Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-280-RC2, 2020

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.

Anonymous Referee #2

Received and published: 9 September 2020

The paper "Vertical distribution of planktic foraminifera through an Oxygen Minimum Zone: how assemblages and shell morphology reflect oxygen concentrations" by Davis et al. presents an interesting study on planktic foraminifers from the Californian OMZ. The vertical distribution of planktic foraminifera seems to be controlled by oxygen availability and the porosity of G. hexagonus appears to be a phenotypic plastic trait that has the potential to be used as a paleoproxy for oxygen concentrations in the water column. While a lot of previous studies focused on the influence of oxygen availability on the distribution of benthic foraminifera, studies on the influence oxygen on planktic

foraminifera are scarce. The paper is well written and the results of this study are new and definitely worth being published in Biogeosciences. Nevertheless, there are a few points of moderate revision that should be addressed before publication:

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 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. 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? 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. 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 CT-based approach." In my opinion a correlation coefficient of 0.37 is too low, to make such a statement, when comparing the two methods.

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.

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. 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?

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.

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.

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"

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?

Other than these few moderate points of critique, this is a very intriguing, explorative pilot study on the development of a potential proxy for oxygen depletion in the water column and I would be happy to see this published in revised form.

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