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May 30, 2024

RE: Environmental Impact Statement – Testing of Polymetallic Nodule Collector Vehicle in the Block A-5 of the Minmetals Contract Area

Sir/Madam,

Below, please find our Commentary on the Environmental Impact Statement from China Minmetals Corporation, open for consultancy in May this year.

As Group Leads, we submit on behalf of the **Deep-Sea Minerals Working Group of DOSI, the Deep-Ocean Stewardship Initiative**. The list of contributors is presented at the beginning of the document. Express Consent for sharing is granted.

Sincerely,

A handwritten signature in blue ink, appearing to read 'Sabine', written over a horizontal line.

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COMMENTARY ON
“Environmental Impact Statement – Testing of Polymetallic Nodule Collector Vehicle in the Block A-5 of the Minmetals Contract Area”

PREFACE

The **Deep-Ocean Stewardship Initiative (“DOSI”)** integrates science, technology, policy, law and economics to advise on ecosystem-based management of resource use in the deep ocean and strategies to maintain the integrity of deep-ocean ecosystems within and beyond national jurisdictions. DOSI gathers expertise across disciplines, jurisdictions and industrial sectors to foster discussion, provide guidance and facilitate communication. As a distributed network, DOSI has over 700 members from 40 countries.

- DOSI was granted Observer Status at the 22nd Session of the ISA in Jamaica in 2016.
- DOSI gives Express Consent to the China Minmetals Corporation to make this submission publicly available.

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SECTION A. GENERAL COMMENTS

General comments

- The structure of the EIS is clear. The provided data is a welcome addition to the growing body of information from the area, and crucial in obtaining a useful baseline for the environment. The contractor is commended for collecting the data, including the number of days spent at sea.
- There is, however, insufficient evidence that supports the statement that the IRZ and PZ1 are similar (The statement from the EIA: *"The delineation of the IRZ and PZ1 is based on similarity of topography, the geological characteristics of the sediments, and environment baselines"*). **The statements that environmental baselines suggest that the IRZ and PRZ are similar is misleading and cannot be supported by the information presented. There remain large gaps of knowledge in the presented data**, some of which may come from the limited information presented, while the text suggests there is more information available. For example, while in chapter 4 the authors regularly make reference to the number of species found for specific groups, the analyses do not show these data at a species level, but instead at such a high taxonomic level that it is uninformative for management. No raw data is shown in the main text, and no reference has been made to the potential for some more detailed information in the appendix. Further, these data are rarely presented separately for the IRZ and PRZ.
- It is unclear from the text which data were collected for this EIS and which are secondary data. More clarity of references is required, including reference to field reports for specific vessels or project years and other supporting reports.
- There are four major data gaps concerning chemical oceanography and biogeochemistry, and additional major knowledge gaps in the biological environment.
- Below are 10 points where specific information is lacking:
 1. While it is true that the presented full-ocean-water column chemical profiles suggest similarity between the three casts done within the PRZ1 and the three casts done within the IRZ, all of these are only from the late summer-fall season (see Tables 4.3-1 – -3); there are no data from the late winter-early summer season from any full-depth CTD data, which is when the one sediment trap mooring data (only in IRZ) indicate sediment flux is highest (see page 150), and also no data from the PRZ2.
 2. The report contains no sediment oxygen profile data (a recommended measurement of ISBA/27/C/11), no water column dissolved organic carbon or total organic carbon data from either the PRZ1, PRZ2 or IRZ, and no water column total suspended matter or particulate organic carbon data from the PRZ1 or PRZ2.
 3. No table is provided of coring operations, so it is impossible to determine whether the reported data reflects conditions within the PRZ1, PRZ2, or IRZ.
 4. There is no AUV optical data from the IRZ, and only very minor coverage in the PRZ1 (see Figure 4.5-9) and none from PRZ2.

5. The description of the biological environment completely lacks sufficient description of microbial communities in the water column or on the seabed. No mention is made of the potential impacts and mitigation requirements of bacterial and archaeal activity, despite the large unknowns and potential for impacts this large biomass fraction contains, e.g. for carbon and toxic metals cycling. This is an oversight that needs addressing (See [Orcutt et al., 2018](#)). In addition, no baseline data were collected on the micronekton, and thus also no inference can be made on the effect of the ecosystem services they provide, including fisheries support and active carbon export.
6. There are no data provided on ecotoxicology (as recommended in ISBA/27/C/11).
7. The very low amount of samples taken for sediment meiofauna and the very limited analyses (on higher taxa level), does not allow for any robust comparison between the IRZ and PRZs. Identification beyond higher taxa level is needed to identify if biodiversity in IRZ and PRZ are similar.
8. The macrofauna data presented are also on higher taxa level only, which does not support informative management.
9. Seawater sampling stations (eDNA) are limited in number and analysis does not show similarity.
10. No seabed pictures were presented for the megafauna, and instead a great effort was made to compile pictures of other studies while their own set was readily available to present. As a result, we cannot determine if the pictures were of sufficient quality to even proceed to the results that were presented. However, the much higher proportion of large organisms (e.g. holothurians: 33%) compared to smaller organisms (e.g. cnidarians: 12%), is an unusual result compared to the opposite trend seen in the western CCZ (cnidarian ~ 30 to 40% and holothurian ~ <10-15%; Durden et al. 2021; Simon-Lledo et al. 2023). Caution is therefore needed on representativity of this study, as its reliability cannot be assessed, whereas the overestimation of larger organisms might be due to insufficient image quality.

To conclude, ***the baseline physicochemical environment and biological environment data are not sufficient for making claims of similarity of the PRZ and IRZ, nor suitable for baselines for the environmental monitoring planned.*** This will limit the usefulness of the IRZ and PRZ design to monitor for potential impacts. Presented data (across the groups) and methods of analyses are not of sufficient detail presented to determine whether the statements made in the document are supported, and for certain key variables the data are lacking. There are no plans presented to close these gaps in the EMMP. Thus, the use of this EIS to inform on the effects of the impacts created by the current collector test as well as the use as basis for future EIA under exploitation is questionable.

SECTION B: ITEMIZED COMMENTS

Specific comments	
<i>Must include the page number from the EIS report for reference</i>	
Page	Comment
Exec. Sum.	
I	<p>It is stated that (i) results of this EIS will be used to optimize the design of the collector vehicle, (ii) environmental monitoring is carried out alongside the testing, and (iii) a technical system is established to provide data in support for EIA of commercial mining. The document, however, does not provide the details of how this upscaling would occur and its feasibility. Given that the presented quantity and quality of baseline data is limited, this information must be provided to build confidence that it can be done in an appropriate way.</p>
V	<p>The total number of days spent at sea is high (537 days). However, the sampling effort is very skewed. Whilst many box-corer samples (208 stations) were taken over this time, all other sample gears were much less used (indicated by the number of stations ranging from 2 to 27). This is an issue because of the high small-scale spatial variability in the deep sea, and the associated fact that sample sizes of less than 5, and even often also <10 or even <20 (per site, so not in total) are typically not sufficient to capture this natural variability.</p> <p>The number of box-corer samples (208) mentioned in the summary is different to what is given in table 1.4-2. For A5 22 box-corer samples are given. Please clarify the difference.</p> <p>“There is no obvious steep slope”. Can you provide the slope value here? The term “steep” is very relative.</p> <p>The nodule abundance shows high variation in the TMA (0.59-25.92 kg/m²). How does this high variability relate to the sampling strategy chosen?</p> <p>What is meant by shipboard photography?</p> <p>It is stated that IRZ and PRZ have similar nodule abundance (10-15 kg). Is this statistically tested? Could you provide any other information about the similarity of the nodule sizes?</p>
VI	<p>Info on TOC in PWA is given, including the variability. It is stated that TOC is</p>

	<p>similar in IRZ and PRZ1, but it is not clear whether (i) it is also similar to PRZ2, and (ii) similar to which part in PWA?</p> <p>It is unclear what sampling was done for TOC and grain size. Since only 1 GC and 1 MUC were collected per IRZ/PRZ, this appears to be box coring. Which sediment interval was sampled? It should be noted that box coring will lose the uppermost 1-2 cm, hence the most reactive fraction and fine grain size. Sampling with a multiple corer would have been the appropriate method.</p> <p>“concentrations of heavy metals are generally low”. Please be more precise, as “low” is relative.</p> <p>Overall, there is a mismatch to what level of detail is given for results. Some show results from the PWA, others from IRZ and PRZ (without putting them into context of PWA), some refer to single operations without giving the location. This hampers the usefulness of the EIA as the data and associated statements cannot be verified for the support. It limits the usefulness of the EIA and provided baseline information for the assessment of potential impacts.</p>
<p>VII</p>	<p>It is unclear how the interannual variation of the physical oceanography characteristics were evaluated in the whole of Block A5 given that there were a total of 13 CTDs and 4 moorings, and it seems only 2 CTD samples were taken in IRZ and PRZ. Please clarify.</p>
<p>VIII</p>	<p>The variation of POC with depth is suggested to be correlated to strong plankton activity (Chl). 12 stations in total were measured. Were plankton samples and POC taken at the same time and area? No information on sampling location is provided apart from “block A-5”.</p> <p>Is the Chl in the IRZ, PRZ1 and PRZ2 similar in spatio-temporal variability? How does this relate to the (different) results presented one paragraph above, where high variability of POC (explained with Chl variability) is reported? Also, how is the (low) number of samples treated as this of course increases uncertainty?</p>
<p>IX</p>	<p>“meiofauna communities are similar in IRZ and PRZ”. Only analyses on higher taxa were presented, which does <u>not support</u> a statement that “communities” are similar.</p> <p>“meiofauna abundance is similar to...”. The limit number of stations sampled (25 multicorer stations) may not have been sufficient for this statement (see e.g. Uhlenkott et al. on sampling replication; they used 319 corers from 88 multicore deployments). However, later in the document, it is stated that only 4 and 4 (or</p>

	<p>2?) samples are compared? Please clarify why these numbers are so different and which the correct ones are. Without a clear idea of how many samples were included in the analyses it is impossible to verify whether the presented data and associated statements are accurate.</p>
IX	<p>Please check the number of the phylum, as some names are replicated</p>
X	<p>While potential impacts are given, it would be good to add the specific literature examples that support the assumptions (or refer to other parts in the EIS).</p> <p>The collector testing will drastically change the sediment biogeochemistry in the biologically active (i.e. bioturbated) layer, and this impact lasts for many centuries to millennia (as documented by scientific literature). In addition, this will have effects on ecosystem functions and ecotoxicological consequences.</p> <p>How is the numerical model on plume dispersal backed up by data, given that only 4 mooring stations were present? Were any other instruments next to mooring used? A reference to the type of model used would be very useful. The information presented in appendix 1 is not sufficient. For example, information on current speeds were used, but the data presented in this EIA also show that currents regularly changed directions, and it does not seem that this has been taken into account</p>
XI	<p>It is important to clarify the taxonomic level that is shown. In meiofauna studies, it is common to clearly state the level of identification (genus for nematodes, families for Polychaeta). This has not been done here, or throughout the EIA.</p>
XIII	<p>Oxygen is mentioned as a ‘key environmental parameter’ but the sole focus seems to be on water column observations. Effects on bottom water oxygenation are expected to be minute and transient and will be hardly resolved based on titration of discrete samples. For an assessment of consequences of the collector test, monitoring of sediment oxygenation and sediment oxygen uptake would be much more informative. This would address potential effects on organic matter remineralization and benthic community activity and would also provide direct evidence whether or not settlement of resuspended fine material leads to a ‘suffocation effect’. The lack of benthic oxygen observations that are recommended in relevant ISA documents (ISBA/25/LTC/6/Rev.3, ISBA/27/C/11) is a major shortcoming of the EIA and the underlying baseline study.</p>
XIV	<p>It is excellent that stakeholder opinions are made publicly available. Could you give the name of the platform (e.g. website) where this information will be found?</p>

	In addition to the term “opinion” the term “analyses” should be added, as a scientific review is not an opinion but an analysis.
Chapter 1	
6	It is unclear what the 15.5% of the total species represent - species numbers, abundance or something else? Please clarify.
6	Ocean water species is an unclear definition. What is meant by ocean water species?
6	It is unclear what dominance was based on - are the nematodes numerically dominant in the meiofauna?
6	Even though it was mentioned that 727 species were identified, the data about Nematodes is not clear. Were they really in the species list? If not, a list of the genera (at least the most abundant) could be helpful.
6	In the executive summary, annelids were listed as the dominant taxon in the macrofauna, but here it is crustaceans. Which one is it? Based on what?
7	What supports the claim that “Block A-5 can represent the overall environmental characteristics of the western part of the CCZ”? The environmental survey effort in different blocks is not balanced and thus caution must be taken in direct comparisons of the environmental features inferred (Table 1.4-2).
7	Towed camera surveys are not listed in table 1.4-2
Chapter 3	
38	<p>Please add further support to the statement “this Project can ensure that the collector vehicle test will not impact the sediment environment of PRZ1” besides the simulation results. See comment above about executive summary p. III and impact on PRZ1 for more detail.</p> <p>What does “upstream” actually mean? In the deep sea (as also evidenced in the presented data in Ch. 4.6.4.4) the bottom currents go in all directions. There is no preferential current direction evident in the presented data, and hence it will be very difficult to predict where the plume may go during the time of the trials. This issue is not addressed in the plume modelling.</p>
66	Deducing sediment properties for a site-specific EIS from a published global map (with naturally very coarse resolution and little to no data points in the region of interest) is hardly sufficient. Can you provide your own analyses? How do these compare to the global map?

Chapter 4	
70	The PRZ is stated as being upstream of the area. However, the report states that hydrodynamic conditions however are highly variable in speed and direction. How was this taken into account in the plume impact model and reference station placement?
72	The depth stated here for the lowest DO concentrations differs compared to that in the executive summary. Which depth is correct? What is the depth of the core of the low oxygen concentration (in later chapters called the Oxygen Minimum Zone, but not here)?
73	<p>The report states that “about 316 stations and several survey lines”. What is the exact number of stations and lines for Block A-5?</p> <p>The report states ‘They mainly include’ completion of various sample collections. What exactly was completed in Block A-5?</p>
75, 76	What exactly is “water depth” – LAT, MSL etc.?
80	Figure 4.3-4: it is difficult to see how many boxcores were taken (and how this relates to info in Table 1.4-2 (22 bc) and Table 1.4.1 (208 bc)). Also, the number of multicores visible in the Figure is 9 (and thus lower than the 25 given in table 1.4-2). It is not clear how many samples were taken in IRZ and PRZ 1 and PRZ 2. According to the Figure, not a single sample was taken in PRZ2? Please add a clear table on what samples were taken when and where and justify why the sampling effort differed between the three regions of interest.
94	Figure 4.5-7: the figure shows that nodule abundances (weight) may be different in IRZ and PRZ1. PRZ1 shows 10-15 kg in the whole area, whilst the IRZ shows for ~75% 10-15 kg and ~25% (a rough estimate by eye) 15-20 kg. How did this influence your sampling design?
95	<p>Figure 4.5-9 gives nice AUV images on nodule coverage (showing high variability). However, this info is not presented for IRZ and PRZ? Is this info available or not presented for other reasons, and if the latter, what are these?</p> <p>What sediment intervals were sampled for the grain size, TOC and others that were used to create the maps (Figs. 4.5-10, 4.5-11)? These variables should be reported as depth profiles and cover at least the upper 0-10 cm (in 1-2 cm intervals). This is the depth that can be expected to be suspended during the mining and hence is relevant for the impact assessment and the sediment plume dispersion modelling. It should be noted that MUC coring would be the appropriate method, not the box coring (because the top 1-2 cm are lost in that method).</p>

96	<p>What is the relevance of the knoll chain data (discussed here) with respect to the CTA, which is quite far south of it?</p>
97	<p>Figure 4.5-11 are these modeled data? If so, please state this in the Figure legend and add how many actual samples were taken. Please also add locations of IRZ and PRZs.</p>
98	<p>Where are the GCs (presented in Fig. 4.5-12) located? Are they relevant and representative for the CTA?</p> <p>When retrieving a gravity core it is typically placed horizontally on the deck of the ship, hence the sediment surface is lost (flowing away), typically this concerns the upper 20-50 cm. From the presented TOC/TN data this is also apparent when comparing to the values presented in the map Fig. 4.5-15). GC data is only relevant to understand the biogeochemical processes occurring well below the bioturbated layer.</p>
100	<p>Figure 4.5-13 are these modeled data? If so, please state this in the Figure legend and add how many actual samples were taken. Please also add locations of IRZ and PRZs.</p>
102	<p>Figure 4.5-15 are these modeled data? If so, please state this in the Figure legend and add how many actual samples were taken. Please also add locations of IRZ and PRZs.</p>
103	<p>No methods are given for how the estimated chemical gradient maps were created, therefore comparability and replicability are compromised.</p>
105	<p>The Chla data of MUC-02 and MUC-06 are actually quite different. What is the small scale variability in the IR and PRZ1? Are they representative cores for the areas?</p>

110-111	<p>It would have been useful to sample the porewater in higher depth resolution. In the chosen very crude resolution you can easily miss important reactive layers. For example, nitrite (NO₂) as a metabolite during organic carbon remineralization will show a peak in the shallow subsurface (within the upper 5 cm), which is an important indicator of the microbial activity on labile organic carbon. As a consequence of your sampling interval this important information is lost.</p> <p>The nitrate (NO₃) profiles indicate that the IRZ and PRZ1 are quite different from each other, i.e. not similar as you state. This is corroborated by the other presented biogeochemical data (TOC, Chla). The IRZ has more than 2 times higher Chla (a labile organic carbon compound), POC remineralisation leads correspondingly to higher NO₃ level in sediment, and bioturbation is also more pronounced.</p>
112	<p>Stating average TA values for a sampling interval of 15 cm (0-15 and 15-30 cm) is quite meaningless. TA is a variable that informs about POC remineralization as well as mineral reactions and a much higher sampling resolution (1-2 cm intervals) is needed to make it meaningful.</p>
114	<p>In slowly accumulating deep-sea sediments ²¹⁰Pbex is not sufficient to determine the bioturbation depth. Its half-life time is too short compared to the age of the bioturbated layer. It would be necessary to additionally determine ²³⁰Th (or another longer lived isotope). Your GC01 data indicates this shortcoming.</p> <p>No bioturbation activity was determined and also the sedimentation rate was not determined. This is essential baseline information to characterize the environment and habitat. See also the relevant documents ISBA/19/LTC/8 and ISBA/27/C/11.</p>
Chapter 5	
160	<p>Please define the depths for surface (same as upper in previous chapters?), middle and bottom “layers of marine environment”. It is unclear what depth is meant by ‘upper-layer’ for upper-layer biological communities. Please define this depth and remain consistent throughout the document.</p>
161	<p>Please list the specific biological environmental parameters used in the Kaplan Project and CeDAMar project for clarity.</p>
162	<p>It is unclear what is meant by relevant studies. Please cite these studies appropriately.</p>
162	<p>The last paragraph on this page lacks citations. For example, the studies mentioned before ‘on 12-24 samples’ are not identified here, nor the study that</p>

	showed that ‘species with only 1-2 individuals’, nor the molecular and morphological studies referred to. Please cite appropriately.
163	No systemic survey for seabirds and marine mammals has been carried out. Any comment on densities is thus speculation.
163	The first sentence on this page (starting with ‘generally speaking’) is not supported by data, statistics and/or citations. Please add this to the document so that this statement is supported.
163	Plankton is mentioned in the ‘studies completed’ section, but it is unclear what kind of plankton is referred to. Please indicate whether you are talking about phytoplankton, zooplankton or other types of plankton.
163	Micronekton, an important component of the midwater biological community, has not been studied in this EIA. This should be explicitly stated. While the appropriate chapters regarding assessing impacts state that no discharge plume is created during the test and therefore impacts on midwater communities will be minimal, baseline environmental data collection ought to appropriately study the water column.
163	The last paragraph on the page lacks a citation that supports the statement that primary production of the upper-layer ocean (what depth is this?) can be used to deduce the densities of seabirds and marine mammals. This seems like a very simplistic way to determine whether these animals occur here. Further, the fauna listed in the bracket are inappropriately placed and should be after prey.
163	What kind of “biodiversity testing” was done?
163 & 164	There is a <u>significant lack of data shown</u> . No species list, no numbers, no dates of observations, no spatial coordinates, etc. have been listed. The only information that is stated that boobies (what species of boobies?) are the most dominant ones, and what is dominance based on, numerical dominance? If these details are to be found in an appendix, the text needs to refer to this.
164	It is possible that the lower numbers observed of seabirds, marine mammals, reptiles or sharks, is because there was no systematic and detailed survey or dedicated person observing these animals (as stated on 163). This should be explicitly mentioned given that on the NORI-D expeditions there was a dedicated person recording observations. <u>The absence of sightings in A5 can therefore not be concluded as an absence of fauna but can easily be the result of poor record keeping.</u>
164	Without explicit numbers (average and standard error ideally) of the NORI sites, it is not acceptable to state that the Chl concentrations are lower in A5 than in NORI-D. You should provide these data.

164	It is unclear what satellite data were used - what satellite was used, and were the data treated in any way? This is for both paragraphs in the remote-sensing section. If these details are to be found in an appendix, the text needs to refer to this.
164	It is unclear what the error represents after 0.08. Is this standard error? This ought to be defined.
165	It is stated that there is no significant difference in Chl concentrations between IRZ, PRZ1, and PRZ2, but it is not stated what test was used, or what the test results were.
165	Here, too, is a mention of satellite remote-sensing data that were collected, but no mention of which satellite was used, or how the data were treated. Please add this information. If these details are to be found in an appendix, the text needs to refer to this.
167	It is unclear what CTD and sensors were used. Please add the make of this to the text. If these details are to be found in an appendix, the text needs to refer to this. While it is admirable that a larger area was studied, it seems like only one CTD profile was performed for A5? Meaning, there is no information on seasonal or inter-annual variation regarding the types of data collected from the CTD?
168	It is unclear from the information presented when the CTD samples were taken. Only the year is provided, but not the actual date. Please add this information. This is especially important given that only one CTD was taken - is this sample representative of the time the potential collector test is going to be conducted? If these details are to be found in an appendix, the text needs to refer to this.
168	Fig5.4-4. On how many samples is this analysis based on? 1 in each block? Correct?
168	Based on 2 samples, it is stated that vertical variation of Cha is consistent. Comparing 1 sample with 1 other sample is not sufficient for this statement.
169	How was the presence of photosynthetic pigments, their identity, and their concentrations determined? The methods are not described in the main text. If these details are to be found in an appendix, the text needs to refer to this.
169	It is unclear what is meant by the statement that the maximum pigment concentration layer depth is consistent with the maximum Chl layer depth. The maximum Chl concentration is shown in fig 5.4-5 to be at 50 m as a strong peak while in the pigment profiles show there is either a strong peak deeper than 50 m or there is a hump which starts at 50 m.

170	Is the distribution of pigments based on 1 sample in Block A-5?
172	4 stations for phytoplankton, 59 species total. Please provide the complete species list (Table 5.4-1 on page 174 only shows the 6 dominant species)
172	What were the dates of the phytoplankton net tows? The only information on timing stated is the year. If these details are to be found in an appendix, the text needs to refer to this.
172	The methods of the phytoplankton net tows are unclear based on the information in the main text. If these details are to be found in an appendix, the text needs to refer to this. It is unclear whether multiple samples at each station were taken, it is unclear how long the tows were, what volume of water was filtered, how deep the tows were, or what the trawl speed was. It is unclear how the samples were treated, including preservation. It is unclear how the species were. There is a lack of intra (seasonal) and interannual variation in the data.
172	The high level taxonomic grouping is uninformative for the interpretation of the data. It does not demonstrate whether there are community composition or structure differences as these ought to be at a much finer taxonomic resolution (species, ideally). Given that the species have been identified according to the text, it is disappointing not to see these data. Further, as it is unclear whether only one tow was done per station or not, it is unclear whether there is any uncertainty estimate surrounding the species number. There has not been an accumulation curve, which of course would have been difficult on the limited number of samples if it is indeed 4 tows, but it means there is no assessment of the potential number of species that could have been found.
173	The analysis cannot be interpreted as no data is presented. There is no complete list of species found, and where. It is unclear what methods were used to determine dominance, and therefore what the significance/meaning is of the 0.02 threshold. If these details are to be found in an appendix, the text needs to refer to this.
174	The methods of the zooplankton net tows are unclear based on the main text. If these details are to be found in an appendix, the text needs to refer to this. It is unclear whether multiple samples at each station were taken. There is a lack of information on the mesh size of the nets, the tow lengths and time, what volume of water was filtered, tow depths - especially for the multinet it is important to know what depth layers were samples - and trawl speeds. There is no information about the timing of the tows, no spatial coordinates, etc. There is a lack of methods described for the preservation of the samples and identification of the species. It

	is unclear where the samples were taken and whether samples have come from both the IRZ and PRZ1
174	193 zooplankton species identified. Please provide a complete species list.
175	It is not appropriate to present the community composition all grouped together rather than by tow to understand if there are spatial differences or not. Further, this work has been done on a very high taxonomic level while the species are identified according to the text. It would be more appropriate therefore to show those data accordingly.
175	Estimates are presented in the text which only represent an average of all samples together (presumably) - where are the error estimates? Where is the information on the difference (or not) between the net samples? For example, in the last paragraph, a statement is made that the biomass of the zooplankton varies only slightly, but only an average is given. This cannot be checked with the information presented.
176	eDNA analysis of epipelagic fish is presented. However, no methods are provided. Therefore, this data is meaningless, as pore size, water volume, replicates, collection depth, collection method, laboratory methods and QC methods are necessary to evaluate data quality. Additionally, the extremely low density of samples across a large geographic area are insufficient to provide any meaningful results. No reference is made to the limitations of this data (especially in relation to the large area covered and low sampling density) or comparison between eDNA and other analysis.
176	The average zooplankton biomass was 39 mg/m ³ (0 – 200 m); average zooplankton biomass (0-200 m) when using multineets, was 76 mg/m ³ . That is double the amount as gained with the towed sampling. Why? Different sampling times, or location?
176	It seems both day and night samples have been taken for the zooplankton. How many samples were taken for the night and day? What time of day were the samples taken? What depth layers have been sampled? What volume of water was filtered?
176	The data on zooplankton against depth were not represented, not for abundance, biomass, or species richness - as a minimum - and the statement at the top of page 176 can therefore not be verified. Does the biomass decline with depth?
176	Were no samples taken deeper than 200 m? No information is presented on the abundance or biomass estimates of zooplankton deeper than 200 m. Why was

	only this depth layer studied? Section 5.4.1 is a surface section (or apparently an upper layer section), but samples from deeper are mentioned here, which is confusing. It is unclear which samples are discussed in the 5.4.1.4 Zooplankton section. There are estimates from 0-200 and 0-50. What data is behind these estimates?
176	Please specify what is meant by ship-mounted equipment? When were the samples taken and at what time of day?
176	Like for the other samples, it is unclear when the samples were taken or how many, or how the samples were treated. What markers were used, ones specific for fishes? If these details are to be found in an appendix, the text needs to refer to this.
176	It is stated that the samples were taken in the surface layer, but the species listed are deeper species that come to the surface at night. Were the samples taken deeper than the surface or not? Or were they taken at night only?
177	What kind of cluster analysis was used? Were the data transformed in any way prior to the clustering? What program was used to performed the clustering? If these details are to be found in an appendix, the text needs to refer to this.
177	Strong conclusions (but no geographical differentiation) are drawn based on a small number of samples. The dendrogram in Figure 5.4-14 actually shows very low similarity between all samples, as all have less than ~45% of similarity (so actually all are dissimilar). There are two “groupings” (F,A,B, CCZ and D,C,E) visible in the dendrogram.
178	It is unclear what is meant by middle layer. What depth does this imply?
178	Figure 5.4-15 as it is unclear what depth layers were sampled with the multinet, it is unclear how the averages here are derived. Further, given that a net in a multinet samples a depth layer, it is surprising to see an average for a specific depth. Please explain how this is possible as this may misrepresent the data you have. Further, this implies only one sample per depth (layer) was taken? If not, please add error bars as appropriate. If these details are to be found in an appendix, the text needs to refer to this.
178	It is surprising to see a finer resolution in depth on the OMZ layer here compared to the previous chapter. Which one is correct?
179	It was stated that multiple samples were taken and later on it became clear that there were both day and night time sampling. Yet, in figure 5.4-16 both these factors are not apparent. How do the data compare when they are split in the

	appropriate day/night category? Where are the error bars? Further, it remains unclear what depth layers per net were sampled. Overall, these methods are unclear.
179	The text makes a statement about how the community structure of the zooplankton varies, but no data are shown to support or verify this statement. Show the data. No complete species list is given per site with their abundances, meaning that the addition of ‘etc.’ at the end of the page
180	The report uses the Shannon-Weiner metric of alpha-diversity, however no reference is made to whether this is a meaningful metric for compositional data, and how it differs from the same metric as derived from taxonomic data.
180	It was stated that multiple samples were taken and later on it became clear that there were both day and nighttime sampling. Yet, in figure 5.4-17 both these factors are not apparent. How do the data compare when they are split in the appropriate day/night category? Where are the error bars? Further, it remains unclear what depth layers per net were sampled. Overall, these methods are unclear.
180	From the description, only two microbial stations were taken in 2021. The claim that “current sequencing depth can cover most prokaryotic microbial groups” only applies down the sediment profile. This must be clarified in the text. In addition, data from one station only is presented. This is inadequate for any meaningful microbial assessment or baseline across the entire site.
180	There are significant data gaps regarding microbial community analysis: <ol style="list-style-type: none"> 1) No microbial data from the mid-water habitat 2) No microbial data from bottom water that is not contaminated with sediment from a multicorer (i.e. improper methodology used) 3) No microbial data from nodule habitats 4) Sediment sampling is reported for two locations but is not clear if either of these are in the IRZ or PRZs. 5) The reference database and approach used for taxonomic assignment of microbial “species” is very outdated. The method described on page 453 cites using a 2017-era version of Silva SSU132, which has almost half of the taxonomic data used as reference as modern platforms. 6) No data provided on total abundance/count of microorganisms
181 (&210)	The report states that the dominant taxa of bacteria and archaea in deep-sea sediment are consistent with those in the German Contract Area and that “microbial ecological function of deep-sea sediments in the CCZ is stable on a large scale”. This is misleading in a number of ways. A 2022 study (Wear et al.

	2021 DOI: 10.3389/fmars.2021.634803) of deep-sea sediment across the CCZ shows that the dominant taxa are Nitrosopumilaceae, Gammaproteobacteria, Planctomycetota, Acidobacteria, and Chloroflexi. The EIA does not detect any Planctomycetes, Acidobacteria or Chloroflexi, and miscorrecly groups Burkholderia (a common contaminant) in with Gammaproteobacteria. This raises suspicion that the microbial community assessment is flawed, perhaps by not having sufficient starting material nor adequately correcting for contamination. The method lacks detail about how much material was extracted and information about how common sequence contamination was addressed. With only one dataset presented, and lack of similarity of this dataset with public datasets from similar environments, the statement that there is “stability” cannot be supported.
181 (&210)	It is impossible to know if the PRZ and IRZ have similar microbial communities, since 1) only one sediment dataset is provided, and no information is provided on where this one dataset comes from, and 2) there are no data from the water column nor from nodule habitats. The data provided are insufficient to form an adequate baseline for monitoring.
183	“four MUC samples for meiofauna, at two stations”. How many multicores were analyzed? 4 according to Fig.5.4-21. This is insufficient replication.
184	Only information on higher taxa level is given. This is not sufficient information to assess the baseline environmental data on.
184	The name of the taxon is “Harpacticoida” and “Nauplii”, with the details about Nauplii in the next comment.
183	Nauplii is not a taxon. Generally, we use Nauplii to refer to juvenile organisms of Copepoda; however, this classification should be used with caution. I recommend checking, besides the size fraction studied, the possibility of sample contamination, as well as paying attention to the identification process.
186	Were only 4 samples analyzed? Is there only information on higher taxa level?
188	Comparison on 4 samples each in PRZ and IRZ is statistically not robust. On what data is the statement “biological groups are similar” based on? In addition, the authors mention themselves that IRZ has many exclusive groups (“The IRZ has many exclusive groups, possibly because there are not enough survey stations in the PRZ1 to cover all groups.”). How does this affect the interpretation?
189	Comparison of abundance shows 4 samples from IRZ and 2 from PRZ2. No conclusion can be drawn on this amount of data.

195	No information is given on the way the final sample of images was selected for annotation. Usually, it will be based on the range of camera altitude for selected images is a crucial parameter to allow picture comparability within and among survey lines, and even other sites in the CCZ. Considering this study was taken with a towed camera, which usually does a lot of ‘yo-yos’, cautious must be drawn on the potential quality of the images, which in the end, are not even presented in the report.
195	10 km transect within the PRZ and the IRZ might be the strict minimum to characterize benthic communities within a given area (Simon-Lledo et al. 2019a). However, transect length is a meaningless value, since it strongly depends on the altitude and on the optical parameters of the camera. Still, no information is given on the seabed area that was actually recorded, and which is considered the most reliable indicator of the sampling effort.
195	The transect lines are positioned west and east of the PRZ and IRZ and oriented over longitude. However, no transect lines were positioned North and South of these areas. I therefore draw caution on the representativeness of their orientation and position, since no study has to date proven differences based on the orientation.
195	No information is provided on the annotation <i>per se</i> . 1. Randomization of the annotation effort is necessary to avoid bias throughout the annotator's learning. 2. No information on the identification of megafauna is provided. Throughout the section, it remains unclear if annotation was made at the lowest taxonomic level (i.e. genus/species level).
195	No bathymetric map is associated with this transect map (Figure 5.4-35). Hence it is relatively difficult to assess if the position of the IRZ and PRZ corresponds to similar geomorphologic habitats which are known to influence community composition (Simon-Lledo et al. 2019a).
195	No habitat information is presented in this section, although it could have significant influence on community composition (e.g., nodule density: Simon-Lledo et al. 2019b; geomorphology: Simon-Lledo et al. 2019a). Furthermore, this makes it difficult to compare the representativeness of the PRZ habitat with that of the IRZ as these should be as similar as possible.
196	If densities of megafauna were presented, this means that the crucial information of area sampled was calculated, but unfortunately was never presented.
196	If average metazoan density was 386.1 ind./ha, this is still far away from Durden et al. 2021, who reported such values only for APEI 7 (380-460ind./ha). APEI 1 and 4 harbored metazoan densities ranging from 530 to 650 ind./ha. APEI 7 which is probably > 1000 km away, APEIs 1-4 are the closest to the block A-5. Therefore, the statement ‘which is close to the abundance at APEI 1, APEI 4’ is not legitimate.

196	However, these densities might be closer (~100s ind./ha) to densities observed in the Western CCZ (Durden et al. 2021) than in the Central and Eastern CCZ (~1000s ind./ha; Simon-Lledo et al. 2019; 2022). However, since we can't assess the quality of the dataset, we can't confirm densities presented in this study are accurate or underestimated (e.g., De Smet et al. 2021).
196	Figure 5.4-36: The relative abundance of Echinodermata predominates (44.3%) compared to other studies in the western CCZ (Durden et al. 2021: 10 to 27%). Particularly, Holothuroidea seems very abundant at this site (27.3%) compared to Durden et al. 2021 (5 to 9%). To my knowledge this holothurian proportional abundance has never been seen in the CCZ. This suggests that image quality only allowed to image big animals, and not the smallest. This observation is complemented with the low proportion of cnidarians (12.3%) compared to proportions of 32-42% in APEI 1-4 (Durden et al. 2021) and in overall in the CCZ (Simon-Lledo et al. 2023). Uncertainty therefore remains on the robustness of this dataset.
198	Figure 5.4-39 is meaningless because there is no separation between the survey lines. I can't therefore draw any conclusion from this graph.
198	Figure 5.4-39 contains sessile benthos, which should be further divided into 'nodule-free fauna' and 'nodule-attached fauna' (e.g. Porifera living on the sediment is common in the CCZ; Simon-Lledo et al. 2023). This makes more sense in terms of conservation as nodule-attached fauna is more likely to be impacted by nodule collection and the sediment plume. If only the graph allowed to compare PRZ and IRZ community in relation to the substrate, this could be useful to assess the representativity of the PRZ.
198	'Analysis also showed that there was no significant difference in the megafauna abundance': no statistical test was ever mentioned. The word 'significant' is not appropriate.
198	"species composition and community structure of the megabenthos in PRZ1 and IRZ are similar": this section never presented any results acquired at the species level, but rather at the class level, and therefore incorrect.
198	No diversity metrics are presented, possibly due to the lack of identification at the genus/species level. Therefore, I stress the need that the comparison among PRZ and IRZ communities is strongly uncertain. Cautiousness is therefore needed when stating that "species composition and community structure of the megabenthos in PRZ1 and IRZ are similar".
198-199	I can't verify the numbers provided in the paragraph comparing the NORI-D area. Plenty of studies exist in the Eastern CCZ and I don't understand why making a comparison with NORI-D specifically.

<p>199</p>	<p>“The megafauna abundance of the Block A-5 is lower than that of NORI-D.”</p> <p>The NORI-D site is clearly located 5000 km to the East. Comparing these two sites is therefore meaningless as block A-5 and NORI-D are located in very different environmental conditions of depth and POC fluxes (Simon-Lledo et al. 2023). It is well known that the abundance is much higher in the Eastern CCZ as these are simply different environments and communities (Simon-Lledo et al. 2023).</p> <p>Similarly, “(with Cnidaria being the most abundant)” and “The megafauna abundance of the Block A-5 is lower than that of NORI-D.” are totally out of context, especially considering the uncertainty regarding data quality.</p>
<p>200 to 202</p>	<p>Figure 5.4-42 These images were extracted from other’s work. Six of them do not have a reference listed. It is unclear if any images of the towed camera were provided. Such an effort to extract a detailed list of pictures suggests low confidence in the image quality, which the authors probably did not want to present. Ultimately megafauna identification at the species level was probably not provided because of that same reason.</p>
<p>200 to 202</p>	<p>Figure 5.4-42 The legend says, “Representative megafauna in the Block A-5”, since images were taken from other studies as examples. Representative is not an accurate word in this case, especially because the species presented here cannot be reliably related to the organisms of the CMC study, as the latter were not identified to that taxonomic level.</p>
<p>210</p>	<p>Please, provide all the taxa names, and also more details about the biomass measurement, some adjustment was done for the different groups?</p>
<p>211</p>	<p>Please, provide more details about the molecular data that you used to compare your results</p>
<p>Overall for megafauna</p>	<p>Based on benthic images retrieved with a towed camera, there was an effort to break down community densities and composition according to spatial factors and at maximum at the taxonomic level of the class. These values were compared with APEI 1-4-7 and the NORI-D area.</p> <p>A detailed list of information on sampling parameters and methodology is clearly missing (i.e., image altitude, area imaged, threshold for image selection, overlapping image removal, annotation protocol). This prevents from assessing the quality of the image set and evaluating the reliability of this study. This statement even becomes a concern when considering that a towed camera (used in this study) usually does not stabilise well in the water column, therefore causing high variability of quality within the image set.</p>

No seabed picture was presented, and instead, a great effort was made to compile pictures of other studies while their own set was readily available to present. As a result, we can't even describe if they were of sufficient quality to even proceed to the results that were presented. However, the much higher proportion of large organisms (e.g. holothurians: 33%) compared to smaller organisms (e.g. cnidarians: 12%), is an unusual result compared to the opposite trend seen in the western CCZ (cnidarian ~ 30 to 40% and holothurian ~ <10-15%; Durden et al. 2021; Simon-Lledo et al. 2023). Caution is therefore needed on representativity of this study, as its reliability cannot be assessed, whereas the overestimation of larger organisms might be due to insufficient image quality.

The megafaunal assessment clearly suffers from a lack of precision in identification which prevents drawing any robust conclusion on the stated similarity between the PRZ and the IRZ, nor with other areas of the CCZ. Furthermore, any attempt to assess post-mining trial dynamics is probably already compromised because of the low taxonomic resolution of this first assessment prior to impact.

Possibly due to the small level of detail of the identification effort, diversity has not been assessed in relation to the sampling effort (i.e., accumulation/rarefaction curves). This is however a crucial result to compare community composition and diversity with other areas.

The functional assessment between sessile and vagile fauna is a good try but is still not appropriate. Typically, it should break down sessile fauna by 'nodule-free' and 'nodule-attached' fauna (Simon-Lledo et al. 2019a). This is much more meaningful as 'nodule-attached' fauna is supposedly less resilient to the impact of mining collectors compared to the 'nodule-free' fauna.

No habitat information is integrated in this section. This prevents a clear comparison between PRZ and IRZ and their possible influence on differences of the community (e.g. nodule density).

Some statements of 'similarity' with APEI 1-4-7 are clearly misleading and even wrong for the community composition with Durden et al. 2021. Comparison with NORI-D is clearly out of context and demonstrates the lack of a thorough literature review as peer-reviewed literature has been published in the Eastern CCZ (Simon-Lledo et al. 2023).

Conclusion: Because of the absence of details regarding the image set acquisition and quality, it remains impossible to assess the reliability of this study. Importantly, based on the information in the document, the image set was not appropriate to assess the similarity between PRZ and IRZ. Furthermore, the community composition assessment was made at a low taxonomic resolution. No presentation of biodiversity metrics was made. This clearly constrains the

	comparison of the PRZ and IRZ to ‘simple’ indicators that seem to serve for drawing ‘easy’ conclusions stating several that PRZ and IRZ are similar.
Chapter 6	
213	While fishing intensity is a useful metric it is by no means comprehensive in understanding fishery operations in the region. It would have been useful to understand catch levels and if possible the timings of the fishing operations in the year. Further, it would have been useful to understand the fishing intensity in comparison to the rest of the CCZ, rather than to specific regions with seamounts/islands where it is known that high biomass of tunas can aggregate. This could lead to false comparisons. There can be quite significant amounts of tuna biomass captured in the CCZ (van der Grient & Drazen 2021 (http://dx.doi.org/10.1016/j.marpol.2021.104564) which may even increase in the future (http://dx.doi.org/10.1038/s44183-023-00016-8).
214	It is unclear what is meant by the statement regarding the western CCZ not being a main fishing area for any major fishing target of high seas fishery. Most of the CCZ is included in the Inter-American Tropical Tuna Commission, and the western part of the CCZ is included in the Western and Central Pacific Fisheries Commission, two Regional Fisheries Management Organisations that are specific for tunas in the high seas. No clear arguments or data are provided for this statement, so please clarify what this is based on.
214	The statement made about the Amon et al. (2023) paper is incorrect. The overall percentages concerning tuna biomass increases are similar or differ only in 1% between the 4.5 and 8.5 RCP scenarios. Further, only for yellowfin tuna is there a clear difference in the biomass increase in the western CCZ, but for skipjack tuna the biomass will increase quite a lot in both the 4.5 and 8.5 RCP scenario.
220	<p>The statement that APEI-1 will effectively preserve the benthic biodiversity in the western part of the CCZ is not supported by the Durden et al. (2021) paper, which clearly demonstrates quite a variation between APEI-1, APEI-4, and APEI-7, with few common morphotypes between the sites, and many morphotypes only observed once. This low overlap between APEIs suggests little connectivity between the APEIs, and without any clear data presented in this EIA about the biology (see previous comments), the statement made in the EIA here is unsupported.</p> <p>Further, the paper clearly demonstrates a difference in communities from seamounts and abyssal plains. APEI-1 contains seamounts, while Block A-5 is an abyssal plain. This further suggests that APEI-1 will not preserve the same kind of benthic biodiversity as is present in A5. It is important to add the adequate nuance here and not overstate what the data is showing.</p>
Chapter 7	

224	Previous published work has shown how the collector plume on the seafloor behaves like a turbidity flow (Munoz-Royo et al. (2023) (https://doi.org/10.1126/sciadv.abn1219), which is not considered here. How will this potentially change the results if that was included? What would be missed if this is not included?
224	The number of sites is not clear. The third paragraph states there are four different depths/habitats, but 5 are listed? Which number is correct?
228	It would be useful if the redeposition thickness was also expressed against the natural redeposition rate to understand the amount of impact this represents against natural background conditions.
229	It is unclear how the duration of the test (100% power collection test) will be shortened. From what is it shortened to what?
230	It is unclear from the second paragraph how quickly the sediment would redeposit - no estimate is given here, nor a citation from a publication where it may have been shown. Nor is it indicated what is considered near field.
230	It is unclear what distances are meant by far, middle and near fields. How far away will these be from the plume? What are these distances based on?
230	As no details have been given yet about the numerical model, it would be better to say 'a numerical model' here, or give some details (type of model, for example, but limited to) of the model here.
233	It is incorrect to state that there is no obvious variation in temperature and salinity in the bottom layer (which is defined as what depth here?) - there is variation and while this looks like it is only a little, given the stability in these factors at those depths, a similar small variation in these factors could be quite significant for biology. <u>These factors have to be interpreted in the context and scale of the natural conditions</u> , not according to what is common at, for example, surface levels.
234	The reference Zhan Lin et al. (2023) is not in the reference list, and it can therefore not be verified whether the settling speeds used are appropriate.
237	It is unclear what was considered as the plume disappearing - what threshold of the plume concentration was used for this and why was this value chosen?
237	It would be useful to have the impact values in the context of natural background values. For example, Garden et al. (2018) (https://doi.org/10.1016/j.epsl.2017.11.008) estimated that the background in the CCZ is 0.01-0.02 mg/L. Meaning that 10 mg/l is 1000 times higher.
239	Why was the 0.1 mg/L isoline taken? That is still 10 times higher than the background conditions.

Chapter 8	
262	8.1. 2. para: It is stated that key data from this test are required to develop a collector vehicle with ‘only moderate environmental disturbance’. In light of the ISA’s remit to apply the precautionary principle and to prevent environmental damage, the aim should be to develop a collector vehicle which causes ‘minimal environmental disturbance’. Please explain why the authors used ‘moderate’ and why they believe it is justified.
262	8.1. end of 2. para: Explain what exactly the ‘etc.’ refers to. This is too vague. Furthermore, references for the reports and studies which have been used should be listed.
263	Items (2) c and (2) e: add to both items at end of sentence ‘as well as on seabirds and benthic fauna’. Table 8.2.1 is required to be amended accordingly.
264	Table 8.2.1 is required to be amended to include potential impact of categories 2c and 2e on seabirds and on benthic fauna.
265 & 266	<p>Figures 8.3-1 and 8.3-2: Please add the outline of the CCZ to the figures as well as the Contract area for which this EIS applies.</p> <p>Furthermore, explanations of the colour code used in the maps are missing. Please amend.</p> <p>It is customary to include a scale or a grid/border in maps, so the readers can have better spatial awareness of the areas represented.</p>
265	Block et al 2011 is an older paper. There is an updated version, Connors et al. (2022) (https://doi.org/10.3389/fmars.2022.897104) which should be used instead.
265	The IUCN conservation status of Leatherback turtles is ‘vulnerable’. Although we agree with the conclusion that the activities as part of this test are not likely to present a risk to this species, we do not agree with the argument that this is due to their wide-spread distribution.
266	Figure 8.3-3: Information provided in this figure is rather old (publication date 2011, which means data are even older). If no newer data are available, then add a map with predicted distribution of such top tier marine predators under different climate change scenarios.
267	Last sentence. The fact that ‘the tonnage of the vessel used in this test is significantly smaller than that of the vessel used in NORIs test in 2022’ is neither a scientific reason nor a scientific justification. This comparison is irrelevant and thus should be omitted.
268	Figure 8.3-4: Information in the legend missing on the particular noise source and which are the recipients. DSM has several noise sources and providing such an

	‘overall threshold’ with no reference to either noise source nor noise recipient does not provide useful information.
269	The reasoning that other industries (such as longline fisheries) cause stronger light disturbance is no valid justification for assuming that the surface light emittance of this trial would not cause a harmful impact. Mitigation for surface light should still be applied to prevent bird strike.
269	In order to prevent light from shining upwards and thereby attracting seabirds, screens for such a purpose should be used as mitigation measures.
269/270	The measures listed as mitigation, i.e., that the vessels used in this trial will ‘not discharge or discard substances that may cause pollution or harm to the marine environment’, ‘the collector vehicle to be tested will not cause uncontrollable discharge of toxic and harmful substances’, ‘hydraulic oil required for equipment test will be properly controlled’, and ‘Wastes generated will be stored properly instead of being dumped into the sea’ are statements but not acceptable mitigation measures. Please list which precautions are in place to ensure that any accidental pollution does not happen, for instance the use of biodegradable hydraulic oils. Furthermore, what are the contingency measures should such pollution occur? If these are laid out in a different section of the EIS, please refer to the section here.
270	Section 8.4. Please state the noise level in decibel of the small propeller of the collector vehicle here so the impact of the noise level can be evaluated by noise experts.
271	Please clarify what is meant by the organisms that are sucked up by the collector head being ‘re-settled with the ocean current’? Is the assumption here that the organisms are redistributed by the current together with the sediment that is also sucked in? Through which size filter will the organisms have to pass to be released from the collector head? This statement is based on untested assumptions that a) that the organisms survive being sucked up and discharged from the collector head; and b) that after this passage through the collector head they are in sufficiently healthy condition to resettle. Both assumptions are likely to be incorrect. Thus, the conclusion from this should read ‘This process may lead to the direct death <u>of the majority of</u> organisms living in the sediments or being eaten’.
272	Please correct the statement that DISCOL cruises were conducted in Germany. The project was funded by Germany, but the cruises were conducted in the South-East Pacific off Peru.
272	Please clarify the statement ‘the species composition of Polychaeta was significantly lower than that before disturbance’. Species composition can only be different but not higher or lower; it is not a quantitative measure but a qualitative one.

272	A reference is required for the statement that the nematode community had not recovered after 26 yrs.
272	The statement ‘the structure of Nematoda communities in the track was significantly lower’ is incorrect. The structure of a community cannot be higher or lower - it can only be different. Please correct and explain in what way the structure is different.
272	It is recommended to exchange the term ‘hard basement’ to the commonly used ‘hard substrate’.
272	The authors state, based on scientific literature (see some minor comments above), that there will be serious impact on the meiofauna community in the impacted area in the sediments and inside the nodules (crevice fauna). Whilst we appreciate this information, the statement of “overall impact is small because of the small area mined” is not sufficient. The fact that the baseline collected for A-5 is not sufficient to monitor impact, is unfortunate and will not allow for any future mitigation (e.g. spatial management according to biodiversity, collector head design and impact on organism depth etc.).
273	Please clarify what is meant by ‘The former [nematode community structure] is small in size and low in density’. As mentioned previously, community structure is a concept and therefore cannot be bigger or smaller.
273-277	Overall, the quality of and language used in Section 8.5.1.3 is sometimes poor. Technical terms are poorly translated into English; for instance, hyperbenthos should read epibenthos, ‘stone Anthozoa’ should read ‘cold-water coral’, sea snow is ‘marine snow’ (p 276). Results of published studies used in comparison to the expected impacts of this test, are given in qualitative terms such as ‘higher and lower’ or ‘longer and shorter’. Such qualitative statements do not allow for a scientific assessment of the impacts to be expected. Furthermore, references provided often relate to species which do not occur in the CCZ, for instance krill or herbivorous copepods, the cold-water sponge <i>Geodia barretti</i> , without mentioning that therefore conclusions drawn from such results are somewhat limited. Additionally, the relevance and implication of the referenced studies to the impacts expected of this test are missing for most impacts addressed in this section.
273	Please clarify by what is meant in the first sentence of section 8.5.1.3 by ‘than that in particles deposited in the upper layer’. It is unclear what exactly the carbon content of deep seabed sediments is compared to. Please also provide the actual values of the carbon contents compared here.
273	The conclusion of particles of the sediment plume not being a good food source is somewhat flawed and irrelevant. Since the plume sediment is the same as the ambient sediment, it has the same nutritional quality as the undisturbed ambient

	sediment. The plume does not provide an additional food source, it just redistributes the particles.
273	Evidence/reference missing for the statement ‘larger particles will soon fall to the seabed’. Clarify what ‘soon’ means and what the spatial footprint is likely to be of the larger and smaller particles from the plume.
273	Exchange ‘hydrobenthos’ with the correct term ‘epibenthos’.
273	The description of the resettlement area of the plume sediment is very poor and lacks quantitative values. ‘More or less, in ‘longer or shorter time’ are qualitative statements which do not allow scientifically based impact assessment. Please provide results of sediment plume modeling. These should include: <ul style="list-style-type: none"> - vertical and horizontal extent of sediment plume with particle densities. - expected times for particles of different sizes to remain in suspension. - overall size of the area the sediment is likely to settle on. - size of the area where the thickness of resettled plume sediment is more than 2 cm as well as size of area with less than 1% plume sediment cover.
274	The correct English expression for ‘cold-water stone Anthozoa (<i>Lophelia pertusa</i>)’ is ‘cold-water coral’. What is meant by ‘bone growth rate’? As an invertebrate <i>Lophelia</i> does not have bones. Clarify what is meant by ‘high concentration particles’ and provide particle concentration the larvae were exposed to as well as how these values compare to particle concentrations expected from the sediment plumes of this trial.
274	The different scales (and likely) the different collection methods can influence the results in a way that does not corroborate with your conclusion. <i>Leptolaimus</i> and <i>Camacolaimus</i> are indeed important genera of Nematoda in nodules, but even in appropriate studies (Singh et al., 2019), the difficulty of associating a group with a particular habitat is shown. Certainly, this extrapolation of the relationship between <i>Leptolaimus</i> and <i>Camacolaimus</i> is inadequate. Generally, Nematoda data is measured at the genus level (although the document mentions the term ‘species,’ even though they are not listed appropriately), but for comparison purposes or establishing general patterns, taxonomic level, collection method, etc., are indispensable for correct interpretations.
274	Provide particle concentration <i>Geodia</i> were exposed to in the experiment as well as how these values compare to particle concentrations expected from the sediment plumes of this trial. It would be useful to mention that <i>Geodia</i> is a sponge species.
275	“Nevertheless, this problem can be ignored in the deep-sea area where the bott” (end of point 2). However, there is absolutely no evidence to support this claim; even in the cited article, it is not clear. In fact, the dynamics of organic matter decomposition on the ocean floor differ in some areas, especially concerning

	isolated regions from each other, making it very difficult to support the assertion that due to "water column is well exchanged, this problem can be ignored in the deep ocean".
276	Clarify what is meant by 'at-sea and upper layer ocean. The term 'at-sea' layer is not a recognised one. It is used further on in the context of 'at-sea' organisms. Do you mean pelagic?
276	Please clarify what 'thick seawater' is.
276	Visual interference would have at least if not even more impact on pelagic organisms than on benthic ones due to high levels of bioluminescence in pelagic organisms. Impacts on the pelagos are missing in the section on visual interference.
276/277	It is not clear what the second area is that is referred to in the statement 'These two areas account for 0.0044 %' since the previous sentence refers to only one area.
277	Please clarify why and for what the studies are not sufficient when stating 'the studies on the direct effect of sediment burial on benthic fauna are not sufficient at present.' What is the relevance of this to the conclusion drawn?
277	Many benthic shallow water species have relatively high tolerances to sediment burial due to being exposed to changing currents and in general higher current velocities. Thus, it is not likely that deep-sea benthic species in soft-sediment areas have the same tolerance levels to sediment burial.
276	Please clarify to what kind of mining the following refers: 'will ingest SPM in mining tail water'.
277	Please clarify what is meant by 'In the extensive oxidation environment in the bottom layer of the CCZ' - it is not clear whether extensive refers to the spatial extent of the CCZ or to the bottom water being well oxygenated.
277	The text mentions 'the release of heavy metals' but does not specify from what the heavy metals will be released and by which action.
277	Clarify what is meant by 'most toxicological studies aiming at the EIA for seabed mining are limited by various conditions and have to use shallow water and at-sea biological species as test organisms' - it does not make sense for studies to 'aim at' an EIA. Furthermore, what are the limiting conditions mentioned, the reasons for them being limited, and limited for what purpose? What are 'at-sea biological species'?
277	Does the sentence 'Since todays mining processes and technologies do not involve discharge in the surface and middle layers of the ocean, the data of

	toxicological assessment using shallow water and at-sea species cannot be directly used as the basis for EIA for seabed mining activities.’ refer to the activities as part of the test? If so, please make this explicit.
278	The assumption that benthic fauna can (and would) escape from heavy metal pollution will not hold for sessile fauna and fauna being buried by a sediment plume. It is not clear from which section which heavy metals are expected to be released into the water as part of this test in Block A-5 and at which concentrations.
279	Provide the noise levels of the collector vehicle stated in the NORI test results (NORI 2022) instead of just stating that they are assumed to be the same as for the collector vehicle to be used in this test.
279	The last two sentences of the page require clarification as to what the expected horizontal radius of the noise of CMC’s test vehicle is going to be.
280	Please expand on what the ‘Reasonable arrangement of test plan’ are. If a chart of the planned test runs is provided in a different section of this EIS, please insert a reference to the figure here.
282	Please explain what causes ‘and conductivity incurred therefrom’. Does the ‘therefrom’ refer to the variation in dissolved oxygen levels? How can variation in DO cause conductivity? Also, conductivity as a harm has not been mentioned before as a potential disturbance.
282	Provide reference and web page address for Ecosim with Ecopath.
283-287	Section 8.7: Reference section: please correct the weird and incorrect splitting of words at the end of lines.
306	A survey of baseline characteristics is foreseen 1-2 weeks before the test 2025. Can you provide more details for such a plan?
306	“Minmetals now can comprehensively describe and compare environmental baselines of the IRZ and PRZ1”. Based on the presented results in this EIS, the number of samples and detail of analyses is not sufficient, and this conclusion can’t be drawn (please see our detailed comments on baseline data). The baseline data that are presented here are not sufficient to monitor impact and study variations.

307	“post-monitoring will be conducted within hours, days, weeks or months””and the results will be used in the EIA for Commercial Mining”. This is insufficient information. Further, the results thus can’t be used for the EIA for Commercial Mining (as baseline data are not sufficient, low number of replicates)
308	“Minmetals hereby promises that it will conduct comprehensive environmental monitoring”. This should not be a promise but be supported with a robust plan. Do provide details of this plan.
313	It is interesting to see two mini-box core samplers on the collector vehicle. However, the low number of replicates (2) won’t be able to provide any robust data (low replication).
315	Table 10.3-2 shows an array of instruments used. Are these sufficient to be able monitor the plume? How do the numbers compare to studies by e.g. DEME-GSR, BGR, and NORI?
323	Figure 10.3-9 Design of monitoring arrays. Could you please show this on a topographic map? Was the topography considered when designing the array? (inclination downwards towards NE)
324	How can the pictures taken with AUV be compared to baseline data?
Chapter 10	
309	In several parts the document references ISBA/25/LTC/6/Rev.3 (‘Recommendations for the guidance of contractors for the assessment of the possible environmental impacts arising from exploration for marine minerals in the Area’). In the EMP summary even specific variables are cited. However, biogeochemical variables, that are mentioned in that document as well are ignored in the list as well as in all other parts of the document and are obviously not a target of the EMMP. Especially observations of sediment oxygen distribution and fluxes that provides key information on biogeochemical conditions and processes in the sediment - and possible changes in response to the collector test (see ISBA/25/LTC/6/Rev.3 paragraphs 15.b.iii and 15.g.). This is regarded as a major shortcoming that violates ISA recommendations (and also guidelines, e.g., ISBA/27/C/11).

325	Please provide information on the amount and location of box-core and multicore deployments. How will spatial heterogeneity created by the collector vehicle be considered?
310	The description of the Environmental Management Plan lacks sufficient detail to determine if the monitoring program will be sufficient to detect impacts. While the proposed design of the monitoring array (Figure 10.3-9) is commendable, none of these arrays will measure metal ecotoxicity.
311-312	It is great to see the plan for sensors and in-situ sampling proposed for deployment on the collector vehicle, including sediment coring and water sampling capabilities! As these capabilities will be limited to only two tests, the plan would benefit from more description of when/where the sampling devices will be used to assess impacts in the CTA.
334	The methods described for the EMP assessment of microbial community ecosystem services are confusing, though they appear to be more robust than the methods used in the baseline study described in Chapter 5 (i.e. the sequencing methods are better). Methods are described for assessing mRNA sequences and metatranscriptomic functional gene expression, but no method is given for how RNA will be extracted and sequenced (methods are only provided for DNA extraction and sequencing, which is different). Furthermore, there is a claim that the metatranscriptomic data will be used to determine carbon and nitrogen fixation rates, which is an unproven claim for nodule habitats, and for most marine environments for that matter.
332-335	<p>The emphasis that the EIA puts on microbial communities is appreciated. However, it seems that microbially driven ecosystem functions (e.g., carbon and nitrogen cycling) are only assessed based on genomic information. At least ‘sediment community oxygen consumption as a metric of whole community (largely microbial) function’ (see ISBA/25/LTC/6/Rev.3 should be introduced into baseline studies and the EMMP. Measurements should follow state of the art and be performed in situ with benthic chambers and/or microsensors profilers (see ISBA/27/C/11 part E.139ff).</p> <p>The EIA mentions carbon and nitrogen fixation rates that will be correlated with information obtained from functional genes but lacks information on how C and N fixation rates will be determined.</p> <p>There are other useful measures of microbial metabolic activity that could be added to the program to improve coverage of benthic ecosystem function by means of shipboard incubations (e.g., extracellular enzymatic activities, tracer (radiotracer, stable isotope)-based measurements of microbial secondary production).</p>

335	“a long-term monitoring observation system will be set up”. Please give this information.
337	Future studies on ecosystem function include metagenome analyses of microbes. Are these also done prior impact? Are there any additional ecosystem function analyses planned?
Chapter 13	
343	We suggest moving the glossary (i.e., tables with abbreviations and terminology used) to the beginning of the EIS where it is easier to access and refer to.
Annex I	
355	Is the model able to match the measured bottom current direction as well? This is equally important to matching the current speeds.
365	Previous work has shown how the collector plume on the seafloor behaves like a turbidity flow (Munoz-Royo et al. (2023) (https://doi.org/10.1126/sciadv.abn1219), which is not considered here. How will this potentially change the results if that was included? What would be missed if this was not included?
367	It is incorrect to state that there is no obvious variation in temperature and salinity in the bottom layer (which is defined as what depth here?) - there is variation and while this looks like it is only a little, given the stability in these factors at those depths, a similar small variation in these factors could be quite significant for biology. <u>These factors have to be interpreted in the context and scale of the natural conditions</u> , not according to what is common at, for example, surface levels.
368	The reference Zhan Lin et al. (2023) was not included in the reference list in the relevant chapter or here, and thus these cannot be checked. This is important to provide as it has such a large influence on the plume behaviour. Without this information the model cannot be adequately interpreted.
372	0.1 mg/L is still 10 times higher than the background condition in particle concentrations for this area, so why was this value chosen as cutoff?
Annex II	
416	No clear information is provided on the sediment sampling strategy and the geochemical analyses.
452	The use of “Random Levelling” in the microbial analysis pipeline does not, as stated, ensure rationality of later analysis. Because microbial data is

	compositional (i.e. proportional - only distance between data points is meaningful), removing random sequences does not rationalize the data, it merely makes it non-replicable and non-comparable. See (Gloor et. al., 2017) for further discussion.
452	What is ‘dilution curve’? From context, it could be ‘rarefaction curve’, which is a term for a specific analysis. Adherence to standard nomenclature and providing definitions where deviations are used is necessary for clarity.
452	There appears to be at least two microbial analysis methods presented here, with no indication which methods are used at each point. One of these methods uses ‘Random levelling’ and one does not, meaning that data from the two methods are not comparable and should be considered separately. The text needs to take this into account, particularly as data was collected over many years.
449	No mention is made of how seawater microbial samples were collected in the field e.g. filter pore size and material, preservative used etc. Samples were stored at between 4 and -80C, but no information is given for which samples were stored. This temperature variation has a large impact on how long and how much DNA can be collected from samples, therefore should be included.
457	Methods of video analysis require clarity e.g. was AI used (and what method was used) or were taxonomists viewing the footage?
457	MMO/seabird observation was, from the methods, only conducted during daylight under good visibility. Why was PAM for marine mammals not conducted to detect under low visibility? This is standard for oil and gas, and windfarm surveys.
	<p>References:</p> <p>De Smet, B., E. Simon-Lledó, L. Mevenkamp, E. Pape, F. Pasotti, D. O. B. Jones, and A. Vanreusel. 2021. The megafauna community from an abyssal area of interest for mining of polymetallic nodules. Deep Sea Research Part I: Oceanographic Research Papers 172.</p> <p>Simon-Lledó, E., B. J. Bett, V. A. I. Huvenne, T. Schoening, N. M. A. Benoist, R. M. Jeffreys, J. M. Durden, and D. O. B. Jones. 2019a. Megafaunal variation in the abyssal landscape of the Clarion Clipperton Zone. Prog Oceanogr 170:119-133.</p> <p>Simon-Lledó, E., B. J. Bett, V. A. I. Huvenne, T. Schoening, N. M. A. Benoist, and D. O. B. Jones. 2019b. Ecology of a polymetallic nodule occurrence gradient: Implications for deep-sea mining. Limnol Oceanogr 64:1883-1894.</p> <p>Simon-Lledó, E., C. Pomee, A. Ahokava, J. C. Drazen, A. B. Leitner, A. Flynn, J. Parianos, and D. O. B. Jones. 2020. Multi-scale variations in</p>

	<p>invertebrate and fish megafauna in the mid-eastern Clarion Clipperton Zone. Progress in Oceanography 187.</p> <p>Simon-Lledó, E., D. J. Amon, G. Bribiesca-Contreras, D. Cuvelier, J. M. Durden, S. P. Ramalho, K. Uhlenkott, P. M. Arbizu, N. Benoist, J. Copley, T. G. Dahlgren, A. G. Glover, B. Fleming, T. Horton, S. J. Ju, A. Mejia-Saenz, K. McQuaid, E. Pape, C. Park, C. R. Smith, and D. O. B. Jones. 2023. Carbonate compensation depth drives abyssal biogeography in the northeast Pacific. Nat Ecol Evol 7:1388-1397.</p> <p>Simon-Lledó, E., C. Pomee, A. Ahokava, J. C. Drazen, A. B. Leitner, A. Flynn, J. Parianos, and D. O. B. Jones. 2020. Multi-scale variations in invertebrate and fish megafauna in the mid-eastern Clarion Clipperton Zone. Progress in Oceanography 187.</p> <p>Durden, J. M., M. Putts, S. Bingo, A. B. Leitner, J. C. Drazen, A. J. Gooday, D. O. B. Jones, A. K. Sweetman, T. W. Washburn, and C. R. Smith. 2021. Megafaunal Ecology of the Western Clarion Clipperton Zone. Frontiers in Marine Science 8.</p>
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