

Review of BG-2024-3295 manuscript

I have reviewed the manuscript entitled “Reviews and Syntheses: Trait-based approach to constrain controls on planktic foraminiferal ecology: key trade-offs and current knowledge gaps” by Kirsty M. Edgar et al. This manuscript aims to provide a review of the state of the art of the trait-based approach surrounding planktic foraminifera ecology and focuses on the gaps that need to be fulfilled to improve the aforementioned approach.

Overall comments:

I think that, overall, the manuscript is well structured and does a good job of gathering all the major ecological aspects of planktic foraminifera, but also, the current knowledge gaps. I consider it is a valuable contribution in the field of planktic foraminifera ecology/modelling as it summarizes the areas that need further research pretty well. Also, it highlights the trait-based approach as a useful tool to make reliable predictions for the impact of the environmental change on these organisms.

Concerning the language, it is well-written, however, some sentences are too long and complicated, making it hard to understand the message behind them. Also, I've noticed some technical writing mistakes such as double spaces, lacking commas and dots etc...

I have mainly 3 concerns in regard to what I think the manuscript misses that should be an easy fix. I hope the following suggestions will help the authors to improve the manuscript.

- eDNA and eRNA analyses are mentioned in the: abstract, introduction and conclusion, however, there are nowhere to be found in the manuscript. I think these approaches should be given the same treatment as they are nowadays one of the main ways we have to decipher some ecological aspects of the planktic foraminifera.

- The trait-based approach is pretty well detailed, and this manuscript presents it as a powerful tool for future predictions. Nevertheless, I feel that only the positive aspects of it are presented, not the limitations. In a synthesis paper I do not necessarily expect a discussion, but I feel that the limits of this approach, other than just the lack of quality input data, should at least be described/acknowledged. Also, it remains a model, which is a simplification by its own nature. Therefore, the differences between models and in-situ observations should be better described.

- Finally, it is stated numerous times that better datasets and experimental data is needed to improve our current knowledge of planktic foraminifera. However, the ideas and suggestions for improving them remain rather vague and, in that regard, how can the trait-based approach contribute to fill the missing data? Even after reading the manuscript a few times, one would struggle to come up with a strategy to overcome those gaps.

Considering the described comments, I think that this is a valuable and interesting contribution that should be considered for publication.

Specific comments:

Lines 138-139. This sentence is odd. Larger sizes in the tropical regions and smaller in the equatorial region?

Lines 172-174. This seems contradictory. Here, the effect of spines increasing the sinking rate is described as a benefit, however, in Table 2, negative buoyancy is described as a cost.

Lines 179-181. I think that this sentence should be re-written as it is correct, but not precise. Non-spinose species are omnivorous, they have the capacity of eating small prey such as phytoplankton, but they prefer an herbivorous diet (Hemleben et al., 1985). Also, the diet is known to change across the development of a given species. For example, *O. universa* shifts from herbivorous during its early and juvenile phase, to a carnivorous diet in the spherical adult stage (Schiebel and Hemleben, 2017).

Lines 203-215. I feel that this paragraph misses the chance of describing 2 mechanisms that affect the calcification in an important manner: the presence or absence of photosynthetic algae symbionts and the metabolic processes involved when forming new chambers during the individuals growth (Schiebel and Hemleben 2017; Takagi et al., 2020, 2021). The first process gives the species that host them a slight advantage as they modify the chemistry of the immediate water column, while the second mechanism is still under poorly constrained. However, I think that, as this is a review paper, these mechanisms should be described.

Lines 217-220. Too many concepts in one sentence makes it hard to read. Maybe separate?

Lines 223-226. See previous comment, long and complex sentence. Maybe rephrase? Also, you consider this effects are “short-termed”, however, the impact on the reproductive strategy, i.e. reduction of gamete/body size, is not that short term. Influencing the reproductive strategy of a species depends on a wide array of factors. I suggest rewriting this sentence in a more conservative approach.

Lines 249-251. See previous comment surrounding spinose/non-spinose species feeding preferences. I think that the two paragraphs surrounding feeding strategy should be more homogenous.

Lines 261-262. This sentence lacks clarity, I would avoid mentioning “most zooplankton”.

Line 266. Considering the development of the paragraph, which is correct, I suggest to state that non-spinose species are omnivorous with a preference towards an herbivorous diet. By the way, that is what the reader could interpret by following the paragraph.

Lines 284-286. Again, long and complicated sentence. Maybe separate in 2?

Line 340. Certain morphotypes of *G. bulloides* have recently been described as hosting bacterial symbionts (i.e. *Synechococcus*) (Bird et al., 2017). I would also correct in Table 2 when *G. bulloides* is described as hosting “none” symbionts.

Line 365. Whole paragraph. See previous comment regarding the effect of photosymbionts on calcification. Further elaborating about it, as suggested earlier, in this paragraph could also be a possibility.

Technical comments:

Please re-read the manuscript carefully as these are examples.

Line 50. Add “in” after challenges.

Line 67. Comma after “years”.

Line 94. Comma after “morphology”.

Line 102. Maybe rephrase with “Species are listed alphabetically here”.

Line 104. Ecological instead of “ecology”

Line 138. Comma after “tropics”

Line 172. Not sure about this one, but I think that if no number follows the measure, it should not be abbreviated.

Line 176. Comma after “Logically”.

Line 197. Add “of” between “lack” and “numerous”.

Line 210. Comma after “data”.

Line 213. Comma after “practices”.

Line 214. Change “to” with “in” before “the pelagic”.

Line 267. “Eukaryotic” instead of “Eukaryote”.

Line 271. Comma after “Rhizopodia”.

Line 299. “With instead” of “between”.

Line 316. Add “the” before non-spinose.

Line 337. Comma after “photoendosymbionts”.

Line 357. Chlorophyll-*a*.

Line 365. Comma after “fixed carbon”.

Line 436. Planktic foraminifera.

Line 447. Commas before and after “which”.

Line 448. Comma afther “sea ice”.

Line 469. Double space before “Its trade off”.

References

Bird, C., Darling, K. F., Russell, A. D., Davis, C. V., Fehrenbacher, J., Free, A., Wyman, M., and Ngwenya, B. T.: Cyanobacterial endobionts within a major marine planktonic calcifier *Globigerina bulloides*, Foraminifera) revealed by 16S rRNA metabarcoding, *Biogeosciences*, 14, 901–920, <https://doi.org/10.5194/bg-14-901-2017>, 2017.

Hemleben, C., Spindler, M., and Anderson, O. R.: Modern Planktonic Foraminifera, Springer New York, New York, NY, <https://doi.org/10.1007/978-1-4612-3544-6>, 1989.

Schiebel, R. and Hemleben, C.: Planktic Foraminifers in the Modern Ocean, Springer Berlin Heidelberg, Berlin, Heidelberg, <https://doi.org/10.1007/978-3-662-50297-6>, 2017.

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Takagi, H., Kurasawa, A., and Kimoto, K.: Observation of asexual reproduction with symbiont transmission in planktonic foraminifera, *Journal of Plankton Research*, 42, 403–410, <https://doi.org/10.1093/plankt/fbaa033>, 2020.