

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.

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Received and published: 9 September 2020

The paper of Catherine Davis and coauthors on “Vertical distribution of planktic foraminifera through an Oxygen Minimum Zone: how assemblages and shell morphology reflect oxygen concentrations” is well written, and adds important information on the ecology and paleoecology of the so far poorly understood planktic foraminifer species *Globorotaloides hexagonus*. The species has been known to be associated to oxygen minimum zones (OMZs), which are currently expanding as a result of overall rising air and ocean temperatures. Consequently, *G. hexagonus* may be used as an

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indicator of past and future climate change. I would suggest to accept the paper with moderate revisions on the following points.

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.

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.

The following references may be added to support the findings of Davis et al.:
Line 40: please refer to the nice paper of Schmidtko et al. 2017 on modern OMZs
Line 75: please add also Warren 1994, see Schiebel and Hemleben 2017
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.

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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)

Some minor issues to be addressed: Line 108: change reaches to reaches 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. Lines 125-126: what was the filtered water volume? 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 227: Foraminifers do not really die in most cases, but reproduce. Therefore, you may change “dead” for “empty” (tests). Line 234: “Empty test assemblages” Lines 299-301: “This species can be considered an indicator of an OMZ habitat and may be useful 300 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. Lines 302-310 present a repetition of the “Results”. Please rewrite. Line 318: “larger” reads better than “more large” to me. 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 decrease is most often caused by reproduction, and cytoplasm is consumed and partly converted into offspring (see above). Line 427: Buchmann year of publication 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. Line 447: better use outnumber or surpass instead of overwhelm. Line 453: better “as in some benthic. . .”

Figure 2: The images may be oriented and organized in a way that makes comparison easier and consumes less space. Figure 8: upper quartile boxes of F-1 and F-2 are

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flawed.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-280>, 2020.

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