

Referee comments in black.

*Author's responses in green.*

The manuscript by Bonifacio and colleagues represents a relevant contribution to the study of polymetallic nodules fields. As the Authors state, it is necessary to understand ecological processes and diversity patterns occurring in these environments and to assess the impact of mining activities before starting with their exploitation, and in this frame, this manuscript is of great value. I would therefore endorse its publication in Biogeosciences. The manuscript is in my opinion clear and well-written. My only concern is represented by the use of the word "morphotype", as it is a somewhat ambiguous term. The most widespread use (at least, in my experience) regards morphotypes as divergent morphological variants within the same alleged species, pointing at either cryptic diversity or phenotypic plasticity. However, in this case it is employed to define individuals that morphologically can be assigned to the same taxon; if I correctly understood, the use of the term "species" or "taxa" has been avoided because molecular data often challenge this interpretation. I think however that in this case the best way would be the use of "morphospecies" or "taxa identified on the basis of morphological features". The term "morphospecies" is employed twice, in both cases at page 8. At line 7 the term is used in the same way of "morphotype" in the remaining manuscript, and as I would advise, but at line 13 the use of "morphospecies" is inappropriate, as here the Authors clearly refer to taxa identified by the combined morphological and molecular data. Morphospecies clearly do not correspond to OTUs (p. 8, line 13), and here the Authors are referring to OTUs. I suggest to carefully re-read the manuscript, as there is some terminological confusion around "morphotype", "morphospecies" and "OTU".

*Sorry about this confusion around "morphotype", "morphospecies" and "OTU". We have changed morphotype and MOTU terms to morphospecies with the addition of the following sentence (page 6, line 18-19):*

*'As genetic data was only used to separate closely related species, the delimited taxa entities in the present study are referenced as morphospecies.'*

*Morphotypes has been changed to morphospecies throughout the manuscript.*

*To be more clear the section "2.4 Operational taxonomic units (OTUs)" has been reworded to "2.4 Taxonomic identification and feeding guilds classification"*

Some minor comments follow.

P. 1, line 10: I suggest to add "environmental" to "footprint".

*Page 1, line 10. Done as suggested.*

P. 7, line 31: Replace "paranoids" with "paraonids".

*Page 9, line 4 Done as suggested.*

P. 11, line 18: "(Magalhães and Bailey-Brock (2017))": replace with "(Magalhães and Bailey-Brock, 2017)"

*Page 12, line 14. Done as suggested.*

P. 11, lines 27-28: Although all Eunicida are usually considered as carnivores, Jumars et al. (2015) suggest that the diet of Lumbrineridae might be more varied, and that sediment and decaying vegetal debris might represent an important diet component for several species. In particular, the reported characterising species belongs to the genus Lumbrinerides, a genus including small, possibly pedomorphic species that at least in shallow environments occupy an ecological niche totally different from larger species of Lumbrineris and Scoletoma. I think that in this case carnivory is not obvious.

*Indeed Jumars et al. (2015) considered lumbrinerids are carnivores with very few exceptions. In our case, we believe that the sampled lumbrinerids are carnivorous because no clear sediment was observed in their body. Furthermore, lumbrinerids follow the pattern observed for other unquestionably carnivorous such as sigalionids and paralacydoniids. In order to reinforce this idea, we add the following sentence page 12, lines 23-24:*

*“Furthermore, other carnivorous families were relatively more abundant in the eastern areas as well, such as paralacydoniids and sigalionids.”*

P. 12, lines 20-21: results by Guggolz et al. (2018) have been partially rediscussed in Guggolz et al. (2019: Scientific Reports 9: 9260). I suggest to check and cite also this work.

*The following sentence was added in order to cite Guggolz et al. (2019) and Guggolz et al. (submitted) in section 4.2 Species turnover and geographic ranges, page 13, lines 23-25:*

*“This was however not the case for species of Laonice, which tended to show large ranges of up to 4000 km across the Eastern and Western Atlantic (Guggolz et al., 2019); or species of Aurospio and Prionospio which could show pan-oceanic distribution (i.e., Pacific and Atlantic oceans; Guggolz et al., submitted).”*

*References cited in the authors answers:*

*Guggolz, T., Meißner, K., Schwentner, M., and Brandt, A.: Diversity and distribution of Laonice species (Annelida: Spionidae) in the tropical North Atlantic and Puerto Rico trench, Sci. Rep., 9:9260, 10.1038/s41598-019-45807-7, 2019.*

*Guggolz, T., Meißner, K., Schwentner, M., Dahlgren, T.G., Wiklund, H., Bonifácio, P., and Brandt, A.: High diversity and pan-oceanic distribution of deep-sea polychaetes: Prionospio and Aurospio (Annelida: Spionidae) in the Atlantic and Pacific Ocean, Org. Divers. Evol., submitted.*

*Jumars, P. A., Dorgan, K. M., and Lindsay, S. M.: Diet of Worms Emended: An Update of Polychaete Feeding Guilds, Ann. Rev. Mar. Sci., 7, 497-520, doi:10.1146/annurev-marine-010814-020007, 2015.*