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Biogeosciences Discussions

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Interactive comment

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

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Many thanks for your thorough review and perspective on our manuscript. Each point is addressed individually below.

"The morphometric analyses were mainly done, using a normal light microscope. This has the advantage, that the use of the phenotypic plastic traits as paleoproxies can be done without electron-microscopy, which is both cheaper and faster. Unfortunately, this approach has also strong limitations. The pores of foraminifera are typically too small

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to measure their size correctly under a light microscope. Therefore, the measured total porosity is also likely to be very inaccurate. This might be one reason for the strong scattering in figure 9a and the low R2 of the correlation between porosity and dissolved oxygen. . .. I would recommend determining the porosity on a few electron microscope pictures and correlating them with the porosity measured on the same individuals using the light microscope method. This would give a coarse estimate about the accuracy of this method. The authors have already done a similar comparison using micro-CT. Nevertheless, micro-CT is also at the limit of resolution, considering the size of pores in foraminiferal tests." Âň We are in full agreement with the reviewer here as to both the limitations and benefits of using light microscopy for measurements of porosity. We will add a line explicitly discussing the trade-offs in using this method in our revised draft. In the case of G. hexagonus, the pores are quite large (see images with more to be included), meaning that the greatest limitation in practice has been the curvature of the shells. Images from micro-CT scans were an excellent way to minimize this problem, by generating an essentially unlimited number of angles available from which to measure porosity. We have included additional CT-scan generated images demonstrating both this approach as well as the resolution of the method. In this particular case, SEM has the disadvantage of being a functionally destructive analysis. This is due to the need to mount quite fragile shells on carbon tape, coat them, and in some cases amputate chambers (to look at internal porosity). Given the limited number of shells available, and the limited benefits of SEM in addition to CT analyses, we have opted not to image our shells in that way. We leave open the possibility that this may be a preferable method for the development of a quantitative porosity-based proxy for oxygenation in G. hexagonus, but such a quantitative assessment is beyond the scope of the current paper, and likely this sample set. At this point, we are only able to demonstrate an empirical trend, captured by two very different approaches.

"Regarding this correlation: What kind of fit has been used to determine R2 and P. Was it a linear fit? Is it possible to give the equation of the fit in the paper?" "In this context the authors state: "A comparison of the two methods carried out on a subset of shells (n = 31) showed that the results from the two approaches are correlated (R2 = 0.37, p-value < 0.001; Fig. 7), indicating that the less labor-intensive use of light-microscope measurements captures some of the same trend as the CTbased approach." In my opinion a correlation coefficient of 0.37 is too low, to make such a statement, when comparing the two methods". This is a linear fit and the equation will be included (y= x*0.23 + 2.64). The comparison is simply meant to demonstrate that multiple methods result in the same trend, but we would caution that due to the low predictive power of this relationship; light microscopy should not be used to estimate CT-based porosity. The two methods are not interchangeable, though both capture the same trend. We will make sure to clarify in our added discussion of the trade offs between imaging methodologies.

"G. hexagonus and H. parapelagica seem to be well adapted to oxygen depleted environments. This is a very interesting finding for planktic foraminifers. What I miss in this paper is a small discussion about different survival strategies of (benthic) foraminifera to oxygen depletion. They might apply to planktic foraminifera, too." We will add an additional short discussion of denitrification, dormancy, and kleptopasty in benthic foraminifera.

"The finding that the size G. hexagonus specimens increases with decreasing oxygen is counterintuitive but very intriguing. A similar observation has been done on benthic foraminifera from the same region (Keating-Bitonti and Payne, 2017). The paper is already cited but it might be worth to mention the finding from above in the discussion." We will elaborate to include a clause on their results as to size, in particular that only two of four species analyzed demonstrate the expected decrease in size with decreasing oxygen concentrations.

"The porosity of G. hexagonus seems to decrease with ontogeny. This seems to be the opposite trend than in benthic foraminifera. As far as I know, usually the last chamber is the most porous. The ontogenetic trend might be a problem for the application as a paleoproxy. Could the authors think about a method, that minimizes the influence of this ontogenetic trend?" 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. It will probably be necessary for more work to be done on shells at their terminal stage, before the findings presented here could be presented as a quantitative paleoproxy.

"Line 439: "is consistent with a reduction of overall calcification in low oxygen, DIC rich environments, where precipitation and maintenance of a shell may be more metabolically expensive." Is there a reference that calcite precipitation is metabolically more expensive under oxygen depletion? Otherwise, this is very speculative. The formation of biomass, for example, is energetically favorable under oxygen depletion." The implication is not meant to be that calcite precipitation is metabolically more expensive under oxygen depletion (this may or may not be the case, but we whole-heartedly agree that there is currently no evidence), but under a DIC-rich environment coincident with the OMZ. This will be rephrased as "....reduction of overall calcification in low calcite saturation states associated with the OMZ...."

"Line 426: The authors write that nitrate increased with depth. Is there a correlation between shell size and nitrate availability? In this case, the increased porosity might be just a secondary feature, due to the lower surface to volume ratio in larger individuals." Nitrate availability increases in the region with depth as does size, however as we do not have data from the same tows, attempting to correlate these two parameters directly would probably be overreach. That increased porosity could in a sense be compensating for lower surface/volume ratio is an interesting possibility and will be added to the discussion.

"The following paper might be worth to be considered in the discussion: "Richirt, J., Champmartin, S., Schweizer, M. et al. Scaling laws explain foraminiferal pore patterns. Sci Rep 9, 9149 (2019). https://doi.org/10.1038/s41598-019-45617-x"" The findings of this paper will be referenced.

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“I think the pictures of G. hexagonus in figure 2 are not very representative of this species. Is it possible to add electron-micrographs of the two species from this figure, focusing on the morphological traits that characterize these species?” We will add additional CT-scan images of G. hexagonus to a new panel in Figure 9, to demonstrate a greater range of the morphologies observed.

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