). Line 672: "assemblages."

Author response: Conclusions have been rewritten.

Referee ≠2: References Two references appear incomplete: Koho 2008 (lines 816-817), Levin 2003 (lines 838-839).

Author response: correction made (see page 28, lines 868 and 890).

Referee ≠2: Figures Fig. 1: overview map in a) appears too small. Bathymetric information is of rather coarse resolution and could be a bit more detailed. Bathymetric legend is missing.

Author response: We changed the overview map and the detailed map. Scales have been added to both maps.

Referee ≠2: Fig. 2: Character size appears too small and should be increased. Reference to Levin et al. (2003) should be given in the figure caption instead in the figure.

Author response: correction made

Referee ≠2: Fig. 3: Character size should be increased. Reference to Levin et al. (2003) should be given in the figure caption instead in the figure.

Author response: correction made

Referee ≠2: Fig. 4: Character size should be increased. The multitude of species and assigned colors/patterns plotted makes it difficult to read the figure, especially for taxa with low abundances. Therefore, either the number of displayed taxa should be reduced or information equalized, e.g. by creating an additional figure.

Author response: We changed the horizontal axes of the calcareous and agglutinated panels to present a better view of species bars.

Referee ≠2: Fig. 5: a) The multitude of species and assigned colors/patterns plotted makes it difficult to read the figure, especially concerning taxa with low abundances. Therefore, either the number of displayed taxa should be reduced or information equalized, e.g. by creating an additional figure.

Author response: The figure has been modified by differentiating between agglutinated and calcareous taxa in two graphs. This makes the figure hopefully easier to read. Same colors and patterns have been used as in Fig. 4.

Referee ≠2: Fig. 7: Character and symbol sizes should be increased.

Author response: correction made

I hope that my comments prove useful to the authors and help to optimize this nice and important study!

Author response: They surely do !