Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-304-RC1, 2019

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Are seamounts refuge areas for fauna from polymetallic nodule fields?" *by* Daphne Cuvelier et al.

Anonymous Referee #1

Received and published: 2 October 2019

General evaluation

The paper addresses the question whether seamounts may act as refuges for fauna that will potentially be affected by large-scale mining for polymetallic nodules. It is based on the assumption that the occurrence of hard substrate at both habitat types may result in similar megafaunal communities. Using ROV video transects, seamounts and nodule fields in the CCZ area were compared with respect to composition and abundance of megafauna. The authors conclude that the observed substantial differences between seamount and nodule field communities make it highly unlikely that seamounts can compensate for the impacts of deep seabed mining.

The recent efforts to promote deep-sea mining for minerals, which, in the case of nodule mining, will involve the destruction of large seabed areas including its faunal com-

Printer-friendly version

Discussion paper

munities, make the paper highly relevant in the light of the need to preserve biodiversity in the deep sea. Although the paper is purely descriptive, it could add to the knowledge of biodiversity in the CCZ and particularly at the seamounts in the area, which had not been sampled before, and make a valuable contribution to the question whether deep seabed mining can be responsibly managed. However, the paper has several weaknesses, which have to be addressed before publication. My main concern is that no environmental data are presented. In additon to missing hydrographical data, there is no detailed description of the sampling sites, particularly the seamounts, such as size, summit and base depth, inclination of slopes/general bathymetry, or current field. More important, no information is given on habitat types encountered along the transects. It is well known that substrate can vary considerably at short distances at seamounts, and of course megafaunal communities are strongly associated with substrate type. This is briefly mentioned in the discussion, but I do not understand why this information is not provided and analysed in the results. It should easily be available from the video footage.

There are also some methodological issues. The basic problem, as also admitted by the authors in the discussion, is that only a very limited number of rather short transects without replications are available and that transects at the seamounts and at the nodule fields were taken at different depths; in the case of Mann Borgese Smt the depth sampled was nearly 3000 m less than on the corresponding nodule field, and hence the data are hardly comparable. Although the depth difference at the other sites was much smaller, it may also limit the comparability of the data. This is mentioned in the discussion, but the consequences should be elaborated in more detail, and it makes the conclusion that "seamounts appear inadequate as refuge areas to help maintain nodule biodiversity" disputable. I am also not convinced that the quantification of the samples is correct. In section 2.1, the authors state that the altitude of the ROV was "kept constant whenever possible". Apart from not providing the information at which target altitude the ROV was kept, and whether this was the same at all transects, the authors inform in section 2.2 that, due to varying altitude as well as pan and tilt

Printer-friendly version

Discussion paper

of the camera, "surface coverage" could not be used for standardisation and instead just transit length was used. However, since the field of view and thus the number of visible objects per unit transit section depend on the altitude and angle of the camera, the standardisation to 100 m transit sections, without taking into account the varying field of view, could strongly bias the results. Generally, the methods section has to be improved with much more detail.

Further, it is not clear how the investigations made in this study relate to those by Vanreusel et al (2016) who also presented results for epifaunal communities in the CCZ, comparing APEI, BGR, GSR and others. Obviously the sampling was done on the same cruise using the same gear. Were the same nodule field transects analysed? If yes, this has to be justified, the additional value of this study as compared to Vanreusel et al (2016) has to be demonstrated (apart from the additional seamount transects) and any overlap and differences in the analysis indicated. If not, a thorough comparison between the results of both studies is necessary.

There are numerous smaller issues throughout the text, including discrepancies between abundances given in the text and in Tab. A1. Details are given below.

Because of the scarcity of information on faunal assemblages in the deep sea in general and particularly at seamounts in the area of the CCZ I suggest to consider the paper for publication despite its weaknesses, but only after major revision, taking into account the comments above and the suggestions for improvements given below.

Specific comments

- Use consequently "Ophiuroida, Asteroida" etc. instead of "ophiuroids, asteroids" etc.

- Abundances given in the text are not always consistent with those presented in Tab. A1. I did not check all entries, but two examples caught my eye: A total abundance of 89.2 ind/100 m is given for ROV10 in Tab. 1 and in the text, but summing up all observations in Tab. A1 results in ca. 67 ind/100 m. Another example: For Porifera,

Printer-friendly version

Discussion paper

numbers given in the text match those in Tab. A1 for Rüppel and Senckenberg, but those presented for Heip and Mann Borgese are much lower than in the table (3 vs. 7.5 and 0.68 vs. 1.9, respectively). This has to be checked and resolved.

- Line 42: Insert common definition for "seamount" and citation

- 73: When did the sampling take place?

- 96: What is the difference between "exploration and opportunistic sampling"? More detail is needed.

- 98: What does "whenever possible" mean? 90 % of the transects? What was the target altitude of the ROV, and was it the same at all transects? How did panning and tilting affect the field of view? (see also general comments).

- 94-99: Generally, much more information on the sampling mode is necessary, including sampling strategy (e.g., straight line, deviations for interesting objects etc.), ROV speed, target altitude, field of view etc.

- 104: What is "ID's"? I guess it should be IDs, but "ID" is not defined in the text. Same in line 111.

- 114: Is there a reason that specimens collected were obviously not used for proper identification?

- 118: Which statistical testing? Did the authors use tests other than nMDS? If yes, they have to be described here in detail

- 127: Here and throughout the text: two significant digits are sufficient, for example 7.6 instead of 7.59 or 89 vs. 89.23. The two decimals pretend a non-existing precision of the data.

- 151/152: Aren't Acrocirridae polychaetes as well? ("... Acrocirridae were observed... as well."). Do you probably mean they were observed in high densities in some of the transects?

Printer-friendly version

Discussion paper

- 189/190: This belongs into the discussion.

- 193: Insert ", respectively" after "Table A1"

- 195: "less linear" - how was this assessed? I cannot see any linear or non-linear relations in Fig. 4d, nor can I see any curves crossing.

- 198: What does "small sample size" mean? I think the sample size in this study was always small.

- 205: Should read "least overlap". Explain similarity between these findings and the results from the seamount: For both habitat types, the samples at APEI3 had least overlap with the other sites.

- 220ff: According to Fig. 3 (not Fig. 8!), the majority of ophiuroids on the nodule fields were unidentified

- 244/245: This is not clear. Variation "along the video transects" was obviously not analysed and cannot be seen in Fig. 5. Probably the authors mean "between transects"?

- 251: Kendall's coefficient is not mentioned in methods section. See comment above.

- 255-258: This makes no sense. If sampling depth differs between seamounts and NF, and nMDS distinguishes between seamount and NF groups, then the grouping must correspond also to depth sampled. Omit this paragraph (and Fig. 7b) and state in the discussion that differences between seamounts and NF could be a result of different depths sampled.

- 262: This is not quite clear. Rephrase: "... at different locations and additionally, for the seamounts, different depth ranges." Possible differences in substrate etc. should be mentioned here.

- 269: Rephrase: "... since (mega)faunal communities could be very different even between adjacent seamounts ..."

Printer-friendly version

Discussion paper

- 270: Which parameters? Name examples for depth-dependent parameters which drive faunal composition

- 278: Why would "elevated topography (peaks)" favour Porifera and Anthipataria? Name possible mechanism(s). By the way: Seamounts are per definitionem elevated topography.

- 288-295: Do the authors mean that faunal density is negatively correlated with nodule coverage? This is in contrast to Vanreusel et al 2016, who found higher abundances at higher nodule coverage. So obviously in this study, the driver for the differences in faunal density was not nodule coverage, but probably organic input.

- 296: Grammar: neither - nor

- 319: Clearly distinguish between own data and data from literature by rephrasing, e.g. "Vanreusel et al. (2016) found that ophiuroids. . .."

- 322 : The available data cannot show a gradient, therefore it should read: ">50% less at seamounts compared to nodule fields"

- 331: ". . . studied here."

- 338: How can an uneven distribution (of holothuroids) affect composition?

- 350: This is an isolated statement here - what does it imply?

- 352: And what about nodule-covered areas - did they host these taxa in Vanreusel's or this study? This paragraph is a bit confusing and should be re-sorted, also clearly distinguishing between own results and those of others.

- 361: "communities"

- 362: ". . . were more abundant. . ." - compared to what?

- 370: ". . .they are known to . . ." Citation?

372: Does the reference (Baco 2007) apply to both statements? I suggest to rephrase,

Printer-friendly version

Discussion paper

e.g. "The exception. . .common on seamounts, as also reported in other studies (e.g., Baco. 2007)." Baco 2007 is not in the reference list!

- 373: Insert: ". . . Enteropneusta which in this study were found only on seamounts, were. . ."

Figures

- Fig. 1: What does the "A" in the upper left corner mean?

- Fig. 2: I suggest to add morphospecies "names" (as given in Fig. 3) to the examples.

- Fig. 3: This figure should be simplified. Most of the morphospecies were observed in very low numbers, and in these cases differences between NF and seamounts are difficult to see in the figure and rather not relevant. I suggest to include in this figure only morphospecies and higher taxa with a substantial mean abundance (e.g., >0.5 ind./100 m per habitat type); other morphospecies could be summarised or omitted. By contrast, Table A1 should be extended and present the results for all morphospecies, not only higher taxa (see below).

- Fig. 4: Axis labels are incomplete (units are missing). What does "exact" on the y-axis in panels a and c mean? And what is sample size (units?) in panels b and d? I guess that not sample size was used for the rarefaction curves, but accumulated number of observations. Caption is incomplete: What do the shaded areas in panels a and c and horizontal and vertical lines in panels b and D mean?

- Fig. 5: What does "values are relative" mean? - percent (of what?)? This has to be explained.

- Fig. 6: y-axis incomplete, should include quantity and unit.

- Fig. 7: Omit panel b).

Tables

- Tab. 1: Be consistent with units: here, #obs/100 m is given, whereas throughout the text and in figures and in Tab. A1 the unit for density is ind/100 m.

- Tab. A1: See also comment to Fig. 3. I suggest to list data for all distinguished morphospecies here and sums for higher taxa. It is irritating that densities for higher taxa (e.g., Holothuroidea) are given, but they do not include the identified morphospecies within that taxon. Not identified taxa should be clearly indicated, e.g. Holothuroidea indet., and they should sum up with the distinguished morphospecies to total Holothuroidea, etc. I also suggest to include absolute number of observations in addition to densities in this table. This would facilitate the evaluation whether, e.g., differences are based on a substantial number of individuals, or rely on just one or two specimens. The order of taxa in the table is not clear.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-304, 2019.

Printer-friendly version

Discussion paper