Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-280-AC1, 2020

Biogeosciences Discussions

Interactive comment on "Vertical distribution of planktic foraminifera through an Oxygen Minimum Zone: how assemblages and shell morphology reflect oxygen concentrations" *by* Catherine V. Davis et al.

Catherine V. Davis et al.

cvdavis@seoe.sc.edu

Received and published: 5 October 2020

Many thanks for your detailed comments on our manuscript. Each point is addressed individually below.

"Most importantly, I would strongly suggest to convey analyses of the molecular genetics of the specimens of the two morphotypes (line 394). For unequivocal proof of the species concept, modern papers of the kind presented here may accept the great opportunity of molecular genetics, and not only rely on the morphospecies concept. Also

line 404-406: "The shells of G. hexagonus in deeper, less oxygenated waters appeared more porous, larger, and less compact than those from shallower, more oxygenated environments." These specimens may or may not represent two different genotypes" We agree that it is possible there are more than one genotype and would love to have genetic data to include. Unfortunately, the preservation of these samples makes them unsuitable for genetic analyses. We also see molecular genetics as an important future step to better understanding these OMZ-affiliated taxa and hope this can be carried out in the coming years.

"Second most importantly, I would strongly suggest to change the statements in lines 375-379, which are not substantiated by data: "We hypothesize that G. hexagonus occupies low-oxygen mid-waters globally (i.e., in the Atlantic as well as the Indo-Pacific), but that its deep habitat, low abundance, and the historical dearth of surveys of living planktic foraminifera in low O2 regions along the western African margin have biased observations of G. hexagonus in the modern Atlantic." – Many studies of sediment trap and net tow samples in the South Atlantic off Namibia (Loncaric and colleagues from Bremen) and the Congo River mouth (Ufkes et al.), as well as surface sediment, would have certainly detected G. hexagonus if present. I have myself seen many net tow, sediment trap, and bottom sediment samples from the Atlantic and other ocean basins, including my PhD project on benthic foraminifers from surface sediments in the Gulf of Guinea, and I have never seen a test of hexagonus in that region." We will remove reference to the African margin, as we agree that we lack affirmative reporting of G. hexagonus in these regions in particular. However, there are reports from Atlantic sediment traps (Smart et al., 2018 as cited) as well as from recent sediments (e.g., the Brown Foraminiferal Database) which demonstrate that G. hexagonus is and has been present in the recent past in some regions of the Atlantic.

The following references have all been added as suggested "Line 40: please refer to the nice paper of Schmidtko et al. 2017 on modern OMZs" "Line 88: see also: Glock, N., et al. (2018) Nature Communications, 9, 1217, doi:10.1038/s41467-018-03647-5

Glock, N., et al. (2019) PNAS, 116 (8), 2860-2865, doi:10.1073/pnas.1813887116" "Line 327: Please refer to Schiebel et al. (2004) for T. sacculifer in another study in a region with a prominent subsurface OMZ, i.e. the Arabian Sea." "Line 369: refer again to Schiebel et al. (2004)"

"Line 75: please add also Warren 1994, see Schiebel and Hemleben 2017" The reference from Schiebel and Hemleben (2017) appears to be "Warren, B.A. Context of the suboxic layer in the Arabian Sea. Proc. Indian Acad. Sci. (Earth Planet Sci.) 103, 301–314 (1994). https://doi.org/10.1007/BF02839540". However, this article does not mention foraminifera or plankton tows as relevant to Line 75. Is it possible that there is another citation that should be added here?

"Line 125: please give the net strata depths and volume filtered for each net in Wishner et al. 2019, 2020b here as well. This does not consume much space, and saves the reader from consulting for additional literature." These can be found in the supplement, reference to which will be included here.

"Lines 125-126: what was the filtered water volume?" This varied between tows and can be found in the supplement, which is now referenced here.

"Line 227: Foraminifers do not really die in most cases, but reproduce. Therefore, you may change "dead" for "empty" (tests)." This is true in most cases and will be altered elsewhere in the manuscript. However, here we specifically reference a few individuals found with cytoplasm (not empty) well below their photic zone habitat. This is suggestive that they may not have successfully reproduced.

"Lines 299-301: "This species can be considered an indicator of an OMZ habitat and may be useful as an OMZ marker in sedimentary records, as discussed below." This should possibly be the final statement of the section. BTW: This finding is not new to science; please refer to the respective literature." The sentence will be moved, and while the finding of G. hexagonus associated with the OMZ is not new, it has not to the best of our knowledge been used as an indicator of an overlying OMZ in the published

literature.

"Lines 302- 310 present a repetition of the "Results". Please rewrite." These lines will be rewritten with a greater emphasis on the implications rather than numeric abundances.

"Line 318: "larger" reads better than "more large" to me." There is a subtle but real distinction here. We don't have data on the distribution of size ("larger") but are rather arguing that more individual foraminifera in the relatively large size class (> 222 μm) sampled by our nets were present. We will rephrase to clarify.

"Lines 322-324: "Use of presence/absence of cytoplasm as an indicator for living foraminifera results in an overestimation of live individuals, as dead individuals may retain some cytoplasm while live individuals cannot be devoid of cytoplasm." This is possibly not entirely true, since decease is most often caused by reproduction, and cytoplasm is consumed and partly converted into offspring (see above)." We agree and this will be rephrased as "dead or post-reproductive individuals" as both cases can result in a shell retaining some cytoplasm.

"Line 427: Buchmann year of publication" I'm afraid, I don't understand this comment. Would it be possible for the reviewer to clarify so that we can address this?

"Lines 431-432: "(e.g., Bijma. . ." Some species increase in weight, others decrease. Please see Beer et al., Geology, 2010, using samples from the Arabian Sea, i.e. another OMZ region." This is an important point, but I would argue not directly relevant to the argument being made in the manuscript. Beer and other authors (for example also Weinkouf et al., 2016) raise the important caveat that carbonate chemistry may not be a primary driver of SNW in all species and regions, pointing to other drivers such as nutrients and temperature. However, it has not been shown, including by Beer et al., 2010, that any planktic foraminifer increases in weight as a response to decreasing carbonate chemistry. Our results are broadly consistent with a widely recognized carbonate ion driver in direction, though we do not explicitly discount other drivers, and

given the lack of both in situ carbonate chemistry and other (e.g., nutrient measurements) a further discussion of potential drivers of SNW is really outside of the scope of this manuscript.

“Line 447: better use outnumber or surpass instead of overwhelm.” We will remove this descriptive language so that the phrase reads “However, the increase in size with decreased oxygen availability is such that. . .”

“Figure 2: The images may be oriented and organized in a way that makes comparison easier and consumes less space.” This figure will be made more compact.

The following changes will be made as suggested: “Line 158: you may want to explain the abbreviations F and F-1 at first mention. Lines 212-214, and 220-221: You may skip the sentence on which species were present and absent. Many other species were possibly absent as well, and are not mentioned. The information on the presence and absence of species should also be available from figures and data tables. “Line 108: change reches to reaches” “Line 234: “Empty test assemblages” “Line 453: better “as in some benthic. . .”” “Figure 8: upper quartile boxes of F-1 and F-2 are flawed.”

References Weinkauf, M. F. G., Kunze, J. G., Waniek, J. J., and Kučera, M., 2016, Seasonal Variation in Shell Calcification of Planktonic Foraminifera in the NE Atlantic Reveals Species-Specific Response to Temperature, Productivity, and Optimum Growth Conditions: PLOS ONE, v. 11, no. 2, p. e0148363.

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