

We would like to thank the reviewer for their thorough read and suggestions. We explained and clarified all the issues raised and incorporated them in the ms when appropriate.

R1: My main concern is that no environmental data are presented. In addition 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.

A: Regarding the reviewer's comment on the description of the sampling sites: There was a supplementary figure (Fig. S1) from which the size of the seamount, the depth of the base and bathymetry can be deduced and which we now have incorporated into Figure 1 (as requested by the other reviewer).

We have added information on the amount of hard substrata under the form of 3 categories: (1) Predominant soft substrata (<40% hard substrata), (2) mix or transition (between 40 and 60% hard substrata) and (3) predominant hard substrata (>60% hard substrata), annotated per 10m distance unit. This has been added to the methods section. The amount of hard substrata was linked with faunal observations in higher taxonomic groups. No significant correlations between substratum type and faunal abundances were found for ROV02, ROV04 and ROV09. This is most likely due to the amount of hard/mix/soft substrata, which were almost equally distributed over the transects (varying between 16-39%, 30-51% and 15-38%, respectively).

This highlights the need for a more in-detail assessment of substratum type, which is currently underway as the geomorphology of the seamount transects is being modelled (based on backscatter data) and investigated in detail, but this falls outside the scope of the current manuscript.

Based on the preliminary substratum results, the following paragraph was added in section 3.1:

"About 57% of all sessile fauna was associated with predominantly hard substrata, followed by 31% on the mixed substrata. For the mobile taxa the pattern was less pronounced with 41 and 42% associated with predominantly hard and mixed soft/hard substrata respectively. The amount of predominantly hard and soft substrata were negatively correlated though, not significantly. This was due to the elevated amount of mixed hard/soft substrata featuring equal amounts 40-60%. Over all seamount transects pooled together, no taxa were significantly correlated with the amount of hard substrata, nor with soft substrata. When looking at the individual transects, no significant correlations were found between taxa and substrata for ROV02 or ROV04 or ROV09, most likely due to the equal distribution of the amount of hard/soft/mix substrata. In this perspective, ROV15 stood out, as it was dominated by predominantly hard substrata (56/%): For this transect, Pennatulacea were significantly negatively correlated with the amount of hard substrata and Zoantharia/Octocorralia were significantly and positively correlated with hard substrata, as were Ophiuroidea, Asteroidea, Crinoidea and Mollusca."

These results, highlight the need for the more detailed geomorphology assessment taking into account much more geological characteristics. This analysis is currently ongoing. We added this in the discussion as well.

Concerning the hydrographical properties, a link with the POC-flux as known for the CCZ area was present in L294-295. Following the comments of the other reviewer, we have added the predominant current direction at the CCZ in the Methods section.

R1: 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.

A: In name of all the co-authors, I think we were very cautious in our conclusions, recognising the sampling shortcomings as well as the limited amount of data. We purposefully stated "*Based on our current knowledge; seamounts appear inadequate as refuge areas to help maintain nodule biodiversity.*" And then urged on for more proper sampling to adequately corroborate or refute observations done here. We recognised the shortcomings throughout the manuscript. We think it is important to take a look at the entirety of the sentences written and to not take parts of them out of context.

Moreover, because of the unknown impacts and extent of mining, we could speculate that only the communities living outside a certain range in distance across the seafloor and upwards in the water column, will be protected from the mining plumes (and other impacts). Hence, it is relevant to also investigate "shallower" areas of the seamounts, as there is a possibility that recolonisation will start from these somewhat shallower areas.

R1: 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 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.

A: Target altitude was 2m above seafloor and travel speed ~0.2m/s, though interrupted by sampling actions, instrument check-ups, exploration, object avoidance (in the case of the uphill seamount transects) etc.

Regarding the comment that ind/100m might not be a good metric, we would like to clarify that the length of the transects was calculated only taking into account the parts of the dive when the ROV was visualising the seafloor. The parts of the dives where the ROV was higher

up in the water column (i.e. >10m altitude) and/or not visualising the seafloor (e.g. Transiting or checking ROV parts or instruments) whilst travelling forward, were omitted out of these calculations, thus granting a best estimative possible and allowing for comparison.

We agree with the reviewer's observation that changing altitude and angle of the camera bias the observations, but by excluding the parts of the transect that were too high up, it is the best approximation possible with the data at hand.

Throughout the paper, we acknowledge the importance of performing standardising video transects and recognise the shortcomings of our study to this respect. However, on seamounts we cannot fly straight all the time, it does not necessarily prevent comparisons between transects, because the bias is systematic across transects. Moreover, while this shortcoming may pose limitations to quantitative comparisons, it does not preclude qualitative comparison between habitats, which are the main objective of this study.

Information on ROV altitude and transects length calculations were added in the Methods section with the following paragraph:

"For the transect length calculation for each dive, we omitted all parts of the video footage in which the ROV was at an altitude of >10m, or sections where the ROV was not visualising the seafloor (e.g. during transiting or inspecting ROV parts or instruments)."

R1: 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.

A: Vanreusel et al. was based on a subset of the BGR, GSR and APEI3 nodule field videotransects/dives analysed here (they investigated 2740 m of the 6100m nodule fields transects as presented here or 44% of our study), and, as stated by the reviewer, they did not study the seamounts. Moreover, and most importantly, Vanreusel et al. did not identify individuals to morphospecies level but stayed at a higher taxonomical level such as Actiniaria, Echinoidea etc.

The following paragraph has been added in the ms for clarification purposes, in methods 2.2.

"A subset of the nodule field transects from BGR, GSR and APEI3 was presented by Vanreusel et al. (2016), and correspond to 44% of what we studied here and limited organism identification to a higher taxonomic level (Order (e.g. Alcyonacea) or Class (e.g. Ophiuroidea)). In our study, the entire transects (100%) were annotated to morphospecies level, allowing a more detailed comparison between seamounts and nodule fields."

Specific comments

R1: Use consequently "Ophiuroida, Asteroida" etc. instead of "ophiuroids, asteroids" etc.

A: Ok

R1: 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, 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.

A: We carefully and meticulously checked all the abundances to make sure they were correct.

R1: Line 42: Insert common definition for "seamount" and citation

A: We added the definition from the glossary of the International Seabed Authority: *"Seamounts are defined as isolated sub-surface topographic feature, usually of volcanic origin, of significant height above the seafloor (International Seabed Authority (ISA), 2019)"*

<https://www.isa.org.jm/scientific-glossary/>

R1: 73: When did the sampling take place?

A: In 2015, this was added

R1: 96: What is the difference between "exploration and opportunistic sampling"? More detail is needed.

A: The words chosen are rather self-explanatory. Explorative dives are dives when a site is visited for the first time and observations made during the dive are key to decide what happens during its course, e.g. sampling when the occasion presents itself or just imagery sampling. It was also mentioned in the text (L113-114).

R1: 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).

A: See answer above

R1: 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.

A: See answer above

R1: 104: What is "ID's"? I guess it should be IDs, but "ID" is not defined in the text. Same in line 111.

A: ID from identification. We altered it.

R1: 114: Is there a reason that specimens collected were obviously not used for proper identification?

A: Samples were used for proper identification whenever possible. Multidisciplinary research cruises such as SO239 based on larger research projects (JPIO) tend to have a multitude of institutes involved, with different or overlapping interests. Samples taken during the cruise were distributed and divided over different institutes, each working towards their own

objectives. The organisms sampled for which we received identifications were incorporated as such, e.g. Porifera identifications as included here. Though as stated in ms L280-282, even when organisms were sampled and identified, they were hard to extrapolate across the video imagery. Same reasoning applied for the Ophiuroidea where many species were revealed based on the samples, though impossible to annotate or differentiate based on the imagery footage (Christodoulou et al 2019). Hence, no information on its abundance, distribution or even presence in other areas than the one sampled can be included. There is no use of having a name for one (sampled) species from one single location if you cannot recognise it elsewhere.

R1: 118: Which statistical testing? Did the authors use tests other than nMDS? If yes, they have to be described here in detail

A: We have added information on the Kendall species Associations test carried out.

R1: 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.

A: Ok

R1: 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?

A: This was corrected.

R1: 189/190: This belongs into the discussion.

A: This is also mentioned in the discussion and it was mentioned here as well to recognise the limited sampling. This links back to the methodological issues as stated by the reviewer, which we fully recognise throughout the manuscript.

R1: 193: Insert ", respectively" after "Table A1"

A: Ok

R1: 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.

A: Replaced linear by straightforward. Curves cross at smaller sample sizes (<100 individuals) for ROV13,08 and 10.

R1: 198: What does "small sample size" mean? I think the sample size in this study was always small.

A: Less than 100 individuals, this was added.

R1: 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.

A: Ok

R1: 220ff: According to Fig. 3 (not Fig. 8!), the majority of ophiuroids on the nodule fields were unidentified

A: This was corrected to *“The majority of the very abundant Ophiuroidea observed at the CCZ seamounts were small and situated on hard substrata (morphospecies 5), while most of the Ophiuroidea at nodule fields (including morphospecies 6) were observed on the soft sediments. Morphospecies 6 was only rarely observed on the seamounts (Fig. 3)”*

R1: 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"?

A: This was corrected to *“among the video transects of both seamounts and nodule fields”*

R1: 251: Kendall’s coefficient is not mentioned in methods section. See comment above.

A: This was added, see reply above

R1: 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.

A: It was already stated in the discussion, but in our opinion it is a visual presentation of this statement, which is why we decided to keep it for now.

R1: 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.

A: Change was carried out

R1: 269: Rephrase: ". . . since (mega)faunal communities could be very different even between adjacent seamounts . . .

A: Ok

R1: 270: Which parameters? Name examples for depth-dependent parameters which drive faunal composition

A: *“parameters that vary with depth, such as temperature, oxygen concentration, substratum type, food availability, and pressure”* This was added.

R1: 278: Why would "elevated topography (peaks)" favour Porifera and Anthipataria? Name possible mechanism(s). By the way: Seamounts are per definitionem elevated topography

A: *Peaks are more exposed and appear thus more advantageous for filter feeders such as Porifera and Antipatharia. This is mentioned in the text.*

R1: 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.

A: *We searched for a possible explanation as to why APE13 stood out and found that this difference, besides their more northward location under more oligotrophic waters (mentioned in L294-295), corresponded to a difference in nodule coverage. The patterns by which nodule coverage or densities influence the ecological patterns are still poorly understood. The nodule coverage data as mentioned in our ms originate from Table S1-1 from Vanreusel et al. 2016,*

and indeed in the body of text these authors reported higher epifaunal densities in areas with dense nodule coverage, reporting >25 versus ≤10 in sessile individuals per 100 m² for nodule rich and nodule free areas respectively. Nevertheless, if we compare the nodule coverage from Table S1 to figure 3 (both from Vanreusel et al. 2016) same patterns as those described in our study are observed, namely: Higher nodule coverage in APEI3 and lower densities both for sessile and mobile fauna. It is possible that Vanreusel et al. made their statement by looking at the license areas only and not included the APEI in this comparison. Based on this, we chose to keep our statement as written in L285 and next.

R1: 296: Grammar: neither - nor

A: Ok

R1: 319: Clearly distinguish between own data and data from literature by rephrasing, e.g. "Vanreusel et al. (2016) found that ophiuroids. . ."

A: Ok

R1: 322 : The available data cannot show a gradient, therefore it should read: ">50% less at seamounts compared to nodule fields"

A: This was corrected

R1: 331: ". . . studied here."

A: Ok

R1: 338: How can an uneven distribution (of holothuroids) affect composition?

A: Unevenly distributed organisms can give different perceptions in sampling. Organisms with a wide distribution range can, when unevenly distributed, be present/absent in adjacent sampling localities, thus resulting in different faunal composition for these sampling locality.

R1: 350: This is an isolated statement here - what does it imply?

A: We moved it to the beginning of the paragraph which now starts off as follows: "Stalked organisms, such as Crinoidea (Echinodermata) and Hexactinellida (except for Amphidiscophora, Porifera) rely on hard substrata for their attachment and are considered being among the most vulnerable organisms when mining is concerned."

R1: 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.

A: This paragraph was re-written (taking into account the comments of the other reviewer) to make the distinction between our results and those from literature more easily:

"Corals were considered to be more abundant on seamounts than adjacent areas, due to their ability to feed on a variety of planktonic or detritus sources suspended in the water column, (Rowden et al., 2010). In this study, the Alcyonacea densities were lower at the seamounts than on the studied nodule transects. The Antipatharia were very abundant at the Mann Borgese seamount (APEI3) compared to all other transects, seamounts and nodule fields. The depth difference of more than 3000m between this particular seamount and the nodule fields could explain the abundance in Antipatharia which were shown to be more abundant at lower depths (Genin et al., 1986). The more abundant Antipatharia and Alcyonacea morphospecies of the

seamounts did not occur on the nodule fields and vice versa, with exception of Callozostron cf. bayeri which was present at the nodule fields but in very low densities (1/10 of those observed at seamounts). Additional presence of Pennatulacea, which were virtually absent from the nodule field transects and appeared more linked to predominant soft substrata at seamounts, resulted in completely distinct coral communities for each ecosystem. "

R1: 361: "communities"

A: Ok

R1: 362: ". . . were more abundant. . ." - compared to what?

A: "... more abundant than on seamounts". This was added.

R1: 370: ". . .they are known to . . ." Citation?

A: This statement was based on our personal observations. We have rephrased it and changed the structure of the paragraph as to convey our point more clearly:

"Some taxa were only observed on the seamounts in this study, while they occurred on nodule fields elsewhere, be it in low densities. For instance, Enteropneusta, which in this study were found only on seamounts, were observed previously at CCZ nodule fields though observations were rather rare (Tilot, 2006). They appeared more abundant at the nodule fields of the Deep Peru Basin (DISCOL area), though a wide range in abundances was displayed there as well (Bluhm, 2001). The exception were the Scleractinia, which were quite common on seamounts, as also reported in other studies (e.g. Baco, 2007, Rowden et al. 2010), but absent at nodule fields."

R1: 372: Does the reference (Baco 2007) apply to both statements? I suggest to rephrase, 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!

A: Baco 2007 refers to the Scleractinia being common on seamounts. We have clarified this and added the reference for Baco 2007 to the reference list.

"The exception were the Scleractinia, which were quite common on seamounts, as also reported in other studies (e.g. Baco, 2007, Rowden et al. 2010), but absent at nodule fields."

R1: 373: Insert: ". . . Enteropneusta which in this study were found only on seamounts, were. . ."

A: Ok

Figures

R1: Fig. 1: What does the "A" in the upper left corner mean?

A: Figure 1 underwent some change taking into account the comments from the other reviewer.

R1: Fig. 2: I suggest to add morphospecies "names" (as given in Fig. 3) to the examples.

A: Their morphospecies names were added to the caption.

R1: 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).

A: Figure 3 was withheld because it was considered a key figure to show how different the presence/absence/abundance of the fauna varies between the two ecosystems, but the 3 parts were separated more clearly with different breaks at the X-axis, thus enhancing readability and interpretability.

Table A1 now includes the morphospecies densities as well.

R1: 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?

A: Sample size is the number of individuals observed (or number of observations as you will). Information was added to the caption and axis were renamed and/or clarified.

R1: Fig. 5: What does "values are relative" mean? - percent (of what?)? This has to be explained.

A: Values are relative due to different transect lengths and differences in richness. This was changed to "Values are indicative rather than absolute due to different transect lengths and differences in richness."

R1: Fig. 6: y-axis incomplete, should include quantity and unit.

A: I am not sure what this is about, since the Y-axis is complete and has ind/100m as title.

R1: Fig. 7: Omit panel b).

A: See comment above

Tables

R1: 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.

A: The number (#) of observations is more of a methodological way to describe it, since it was used prior to the results. This could be changed.

R1: 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

A: Morphospecies densities (ind/100m) have been added to the table. We opted for densities to correspond to the data used in the manuscript. Absolute numbers are easy to deduce since the length of the transects are given in Table 1. The order of taxa in the table was reorganised alphabetically.