

Interactive comment on “Geographic distribution of free-living marine nematodes in the Clarion–Clipperton Zone: implications for future deep-sea mining scenarios” by Freija Hauquier et al.

Anonymous Referee #2

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This MS is on deep-sea meiofaunal distribution and diversity. Dataset is rather small, and analyses are simple. But the studied site CCZ is well-known deep-sea nodule mining area proposed and solid environmental/ecological assessment is urgently needed. In this regard, the dataset and analyses are basically reasonable enough. So the MS is of high importance and timely. I suggest relatively minor revision. Broader citation on deep-sea ecology & biodiversity and their environmental control is needed (as detailed below).

Specific comments are listed below:

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P. 2, Line 11 "...poorly understood (...)" Add Sweetman et al. and Yasuhara & Danovaro 2016

Sweetman, A. K., Thurber, A. R., Smith, C. R., Levin, L. A., Mora, C., Wei, C. L., Gooday, A. J., Jones, D. O. B., Rex, M., Yasuhara M., Ingels, J., Ruhl, H. A., Frieder, C. A., Danovaro, R., Würzberg, L., Baco, A., Grupe, B. M., Pasulka, A., Meyer, K. S., Dunlop, K. M., Henry, L. A., Roberts, J. M., 2017. Major impacts of climate change on deep-sea benthic ecosystems. *Elementa*: 5, doi:10.1525/elementa.203.

Yasuhara, M. and Danovaro, R., 2016. Temperature impacts on deep-sea biodiversity. *Biological Reviews*, 91: 275–287.

P. 2, Line 14 "...complicated by low sedimentation rates at these depths" Why?

P. 2, Line 15 "...the smaller-sized macro- and meiofauna (Ramirez-Llodra et al., 2010)." Add classic Hessler and Sanders papers for high deep-sea biodiversity.

P. 2, Line 15-17 "This is partially a consequence of high heterogeneity that exists as a result of the complex geological and hydrological features of the deep seafloor (Vanreusel et al., 2010), which create microscale patchiness in both abiotic and biotic features." It seems the authors' result doesn't support this statement. Dominant species are the same in all the site, showing low heterogeneity. You may site Levin et al reviews and some Yasuhara and McClain papers on temperature vs POC issues. Levin et al., 2001. *Ann. Rev. Earth Planet. Sci.* 32, 51–93

Yasuhara, M., Hunt, G., Cronin, T.M. and Okahashi, H., 2009. Temporal latitudinal-gradient dynamics and tropical instability of deep-sea species diversity. *Proceedings of the National Academy of Sciences of the United States of America*, 106: 21717–21720.

Yasuhara, M., Okahashi, H., Cronin, T.M., Rasmussen, T.L. and Hunt, G., 2014. Response of deep-sea biodiversity to abrupt deglacial and Holocene climate changes in the North Atlantic Ocean. *Global Ecology and Biogeography*, 23 (9): 957–967.

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McClain et al. 2012. Energetics of life on the deep seafloor. PNAS

Tittensor et al. 2011. Species–energy relationships in deep-sea molluscs. Biology Letters

P. 3, Line 29 and other places. Brackets usage are strange in several places like “using the guide of (Higgins and Thiel, 1988).”

P. 4, Line 12 “For this study, clay (sediment particles < 4 μm) and silt (4–63 μm) size fractions were considered in further analyses.” Good to specify what are the “further analyses”

Did you address spatial auto-correlation for your regression models?

P. 7 line 1. “repeatedly (e.g., (Wei et al., 2010)).” Check the house style of bracket usage. Not only here, but also other places.

P. 9, line 4. “only few genera occurred in higher numbers” Higher than what? This sentence is unclear a bit.

P. 9 Line 30: “. . .ecosystem functioning” Cite Danovaro and Yasuhara papers regarding ecosystem functioning.

Yasuhara, M., Doi, H., Wei, C. L., Danovaro, R. and Myhre, S. E., 2016. Biodiversity–ecosystem functioning relationships in long-term time series and palaeoecological records: deep sea as a test bed. Philosophical Transactions of the Royal Society B: doi:10.1098/rstb.2015.0282

Danovaro et al., 2008. Exponential Decline of Deep-Sea Ecosystem Functioning Linked to Benthic Biodiversity Loss. Current Biology

In Discussion, the authors emphasized high number/diversity of rare genera and species. But such is not emphasized in the abstract. These rare taxa tend to be found in only one site and thus are highly vulnerable for mining, right? If this is correct, the authors need to emphasize this more.

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Discussion is a bit lengthy too much and can be simpler, given the simple and small dataset. It seems the authors talk too much based on small data.

Do you see any environmental control of diversity in your data?

–End of Letter–

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-492>, 2018.

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