

Reviewer 1:

*“About the comparison between CT and light microscopy: The authors now included the equation of the linear regression. For the sake of completeness it would be great if the authors can add the statistical errors of the (i.e. the error of the slope and the intercept).”*

The standard error has been added to the reported equation as:  $y = 0.23(\pm 0.05) x + 2.64(\pm 1.49)$

*“One comment about the following sentence of the authors within the response letter:*

*“The reviewer raises a very good point. Unfortunately, all of these analyses were carried out on plankton tow specimens which may have been at different stages of (late) ontogeny, which complicates the meaning of a “last” chamber.” In this case, using the term “last” chamber without further explanation might be misleading. Please clarify directly, that not all individuals were in the same ontogenetic stage.”*

Thank you for pointing out that this needs further clarification in the manuscript. We have added the following to the methods: “We chose to focus our analysis of porosity on the most recent chamber as it was the chamber most likely to have formed under the conditions recorded at collection, however as the foraminifera analyzed had not yet reproduced, it is not possible to know whether this chamber would have been the terminal chamber, analogous to the final chamber in a fossil shell.”

Ralf Schiebel

*“I understand that new data on the molecular genetics of *G. hexagonus* are not easy to get on short notice. I also understand that Davis and coauthors may have the feeling that *G. hexagonus* is a normal component of Atlantic assemblages of modern planktic foraminifers. However, I know the specimens presented by Smart et al. (2018) by heart; the specimens really look like *G. hexagonus*, but they are not numerous. Therefore, we could not get data on the genotypes. Also, the *G. hexagonus* found in sediment assemblages are not numerous, and may have been transported into the Atlantic Ocean by currents (if these are *G. hexagonus* at all; I did not have a look at them). Therefore, I would suggest to be careful with stating that *G. hexagonus* is present in all modern oceans; this would need proof of an entire life cycle of *G. hexagonus*, including reproduction, in Atlantic water masses. Having said all this, Section “4.2 Globorotaloides *hexagonus* as an OMZ Indicator Species” reads better than before. The authors may still add that “final proof of the presence of *G. hexagonus* in the Atlantic may be provided by molecular genetics”, which I would regard as a good way to solve the issue.”*

We appreciate these comments and the reviewers experience in this area. However, we would point out that *G. hexagonus* is a rare taxa globally, making up generally <3% of the sedimentary or sediment trap assemblage even in the Indo-Pacific. Nor has the entire life cycle been demonstrated anywhere in the modern ocean. We understand that suggesting that *hexagonus* may be present in the modern Atlantic goes against some received wisdom, but given that it appears not to be entirely absent, we believe that it is worthwhile to revisit this question as we seek to better understand the ecology and biogeography of *G. hexagonus*. Ultimately, we are in agreement that molecular genetics could be useful in answering this question and have included the suggested addition rephrased as “However, additional evidence, such as molecular genetics, may be required to finally resolve this question.”

Minor points:

*“Line 44: Breitburg et al., 2108 should possibly read 2018”*

Of course it should! Thank you.

*“Line 196: “extremely oxygen depleted OMZ” is not a term defined below in this paper. I would suggest to drop “extremely”. Along the same lines, the paper would benefit from dropping a number of adjectives in places, which would make reading more scientific.”*

“Extremely” has been dropped and a few additional adjectives have been removed throughout where not entirely justified.

*“Line 211: change “transitional (nets sampling between these two extremes)” to “between these two concentrations”.”*

This has been altered as suggested.

*“Line 213: I would suggest to change “the densest population” to „highest standing stock”, which is possibly more correct in a scientific way. In general, densities may be changed for standing stock throughout the paper.”*

This has been changed as suggested

*“Lines 244-252: I would suggest to add the numbers of empty tests to the Table 1.”*

These have been added.

*“Line 273: better change “interaction” to “relationship”; interaction insinuates determination, which is possibly not the case.”*

This has been changed

*“Line 287: “Globorotaloides hexagonus tests were light for their size”, relates to something that is not mentioned. “Light” compared to what?”*

This has been removed.

*“Line 299: Please briefly describe what is meant by “compactness” and “aspect ratio”.”*

These two parameters are defined on lines 178-183 as “The compactness of tests was assessed as the ratio of the 2-dimensional surface area to the area of a circle (the most compact possible geometry) of the same perimeter. The aspect ratio was defined as the ratio between the height (longest dimension) and width (perpendicular to the longest dimension) as measured in the AutoMorph software (Hsiang et al., 2016).”

*“Line 337-339: „It is more likely that cytoplasm-bearing tests of T. sacculifer found below the photic zone are a consequence of their very high abundance in the surface ocean and reflected premature mortality and/or the retention of cytoplasm following reproduction.” Did these specimens still have their spines? If yes, they may not have reproduced.”*

This is a wonderful thought. For the most part, no, but spines were not consistently preserved in these tows, even in near-surface cytoplasm-bearing shells. It is possible this is the result of relatively long tow times (~1 hour in most cases), but regardless of cause poor spine preservation would not be a reliably indicative of reproduction in these particular samples.

*“Line 358: Predation may be another reason to be taken into account.”*

Great point. This has been added

*“Line 427 of the 1st version of manuscript: Sorry, this should have read Buchwald. Lines 441-442 in the new version of manuscript does still contain the same transposed digits: “Buchwald et al., 2105”, which may be changed to 2015.”*

Thank you for catching this. It has been corrected

*“Lines 456-464 presents a repetition of statements presented above, and may be rewritten or removed.”*

We’ve removed some repetitive language but retained the discussion of increased chamber number.

*“Final paragraph, lines 465-480: Another explanation for larger test sizes may be that *G. hexagonus* continuously grew larger under less optimal environmental conditions, i.e., lack of oxygen, and only reproduced with a delay when the environmental conditions had improved to support survival of the offspring (see Mojtahid et al., 2015 for *G. ruber*) and Schiebel and Hemleben (2017) for a more general explanation of the phenomenon.”*

We agree that delayed or slower reproduction at lower oxygen could be a plausible, however, it is unlikely that environmental conditions will “improve” in this habitat. i.e. the deep > 400 m OMZ where the largest individuals tend to be found is unlikely to ever be truly released from oxygen pressure. We have added this general idea to these lines as “Larger sizes could also result from delayed reproduction at lower oxygen levels.”

*“Figure 1: O<sub>2</sub> concentration larger than 7.5 ml L<sup>-2</sup> are not presented in the map, and may be cut from the scale. By doing so, the relevant part of the scale may be more detailed.”*

Figure 1 has been redrawn and rescaled.

*“Figure 9: The few red and yellow markers are not easy to see in the paper copy of the figure, and I would suggest to use darker color. Also, the green and blue markers would benefit from darker color.”*

We have replaced these markers with darker hues of the same colors.