

We thank both reviewers for their effort to improve our manuscript. Below we give a point-by-point response to the reviews of both reviewers. The relevant changes in the manuscript can be tracked by the answers to the comments of the reviewers. All changes in the revised manuscript related to the reviewers' comments are indicated and written in italic letters.

## **Answers to the comments of Anonymus Reviewer 2:**

We thank the reviewer for his constructive comments and positive feedback. Since the main issue of this review was the balancing of some interpretations about our results we tried to mitigate these in our discussion and also considered the different approaches of interpretation suggested by the reviewer. The manuscript already was going through a so called "quick review" before it was published in Biogeosciences Discussions. This review is actually by word the same as this initial "quick review". Since the reviewer's comments have already been taken care in the version published in BGD we assume this is a misunderstanding.

Below we comment in detail the points of revision.

**AR2:** Infaunal species showing a statistically better calibration in this sample set does not prove that infaunal species are better than epifaunal one for bottom water O<sub>2</sub> reconstruction. The O<sub>2</sub> penetration/gradient in shallow porewater are highly variable spatially and temporally. The living depth (below seafloor) may vary among infaunal species and may also change during the life cycle of the same species. Intrinsically, it is complicated to use infaunal species for quantitative bottom water O<sub>2</sub> reconstruction. I will probably remain unconvinced until similar calibration for *Uvigerina striata* is observed in another location.

**Reply:** This might be a misunderstanding and maybe we were not careful enough with our formulations, but we did not interpret the better statistical correlation of *Uvigerina striata* as a proof that infaunal species are better for bottom water O<sub>2</sub> reconstruction. In our manuscript we just described our observation for which species we found the best statistical correlation. Nevertheless, the reviewer is right that the problems in respect to the use of infaunal species for bottom water oxygen reconstruction have to be addressed. Thus we added the following part to the introduction:

*Infaunal foraminiferal species are able to migrate into the pore waters. Oxygen in the pore waters is consumed by the diagenesis of organic matter (Froelich et al. 1979), which might complicate quantitative O<sub>2</sub> reconstruction through infaunal species. Nevertheless, bottom water oxygenation usually has a strong influence on the oxygen gradient and penetration depth into the pore waters (Morford et al. 2005), which justifies to use also infaunal foraminiferal species for this study.*

**AR2:** My second major comment is about the vital effect. That indeed could be the reason for the large difference between *striata* and *pergrina*. However, the O<sub>2</sub> and hence iodate gradients in porewater are very steep. Because of the foram migration within sediments, the actual calcification depths for these species may be slightly different within the same genus, which could correspond to very different porewater iodate concentrations considering the steep

concentration gradient. I'm not sure it is a matured conclusion to pin it completely on vital effect, based on the observations in this study.

**Reply:** We changed the following part to our discussion to balance the interpretation of the differences between these two species and not just to pin it on the vital effect:

*This difference might either be related to a strong vital effect or to a species dependant difference in calcification depths. Oxygen gradients in the pore waters of a comparable OMZ off Pakistan are quite steep under suboxic conditions (Bogus et al., 2012) and  $IO_3^-$  probably follows this gradient. Thus, a difference in calcification depth might have a severe influence on the I/Ca ratio. These results suggest that a careful distinction of the analysed species is essential for the application of this proxy at least for the infaunal species.*

Furthermore, we modified the following part in our conclusions:

*This difference might either be related to a strong vital effect or to a species dependant difference in calcification depths. Oxygen gradients in the pore waters of a comparable OMZ off Pakistan are quite steep under suboxic conditions (Bogus et al., 2012) and  $IO_3^-$  probably follows this gradient. Thus, a difference in calcification depth might have a severe influence on the I/Ca ratio. These results suggest that a careful distinction of the analysed species is essential for the application of this proxy at least for the infaunal species.*

**AR2:** The large variability in *P. limbata* seems to be discouraging. However, can it simply be the real changes in bottom water O<sub>2</sub>? The OMZ boundaries could easily move up and down over time scales of seasons or even weeks. If I have to pick one calibration that I trust the most for bottom water O<sub>2</sub>, I may still pick epifaunal over infaunal ones, regardless of the variability within shells.

**Reply:** Indeed we also would have preferred to test more epifaunal species but *Planulina limbata* was the only epifaunal species we found in habitats with a broad range of oxygen availability. Nevertheless, we added the following part into the discussion to address the concern of the reviewer:

*In general due to the steep chemical gradients in the pore waters mentioned above epifaunal species might be more suitable for oxygen reconstructions because they should directly represent bottom water conditions not influenced by the microhabitat in the pore waters. ... The strong inter-test variability might indeed be related to real changes in oxygenation of the habitat, since there are strong seasonal fluctuations in the magnitude of the OMZ (Paulmier and Ruiz-Pino, 2009).*

**AR2:** For the analyses part, our JCp-1 is fairly homogenized straight out of the bottle. Measuring multiple powder splits or multiple dilutions from a single dissolved sample do not show large differences.

**Reply:** We just described our observation that the reproducibility of the JCp-1 significantly improved after regrinding the standard powder. Probably these problems and differences base on the use of different aliquots of the JCp-1.

### **Response to the comments of Jelle Bijma:**

We thank the reviewer for his constructive comments and positive feedback. Below we comment in detail the points of revision.

**JB:** I have received this manuscript before for a so called “quick review” and provided some comments. Many of those comments have already been taken care off in the present manuscript but some issues remain. First of all, this paper is a valuable contribution for BG(D). The msc is well written but some formulations should be checked by a native English speaker. For instance, introduction, line 14, p. 11639: “foramnifera“ should be “foraminifera”. Introduction, line 23: “oceanic warming“ should be “ocean warming”. Line 25, p. 11647: “Due to the TROX model the living depth ....” should be “According to the TROX model, the living depth ....”. Line 29, p. 11647: “.... to migrate in the pore waters...” should be “... to migrate into the pore waters ...”. These are just a few examples. Also, please spend some “,”.

**Reply:** The suggested changes have been done. The paper has been native checked.

**JB:** Below are a few minor remarks. The text can also be more concise, for instance: Abstract, line 5-7: “We test cleaning and measurement methods to determine I / Ca ratios in benthic foraminifera from the Peruvian oxygen minimum zone.” Leave out the last part because you have already mentioned that. Just write: “We test cleaning and measurement methods to determine I / Ca.”

**Reply:** As the reviewer suggested we changed this sentence:

*Cleaning and measurement methods for the determination of I/Ca ratios are tested.*

**JB:** Also: Abstract, line 12-14: “Although I / Ca ratios in benthic foraminifera might prove to be a valuable proxy for changing redox-conditions the iodine volatility in acidic solutions, ::: .”. Leave out the first part to be more concise: “The iodine volatility in acidic solutions, ::: .”. Abstract, line 15-16: “severely interfere with ::: ” change to “need to be accounted for when applying the :::.”

**Reply:** As the reviewer suggested we changed this sentence in the revised version of the manuscript:

*The iodine volatility in acidic solutions, the species dependency of I/Ca-[O<sub>2</sub>]<sub>BW</sub> correlations, and the individual variability of single tests need to be accounted for when applying the I/Ca ratio as a proxy for redox conditions.*

**JB:** The authors picked two infaunal species and two epifaunal species: Is there any proof that “In an eutrophic environment like the Peruvian OMZ where organic matter at the seafloor is available in excess (Mallon et al., 2012) an overprint by the organic flux is not to be expected.” (line 15-17, p. 11639)? I doubt that and would still expect to see a difference between bottom and pore water...

**Reply:** Actually this was a misunderstanding from our side regarding the initial “quick review”. Of course oxygen is consumed within the pore waters by the degradation of organic matter and later in the paper we also commented on oxygen profiles within the pore water. This sentence has now been removed from the introduction since it was intended to comment on the influence of organic matter on the living depth of infaunal foraminifera according to the TROX model and not on the diagenesis of organic matter. Furthermore we changed this part of the introduction slightly to avoid further misunderstandings:

*Infaunal foraminiferal species are able to migrate into the pore waters. Oxygen in the pore waters is consumed by the diagenesis of organic matter (Froelich et al. 1979), which might complicate quantitative O<sub>2</sub> reconstruction through infaunal species. Nevertheless, bottom water oxygenation usually has a strong influence on the oxygen gradient and penetration depth into the pore waters (Morford et al. 2005), which justifies to use also infaunal foraminiferal species for this study.*

**JB:** ...and in line 11, p. 11647 you write: “... since the oxygen gradients in the pore waters are quite steep.”. Any data available?

**Reply:** Actually there is no published data available from the Peruvian oxygen minimum zone regarding this topic. Nevertheless, unpublished data (Sommer et al., pers. comm.) is showing, that at 1000 m water depth where fluctuations of bottom water O<sub>2</sub> in the range of 30 to 44 μM were recorded (Sommer et al., unpublished data) in situ micro-profiling revealed that O<sub>2</sub> penetration depth into the sediments only reached a maximum of 5 mm. The boundary to the permanent anoxic center of the Peruvian OMZ is located at around 500 m depth and closer to that boundary oxygen in the pore waters was not even measurable at all. Since this data is not published and cannot be cited in our manuscript we changed this part and added the following citation regarding oxygen pore water profiles from the OMZ off Pakistan:

*This difference might either be related to a strong vital effect or to a species dependant difference in calcification depths. Oxygen gradients in the pore waters of a comparable OMZ off Pakistan are quite steep under suboxic conditions (Bogus et al., 2012) and IO<sub>3</sub><sup>-</sup> probably follows this gradient. Thus, a difference in calcification depth might have a severe influence on the I/Ca ratio.*

**JB:** Although this msc focusses mostly on “analytical methodology and evaluation as a proxy for redox conditions”, it would be good to expand a bit more on some of the other aspects, that are just as important if applying proxy relationships. The authors already did quite a good job in section 4.2 but could, if data allow, be more specific. For instance, if you look at the variability of I/Ca (see 4.2, p.11646, line 17-20: “Furthermore, the variability of foraminiferal I/Ca ratios by location (e.g. [O<sub>2</sub>]BW) or species is much higher than the uncertainties discussed in Sect. 4.1, which indicates that the trends in the I / Ca-[O<sub>2</sub>]BW

relationships are robust in respect to the technical issues.”), it seems that, at this stage, it is more of a qualitative proxy (more or less oxygenation) than a quantitative one. They should comment along those lines or add some additional information....

**Reply:** We added the following sentence into the discussion regarding this topic (Page 11 line 18-20):

*Regarding these issues, samples have to be carefully prepared and measured or foraminiferal I/Ca ratios might be considered more a qualitative to semiquantitative proxy at this stage.*

**JB:** ... For instance, line 2-3, p 11647: “Consequently, some samples are limited to one analysis.”. If data are available, I suggest to add a discussion on “patchiness” (how much of the signal is local variability) and to add a statistical discussion on how large the sample size should be to get robust numbers (e.g. use a “Jack-knife” technique).

**Reply:** Unfortunately there is no data available about the foraminiferal patchiness off Peru nor on the influence of patchiness on I/Ca. All sample duplicates we measured are from the same cores and not from other cores close to the first core.

The statistical discussion on how large the sample size should be using the “Jack-Knife” technique is completely out of focus of this initial study on foraminiferal I/Ca ratios. “Jack-Knifing” makes sense if a high amount of replicate measurements on different specimens from the same location is available like in the study of Schiffelbein and Hills (1984): We got a maximum of 5 replicate measurements on different specimens from the same location (not ~30 like Schiffelbein and Hills).

**JB:** Last but not least, the last sentence in the conclusion reads: “When samples are carefully prepared and measured, accounting for the pitfalls outlined here, the resulting I/Ca ratios from benthic foraminifera analysis may be considered a robust proxy for redox conditions in the ambient water mass.” In the light of the discussion, I suggest to tone down this conclusion.

**Reply:** We toned this sentence a bit down in the conclusions of the revised manuscript:

*When samples are carefully prepared and measured, accounting for the pitfalls outlined here, the resulting I/Ca ratios from benthic foraminifera are considered to be a promising proxy for redox conditions in the ambient water mass.*

Reference:

Schiffelbein, P. And Hills, S.: Direct assesment of stable isotope variability in planktic foraminifera populations, *Palaeogeography, Palaeoclimatology, Palaeoecology*, 48, 197-213, 1984, DOI: 10.1016/0031-0182(84)90044-0.