DEEP-OCEAN STEWARDSHIP INITIATIVE
Deep-sea minerals

Beijing Pioneer Hi-Tech Development Corporation Ltd
Beijing, China
(submitted via email to pioneer_eis@sina.com)

June 5, 2024

RE: Environmental Impact Statement – Joint Test of Deep-Sea Miner and Buffer Station in Beijing Pioneer Polymetallic Nodule Contract Area, Western Pacific

Sir/Madam,

Below, please find our Commentary on the Environmental Impact Statement from Beijing Pioneer Hi-Tech Development Corporation Ltd, open for consultancy till June 6 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,

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DEEP-OCEAN STEWARDSHIP INITIATIVE
Deep-sea minerals

COMMENTARY ON
"Environmental Impact Statement – Joint Test of Deep-sea Miner and Buffer Station in Beijing Pioneer Polymetallic Nodule Contract Area, Western Pacific"

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 Beijing Pioneer Hi-Tech Development Corporation Ltd to make this submission publicly available.

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General Comment

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 and informing the wider EMMP. The contractor is commended for collecting the data. The structure of the EIS is clear.

There is, however, insufficient information presented that would support the EIS, and especially regarding comparison between the IRZ and PRZ. Below we have captured these points in general comments, followed by the specific comments identified by the relevant pages.

The IRZ and PRZ are inadequately sampled to inform an environmental baseline. There is little replication (if at all) across various categories essential to inform the EIS. It is unclear how many samples were obtained from the IRZ and PRZ, specifically. There is a lack of description on how samples were analyzed. Clarifications are needed, as for example meiofauna samples included freshwater species – which may suggest contamination -, and macrofauna abundances were extremely low – which may suggest issues with sampling compared to other western Pacific areas. No raw data seem to have been provided. The EIA is therefore incomplete and as a consequence cannot fulfill its basic purpose: monitoring and assessing the environmental impacts and risks. Below are some key points:

- Essential ecosystem functions, such as bioturbation activity, POC degradation, benthic oxygen consumption/respiration, and microbial activity, have not been determined and underlying variables have not been measured.
- No biogeochemical data are presented for the IRZ (including the CTA) while only 5 TOC profiles are presented for the PRZ. This is insufficient to inform a baseline on that needs to establish that the PRZ and IRZ are similar. No pore water nutrient profiles or pigments as proxies of reactive organic matter availability in the sediments is presented.
- Only two box cores with complete depth for macrofauna were analyzed. The taxonomic resolution is mostly to phylum or order, thus making biological comparisons impossible.

- The number of macrofaunal specimens is surprisingly low: a total of 12 specimens from 3 box cores were collected for the IRZ and 17 specimens from 3 box cores for the PRZ. In addition, 3 box core samples are in themselves insufficient replication. The total number of box cores from the wider area (39 box corers) suggest only 69 macrofaunal specimens were obtained, and 8 box cores included no macrofauna. These numbers are highly surprising and may suggest issues with sampling and processing designs. Details on methods used are therefore crucial to understand the results presented here.
- A table 3-1 on meiofauna resemblance gives *Daphnia magna* as the second most abundant meiofauna. However, this is a freshwater and brackish water species, with no observations in the marine realm (as per the World Ocean Register of Marine Species (WORMS)). Given the lack of methods described, it is unclear whether this is because of contamination of equipment used, misidentification, or something else. Please provide a clarification.
- There was little replication for meiofaunal sampling, and samples were only analyzed to a higher taxonomic level (e.g., Phylum, Order), with the exception of 5 stations where nematode genera were analyzed (these were only from the IRZ). No information on the biodiversity in the PRZ was presented. Both the high taxonomic level and the lack of samples from the PRZ make it impossible to verify whether the PRZ and IRZ are sufficiently similar for monitoring purposes.
- The results of the megafaunal imagery data are unreliable because of the lack of a clear description in methods on data selection, annotation and analysis computation. The sampling effort was low, and unlikely to have captured a representative sample. No comparisons were made between the IRZ and PRZ. The abundances are suspiciously low, suggesting poor methods used. Results presented in the figures seem to vary, questioning their veracity. Some conventional methods (e.g., rarefaction curve) are missing. Data standardization is lacking, thereby preventing comparisons with future surveys. No comparisons were made with other areas.

The available presented data suggest that the IRZ and PRZ are dissimilar, and thus that the chosen PRZ cannot fulfill its purpose in the EMMP.

- The TOC content of the PRZ core (BC17) is about half of that in the IRZ (CTA). This indicates that the PRZ is not similar to the IRZ for this variable.
- We note the differences given in the table 3-1 (page 66) between the IRZ and PRZ in the following variables: Cu and Fe, nodule abundance and coverage (35

& 14 kg and 65 & 25 % coverage, respectively), particulate organic matter (0.4 & 0.8 mg/L, respectively), primary productivity (13 & 34 mgC/m2h, respectively).

- The presented OTUs show very little overlap between the PRZ and IRZ.

The environmental baseline survey SubphaseI-1 (planned for 2024) is insufficient to close the baseline knowledge gap. Table 9-5 indicates only one station in the CTA and one station in the PRZ will be sampled. **There is no replicate sampling planned**. Several replicate samples will be needed prior to "text-mining" to determine the baseline and quantify the uncertainty around this estimate to be able to monitor for impacts and the recovery patterns.

The plume model requires input of several more parameters for adequacy including: i) sediment size/composition – this is well documented, and includes an experiment done with sediment from the test site; ii) sediment flocculation – the results presented here need careful review. The data are derived from a land-based settling experiment in small beakers with sieved sediment. Stirring in this microcosm may not represent large-scale turbulence from the mining machine. Data from 35 particles appears inconclusive, and replication on a larger scale is required. The plume model only uses a single particle size of 387 microns while the experimental results show a broad range of sizes and settling in completely still water. How does this compare to field conditions, as it seems difficult to understand how realistic environmental conditions are modelling with this selectivity? iii) Currents at the test site: it was not possible to discern which station data were included in the model. It is therefore also unclear what depth the model was representing. This is important as there may be some notable tidal current swings at the test site. iv) density of sediment resuspended: this information does not appear to have been included in the model.

The plume model results and selection of the buffer zone based on the model results is not supported. The monitoring area around the test site is based on the model output. Is there enough confidence in the model or should a larger buffer zone be included? The model predicts a plume dispersal of only <3.5 km distance from the CTA. This is very surprising and much less than previous nodule collector trials (of GSR and TMC) also benthic impact experiments have shown. Previous collector tests have observed the sediment plume to travel farther than 4-5 km away from the respective CTA's, and at that distance the plume was still having SPM concentrations more than 10-times higher than ambient background values. Therefore, the employed plume modelling seems to be based on inaccurate and

poor parameterizations, and the monitoring plan should not rely on the model predictions. Monitoring during the test appears to concentrate in WNW of the IRZ. Yet, most bottom current plots show reversing tidal currents. Will the plume behaviour be adequately captured in the model? This is important as poor planning and monitoring of the plume, required in the EMMP, could mean that the plume could be missed or get poorly sampled, and thus underestimating the potential impact. It is recommended that a table of objectives for monitoring during and after the test be presented with the instruments planned to meet the objectives. For example, what equipment will test the particle flocculation assumptions of the plume model?

Specific Comment	
Page	**Comment**
Executive summary	
1	It is unclear how 'green' is defined here in relation to green deep-sea mining.
2-3	Note that the "protect and preserve" is not limited to the deep-sea environment, but to all the water in which you operate, including surface and midwaters. The long-term objectives are very broad and lack the details to clarify how these will be achieved in the project. The short-term objectives are from a time in the past (2020-2023), and it is therefore unclear how they are applicable here. It would be helpful to understand whether these short-term objectives were made, and if not, why not and how they are carried out in the future (with a timeline) with a strategy to close the gap.
5	The EIS states that there are plans to collect the missing 13 parameters before the collection tests. What are these plans and when will those data be submitted and who will review these, given that the EIS must be

	submitted one year prior to the test (which is planned for autumn 2025)? The missing parameters include for example total organic carbon, which is an essential parameter. In addition, the statement that a parameter was obtained, provides no information on the quality and quantity of specific parameters and can be misleading (please see detailed comments on quality and quantity of specific parameters below).
6	The table should also specify the parameters that are planned to be collected after the collection test. Also, the parameters that are missing in the current baseline data and that are planned to be collected prior to the collector test should be fully spelled out.
7	The model predicts a plume dispersal of less than 3.5 km distance from the CTA. This is very surprising and much less than previous nodule collector trials (of GSR and TMC) have shown and benthic impact experiments have demonstrated this, too. Previous collector tests have observed the sediment plume to travel farther than 4-5 km away from the respective CTA's, and at that distance still having SPM concentrations more than 10-times higher than ambient background values. Therefore, the employed plume modelling seems to be based on inaccurate and poor parameterizations, and the monitoring plan should not rely on the model predictions. The monitoring plan should extend well beyond the plume dispersion distance predicted by modeling to ensure data on plume effects are adequately sampled, and not missed because the plume dispersal distance was not verified by samples. It is unclear from the information presented in the section 'Selection of Test Area (IRZ/PRZ)' whether these sites were chosen based on similarity. The information presented suggests the site was selected because it was sufficiently far away from the CTA and IRZ, but that is a poor selection method for selecting a PRZ. The similarity in physical, chemical and biological parameters is important to ensure it is sufficient in its use for monitoring impacts.

8	What are the assumptions behind the estimated suspension of 17500 t of sediment during the test? Where and how often will the collected nodules be deposited at the seafloor (since the collector does not have a riser system)? The equipment (Manta II) is planned to be completed and tested in 2024. How is this taken into account in the current EIS given that the equipment currently is not ready for the proposed activities?
10	What is meant by "echo intensity"? The dampening of the acoustic backscatter intensity?
11	A sediment pH of <7 or >8.5 seems incorrect. Was the pH measure measured in situ (or ex situ, which is incorrect due to depressurization artifacts of the samples that cannot be corrected for)? The bottom water pH is ~7.8-8.0 (see CTD data presented on p. 249). Oxic POC remineralization will decrease the pH slightly (about 0.3-0.5 pH units) and suboxic POC remineralization will increase the pH again (by 0.2-0.4 pH units). More pH variation in the deep sea is hinting at an analytical problem of the measurements.
12	The bottom water currents are quite variable. It would therefore be more informative to also report on the temporal patterns and frequencies. Please add this information.
13	How deep did the observed eddies reach down to depth? How large are they? How much did they increase the current velocities? How do they affect the plume dispersion (has this been modeled)? "nitrite, nitrate, and phosphate are typically below detection limit." This suggests that inappropriate equipment was used, as since 2008 there are methods available that can measure nM level of these nutrients. Given that these nutrients play an important role in controlling primary productivity and carbon sequestration in these waters (Patey et al. Trends in Analytical Chemistry, Vol. 27, No. 2, 2008), it is important that

	these nutrients are appropriately measured.
14	What is the natural SPM concentration measured in the bottom water? Typical values for the deep sea are <50 micro-gram per litre. A background noise of 120 dB would be very high. Later in the document the presented noise data recordings are only 85-95 dB. Please correct the statement here. A maximum DO conc. in the water column of 479µmol/L at 75 to 100m water depth is hard to believe as it is far beyond 100% air saturation. Please clarify the methods used to obtain this value or explain why this value may be incorrect.
15	Please comment here on the status of the nodule and abyssal plain fauna; the seamount gradients are less relevant to this EIS.
16	Dominant microorganismal phyla recorded in the results can maintain metabolism through nitrification and denitrification and thus play a major role in nitrogen cycling. How does this finding relate to finding on page 13, that states “nitrite, nitrate, and phosphate are typically below detection limit.”
16/17	Information on the number of samples would be useful to evaluate the robustness of data (zooplankton, meiofauna, macrofauna, megafauna). We also note that no micronekton samples were collected.
17-18	Information on nodule meio- and macrofauna is missing. Megafauna probably includes nodule-attached organisms, but the list of groups found in the area should differentiate between nodule-attached and

	sediment / motile fauna.
18	For the eukaryotic DNA summary, it is not clear how many of the assigned taxa were duplicated – e.g. were families also included in phyla?
18-24	The list of environmental impacts does not address effects on benthic biogeochemistry and biogeochemical functions (e.g., seafloor oxygen uptake, organic matter remineralization, nutrient distribution and fluxes, trace metal release). Please add these with the appropriate considerations of these impacts or provide a clear and robust reason why these should not be included.
21	Nodule mining will remove all benthic fauna (sessile and mobile), not only “some”. How does sediment compaction increase microbial abundance? Please explain. What is the scientific basis for the assumption that community structure etc. are not affected? Research in the past years (including of the GSR trials) has clearly demonstrated the opposite.
22	“there is no scientific evidence to confirm that light pollution in deep sea causes adverse effects at community level”. While I agree that for this scale and time period light may have very limited effect, caution is needed when coming to a conclusion whilst there is no data (there is also no scientific evidence for adverse effects).

23	There are already some ecotoxicology studies available. Further, experiments could have been conducted (or planned) to aid in informing this EIS. The limited scientific information suggests that a precautionary approach should be taken. It is unclear from the text here how this is done. Please clarify how you incorporated this uncertainty in the impacts. In the deep sea, typically 40-80% of the accumulating POC is remineralized in the surface sediments. This is also what the presented TOC profiles show. Biogeochemical modelling should be employed to quantify the respective rates and fluxes to characterize the benthic ecosystem and allow for an assessment of the impacts.
26	Whilst it is for sure interesting to study the seamount, it is not clear why attention is put on this feature in this EIS. Is it because the seamount is within an AINP area in the draft REMP? Please explain.
26	The text states that in 2024 a baseline study will be conducted. How will these data feed into this EIS? How is the temporal effect (months of data collection and test-mining) considered in this EIS?
28 (& 572)	The presented sedimentation experiments are extremely limited. Basing a plume model on the results of 35 particles in still water is not robust or supported. For example, how is the sieving and stirring and wall effects of glassware reflective of field conditions? The limitations of the study need to be detailed and explain how assumptions may affect plume model results.

28	It is unclear how the sediment coverage experiment will extract exposure information from genome analysis of baited animals.
31	A model background SPM concentration of 0.5 mg/l is about 20-times higher than typical for the deep sea. The modelled maximum concentration of 10 mg/l is underestimating the situation by a factor of at least 50 (considering observations during previous collector trials). The monitoring plan needs to consider the risk of the modelling being way off. Please include this information.
Chapter 1	
36	Figure 1-2: please include the information on the location of the PRZ and IRZ on the map.
Chapter 3	
63	3.1.2: Provide the methodology used to sample both the PRZ and IRZ: number and type of samples; how they were processed; and how identifications were made. If this information is available in the appendix, please refer to the appendix in the appropriate places.
66	Table 3-1 is useful as it gives an overview of environmental parameters. It, however, shows that IRZ and PRZ may be dissimilar. We note the differences given in the table between IRZ and PRZ in: Cu and Fe, nodule abundance and coverage (35 & 14 kg and 65&25 %coverage), Particulate organic matter (0.4 & 0.8 mg/L), primary productivity (13 & 34

	mgC/m2h). The number of samples is given only for sediment type (n=13 IRZ, n=7 PRZ). This replication is very low. For other values, the number of n is not given, making it impossible to draw robust conclusions. It is unclear what is meant by meiofauna resemblance and if this concerns benthic or pelagic samples. Is *Daphnia magna* really dominant or is there a mistake? Is it possible copepods were meant here? A check in the World Ocean Registry of Marine Species (WORMS) indicates that this species is restricted to fresh and brackish waters. The statement of 'significant differences in meiofauna abundance' cannot be verified without information on sampling numbers.
66	Fig 1-2 on page 36 indicates the bottom samples in the proposed PRZ: I assume that the red square with a dot resembles a box core. In any case, there appear to be few (maximum three?) bottom samples taken for meiofauna. Please clarify how many samples were taken for the meiofauna. From Fig 5-61, p 358, there are three samples, with two pseudoreplicates each, for both IRZ and PRZ. This sample size is inadequate. The data are also too limited to make comparisons with other areas.
66	The PRZ density of nodules is notably different from the IRZ (25% vs 65%). This aspect is unfortunate as it will influence the recovery assessment in the test area and overall densities/types of attached fauna for which nodule abundance may be a relevant factor. It suggests that the PRZ may be inappropriately selected. The primary productivity (Tab 3-1) of the PRZ is 3 times higher than for the IRZ (CTA). Hence, the PRZ is inadequately chosen and cannot fulfill its purpose in an EMMP. A more appropriate PRZ needs to be chosen.

	Biogeochemical data are an essential and integral part to gain a basic understanding of the ecosystem and its functioning. Since all of these data are missing in this document, this document is not adequate as EIS. The impacts of the trials cannot be assessed.
68	There are no comments on nodule attached fauna or megafauna. Further, the camera transects on the M1 transect data do not seem presented? As this collector test has the greatest impact on nodules, an adequate diversity comparison with the proposed PRZ is important. While a list with photos appears on p. 733, only 16 taxa use nodules for attachment (just 5 are shared IRZ/PRZ).
69	Table 3-2: species information on benthic organisms PRZ and IRZ. The level of detail of shown data ("crustaceans & polychaetes") is very low and does not support the assessment whether the IRZ and PRZ are similar. Based on this table the "species richness" is different between the two areas. The number of specimens is surprisingly low: only a total of 12 specimens from 3 box cores from IRZ and 17 specimens from 3 box cores in PRZ were collected? Please clarify the methods used to verify that this is not a methodological issue. A similar issue seems to be present in Table 3-3: macrofauna identification from box cores. 4 box cores, and only 13 specimens from IRZ! Note that one box core sample was not included in table 3-2 (DY-BC56 with 1 individual). 3 box cores, and only 17 specimens! Please add details on the methodology.

71	Table 3-2 gives the dominance of nematodes as 84-92% (page 70), while on page 71 it is stated that nematodes account for 65%. What number is correct?
71	The study of 18S OTUs provides very interesting first insights: IRZ and PRZ share 223 OTUs from a total of 4234. PRZ OTUs: 2698, Shannon 6.12; IRZ OTUs: 1759, Shannon 4.6. It actually shows that the diversity is high and in the IRZ and PRZ have very little overlap. However, it is not clear how many samples were analyzed. For comparison: Macheriotou et al. 2020 (https://doi.org/10.1098/rspb.2019.2666) analyzed 23 rarefied samples that generated 5574 ASVs of which a large fraction (41%) were 'Unassigned', 1981 were assigned to 'Nematoda' (35%), followed by 'Arthropoda' (12%) with each of the remaining 33 phyla being represented by less than 2% relative abundance. They also found that generic richness of genus-assigned ASVs differed significantly between areas (different contract areas and APEIs).
72	A map of nodule abundance etc. would be more informative to assess the habitat than the bar graphs. 109 box cores were analyzed for nodule density in M2: 26 kg/m2. No information is provided on nodule density in M1 (and thus the PRZ)? Please add this information.
87	Table 3-9: no information on when further baseline data will be collected is given (it says august 1st2024 until august 1st 2025). Same for

	monitoring plans after test. Please add this information.
Chapter 4	
97	It is unclear from the report how many sampling stations were undertaken. This table appears contradicted by descriptions further into the text. e.g., pg 160 states that 110 stations were sampled for surface sediment type.
98	Table 4-2: No information is given on how many samples were taken in the IRZ and PRZ.
100	Table 4-3: TOC in water is missing. The POC flux is the most important driving factor for life in the deep sea. No sediment biogeochemical data (nutrients, oxygen, POC, carbonates etc are presented or have not been measured. This is an essential part of an EIA.
100	The table shows what parameters were acquired. It gives this, however, in checkboxes for many variables where only a few replicate samples were taken. This suggests that the checkmark is perhaps insufficient. Further, a table that includes number of samples taken and analyzed would be more useful.
118	Where are these stations? There is no list of coordinates or a map with station labels to match.

128	What does “surface sediment samples” refer to? An average of the top 0-1 cm, 0-5 cm, 0-10 cm? The results will depend on the sampling interval. A depth profile is much more informative and also allows to analyse important ecosystem functions necessary to understand the ecosystem and allow for an assessment of impacts. There is little use in reporting average values of an unknown sample interval. Particularly, also because most variables actually do vary over depth - the few presented profiles (e.g. p. 138) are proof of this.
136	Figure 4-28: this seems to suggest that the surface sediment grain size is different in PRZ and IRZ? This suggests that the two areas are not similar, limiting the use of the PRZ for monitoring purposes.
140	How was the pH measured? In situ (or ex situ, which is incorrect due to depressurization artifacts of the samples that cannot be corrected for)? How was the electrode calibrated? The observed scatter and values are not explained. The bottom water pH is ~7.8-8.0 (see CTD data presented on p. 249). Oxic POC remineralization will decrease the pH slightly (about 0.3-0.5 pH units) and suboxic POC remineralization will increase the pH again (by 0.2-0.4 pH units). More pH variation in the deep sea is hinting at an analytical problem of the measurements.
141	Were no pH and EH measurements taken in the PRZ?
142	Depth profiles of the mineral composition would be much more informative than presenting average values for an unknown sample interval.

143	Tab. 4-10: Why is the M/I ratio zero? It should be 0.04 to 0.2 (see also the text).
161	The TOC content of the PRZ core B17 is much lower than those of the IRZ, indicating that both areas are biogeochemically significantly different.
165	Why do the cores for sediment samples and porewater samples not match? Due to the considerable small-scale variability in the deep sea both analyses should be done on the same cores, in order to ensure a consistent data set.
169	Depth profiles of porosity would be much more informative.
180	What is meant with “natural porosity ratio”? Porosity would be the variable of interest.
229	There appears to be no analysis of the deep current meter data from the DY81 cruise. It seems, given the limited measurements from the test site, that adding a third set of data for the plume model is important. “near-bottom” currents: I assume they are measured by ADCP which does not catch the benthic boundary layer. Please clarify how this effect was incorporated into the model or not, or how you consider it may affect impacts. The dimensions on Fig 9-7 are illegible.
246	The succession of the La Nina and El Nino events along the time course of the investigations raises doubts on the representativeness of the presented baseline observations available at this point in time. The

	points raised here suggest that the usefulness of these baseline data in monitoring are inadequate.
247-248	DO concentrations in the water column of ~400µmol/L down to 300m water depth are hard to believe. For the temperature and salinity reported this would easily be beyond 150% air saturation. Also, concentrations of 406µmol in bottom waters as reported on page 263 seem far too high. Generally, oxygen measurements in discrete samples are not state of the art in water column observations. Good for validation but the profiles should be recorded with sensors.
273	The sound recording includes whale sounds. These data are included in the appendix section on marine mammal observations. Nice.
276	Noise levels were measured in 2022 (SPL 92-115 dB) and in 2021 (SPL 77 – 97 dB, with occasionally 105 dB). This is a rather big difference between the years. Any explanation? What was the HZ band used in 2021? Where were the sound-recordings taken, and were any sound recordings taken in the PRZ?
Chapter 5	
294	It is unclear in Figure 5-6 what the coloured boxes represent in the top figure and why there is a part of the bottom figure boxed. Please explain why attention should be drawn to these sections.
294	The chlorophyll estimates seem to differ between sections. Should the 38.45 be 0.3845 mg/m2? Please clarify which is correct. Further, please

	refer to the appendix if the methods are described there, as this needs to be reviewed to aid in the interpretation of the data.
295	Figure 5-7 and all consecutive Figures: please show where the PRZ and IRZ are and give the number of samples analyzed. How many samples were analyzed for Chla?
299	Please explain how the samples were taken, processed and analysed. If this information is available in the appendix, please refer to the appropriate sections.
300	Please explain how the species were identified. If this information is available in the appendix, please refer to the appropriate sections.
302	For microbial OTUs in the water (collected with CTD); Table 5-4: from which locations and depths were the samples taken? It says 6 stations and 72 samples; how many stations in IRZ and PRZ, at what depths?
306	Please explain how the samples were taken, processed and analysed. If this information is available in the appendix, please refer to the appropriate sections.
311	The text states that 155 microplankton species were identified. Please show a table with species-abundances. How many samples were analyzed? Stations are given in Table5-9 and 5-10, but please add info on location and depth (and if it was sampled within IRZ, PRZ, or elsewhere).

320	It is unclear what depth is meant by middle and deep layer. Please explain. It is unclear how the samples differed between each other across time and space as only high-level summaries are given. It is unclear whether the samples were taken in the same season across years.
325	Please explain how the samples were taken, processed and analysed. If this information is available in the appendix, please refer to the appropriate sections. No details on similarity or difference between IRZ and PRZ (only info on "block M") is given. Many tables are shown. It would be nice to see a multivariate analyses on differences between years and depth.
325	The text states that the total abundance in 2022 was significantly lower compared to 2021 or 2023, but information on a statistical test (method, results, etc.) are provided. Please include this information or remove the reference to significance.
328	It is unclear from the information presented how many species were shared between stations and years. Please provide this information. It is unclear if both the PRZ and IRZ were sampled and if they were, if they differed as the information presented is too aggregated.
330	Please explain why the depth intervals for the multinet were chosen like this. Was there any consideration of the environment taken into account? E.g., pelagic communities can differ depending on oxygen concentrations meaning it is more informative to target such layers

	rather than a very standardized approach as taken here. This approach risks mixing communities and limiting the comparative use of the samples.
331-337	The number of zooplankton species identified shows a great deal of care. It would be great to see some more detail in these, for example via graphs showing abundance (and biomass) change with depth, more information on species data (currently most is presented at a relatively high taxonomic level), and number of shared species between the depths and years.
341	Figure 5-46 is illegible and therefore not possible to interpret.
343	One station, with 2 samples for day and night is not a robust sampling method. The low replication suggests high uncertainty – although note that this uncertainty is not represented in Figure 5-49. A mention of significant differences is made, but no information on test and methods is presented. Please add this information or delete the word significant if no test was used. Further, explain how the test can be supported given the low number of replicates taken?
343	It is very difficult to get a picture of the deep-water conditions for the test mining site from this section. Readers have to pick out each parameter for the relevant test (and reference) site location and compile them separately.

346	There seems to be very high variability for Shannon diversity for microbes from sediments. Also, it is unclear whether this information is for the IRZ or PRZ.
334, 346	No reference is made to the applicability of compositional data (i.e. relative data) such as microbiology to alpha-diversity metrics. Values with no reference to this issue need to be interpreted with great caution, because the statistical basis of alpha diversity metrics presented here means that, without additional analytical steps (which is not detailed in the report), and have limitations; Gloor et al., 2017).
357	Foraminifera: 6 stations were samples, but how many sites? This is very little for the deep sea in general. Were these stations located in the IRZ and PRZ?
357	For the meiofauna, only 14 sites and 7 stations seemed to be sampled, with 3 in the PRZ and 3 in IRZ, and 1 in-between station. This is very little replication and does not support a robust analysis.
359	The data suggests there is very high meiofauna variability (even on higher taxa level) in the IRZ. This shows that more samples need to be taken to capture the natural variability. Also, the upper size limit of the meiofauna was not specified. What was considered the upper size (in µm) for meiofauna? The separation of the individuals in size (intervals) class was based on which concept or previous studies?

360	Comparison with the CCZ is not appropriate: the “east-west” gradient for the CCZ is over 3,570 km; the PRZ to IRZ distance is only 90 km. The sample size is too small to evaluate if meiofauna densities differ or do not differ from COMRA West .
364	Meiofauna were identified to higher taxon level, except for nematodes, which were identified to genus level. Five stations were selected, all from the IRZ. Thus, no meiofauna diversity data is available for the PRZ, meaning no comparison between the two areas can be made, limiting the PRZ use in monitoring. Is there a species abundance list for nematodes? The term "larvae" for free-living nematodes is not correct. Some groups of Nematoda do indeed have larval stages, primarily parasites and some families of free-living nematodes. In cases where it is not possible to identify individuals to the genus or species level due to sexual immaturity, the more appropriate term would be "juveniles."
365	About Figure 5-71: the resolution is low and should be increased. Additionally, it is important to maintain the consistency of the genus names. "Diplopeltoids" should be corrected to "Diplopeltoides Gerlach, 1962."
366	It is not clear how many samples were collected where. It is thus difficult to follow the discussion on interannual variability. The results show that from 15 groups (=higher taxa) found in 2022, only 10 groups were found in 2023.

368	Table 5-35 mentions 14 stations for 2022, whilst on page 357 it is 14 sites and 7 stations. Please clarify the difference. 14 samples are not sufficient to derive an interannual variability trend in abundance for as there are too few samples.
369	Is abundance significantly lower in Zone 1 (PRZ)? And if this is determined, what test was used?
370	What methods were used to sieve the macrofauna? No specimens were detected in 8 stations, which is surprising. Only a total of 69 macrofaunal specimens were detected in 39 box core samples. That is very low. No methods were given (for any of the faunal processing or analyses) in the EIS, and thus is impossible to verify whether the correct methods were used or whether these results are an artefact of methodology. Washburn et al. 2021 (https://doi.org/10.3389/fmars.2021.626571) provided a review on macrofauna abundance and diversity in the CCZ and reported higher abundances (supplementary figure 1). Although the area studied here is west of the CCZ, these low abundances are difficult to understand. A detailed description of methods would aid the evaluation of the presented data. The level of macrofauna identification is low, typically on order level, sometimes on family level.
373	What is the scale on the pictures?
374	Was only one box core collected in the PRZ? How many box cores were collected in the IRZ?

376	The figure is difficult to interpret: what are the color codes; what kind of analyses were used (what is “height”)? It is a very far stretch to interpret these data. Maybe seamounts have an influence on the distribution of macrofauna, but with the amount of data presented, this can’t be interpreted.
376	There seem to few data available to support the calculation of annual changes (using statistics).
377	“intervals quadrat for statistics”: no statistics were computed on the data (e.g., no analyses were presented any p-value).
377	I question here the survey design. To me, no towed-camera transect was performed strictly within the IRZ (Block M2; Figure 4.1 and Figure 5.84). Towed camera transects were performed in the PRZ (block M1; Figure 4.1) but were not annotated for no explicit reason. Therefore, this study design cannot provide insight to what level the PRZ and the IRZ are similar.
377	There is a lack of standardization on the image sample. Only the number of photos taken is presented while there should be an estimation on the area sampled (based on altitude and camera optical parameters or lasers). Additionally, “high-resolution photos” is not complemented with a value of mm/pixel. This will clearly limit the comparison of this dataset across preexisting or future studies.
377	The number of images is pretty low for each transect, from 1127 to 1547 images for a sampling effort of 65 km to 106 km. In average, the number

	of images per transect quadrat of 10 km, reaches on average 161 for PL01, 243 for PL02 and 141 images for PL03. This is a small amount of images for 10-km quadrat compared to sampling effort usually provided by classical 2-km transects performed in Pacific abyssal plains (Simon-Lledo et al. 2019). This low number of images translates into low abundance per quadrats, reaching in average 148 ind./quadrat in PL01, 188 in PL02 and 126 in PL03. I therefore suspect that the 10-km quadrats do not provide a representative assessment of the megafaunal community, although I cannot really assess that because of the lack of rarefaction curve. As a result, the study is probably more reflective of the overall community found over distances of 50 km. At these spatial scales, we know little about the megafaunal community variability, so I can't really assess if this is an appropriate survey.
377	There is a lack of description regarding the methodology on image selection for annotation and the computation of abundance per kilometer. No information is provided on the selection of the images considered for annotation. Especially for towed cameras prone to yo-yoing in the water column, the image sample should be threshold by the camera altitude to avoid biasing the community variability with different quality of seabed images.
377	There is a lack of description regarding the methodology on image annotation. For instance, no description is provided on the sequence of annotation, as the latter should be randomized across transects to avoid gradually biasing community variability by annotator training.

377	An exhaustive catalog of morphotypes is provided in Annex 4 (n morphotypes=35, while p. 377 mentions n=37). However, there seems to be only 'big' animals listed (roughly > 5cm). I cannot further discuss that because of the lack of information on image resolution (unit = mm/pixel) and seabed areas sampled compared to other studies in the CCZ. However, I suspect that richness was considerably underestimated because of the lack of small actinarians for instance and will limit comparison with other studies typically targeting organisms > 1 cm (e.g. Simon-Lledo et al. 2019a; Durden et al. 2021).
378	Figure 5-85 and all results below: Foraminiferans are included in megafauna analyses. However, they are usually excluded from analyses because of the difficulty to assess if they are 'alive' or not and because their high abundance biases community structure assessment (Simon-Lledo et al. 2019a).
378	"Foraminifera was detected only in quadrat PL01-01": This is an odd result. Large xenophyophores are known to widely distribute in the deep Pacific Ocean (Gooday et al. 2021), and their absence over a sampling effort of > 100 km makes me suspicious of the data or annotation quality.
378	Densities are presented as ind./km. This is not a standardized unit of sampling, as it should be ind./m2 to cross-check with other studies (Simon-Lledo et al. 2019a). Therefore, I cannot interpret these values further.
378	"PL02 had the highest abundance of megafauna with an average value of 18.7 ind./km.": There was no information on the way this imbalanced

	image set (see Table 5-39) was standardized across transects to come up with supposedly 'average abundance per km'.
378	Abundance is very low, ranging from 6.7 to 26.4 ind./km. These are very low values that do not reflect what could be observed with ROV transects in the abyssal plains of the Clarion-Clipperton zone, to my experience. These values do not even compare to the hadal ecosystem which is supposed to be more oligotrophic and lacks the wide distribution of hard substratum with nodules, hence supposedly with lower abundance (~50 to 250 ind./km in the Yap Trench; Zhang et al. 2021). Because of the lack of clear methodology and aberrant abundance values, I therefore put cautiousness on the densities presented which I suspect may not be valid at all.
378	The low abundances and dominance of large-sized organisms suggest that the photographs obtained of the seafloor were low in quality and insufficient to image organisms with 20 mm maximum dimension as recommended in ISBA/25/LTC/6/Rev.3
378	Based on the observation made on the previous comment, I fear that ind./km values were calculated based on the total abundance observed over a transect of, for instance, 65 km for transect PL01. Hence, the unit 'ind./km' should be 'ind./km of the distance traveled by the camera'. However, this is not representative of the actual distance imaged with the towed camera. In PL01, 1127 images were taken, which means on average, 1 image every 58 m. Therefore, there is undoubtedly a large distance between each image (at least 50 m, considering an image imprint is max. 4 m high). If this was the case, this considerably reduced

	the presented abundance of megafauna per km which should probably be 10 to 12 times higher (based on image overlap) than what is actually presented (therefore reaching 6.7*10=67 ind./km to 26.4*12=316.8 ind./km). Therefore, we ask the authors to explain the methods to compute these abundances and to rectify abundance if needed.
380	Figure 5-88: There is something wrong with this graph as it does not reflect the taxa proportions described in Figure 5-85 (e.g., for Annelida). The veracity of the results presented is therefore questioned.
380	There is a clear lack of discussion of the results with previous studies in the area, or even in the Clarion-Clipperton Zone.
381	There is no methodology detailing how the clustering was performed, and this has strong implications in the outcome of the grouping. Furthermore, biological variables usually require mathematical transformation before being used in multivariate analyses such as clustering. Therefore, I cannot further interpret Figure 5-89.
381	The analyses provided here are sparse, difficult to interpret and unconventional. - Rarefaction curves are missing to evaluate the sampling effort and the behavior of the diversity. - β-diversity among transects are not provided, not even a Venn diagram. - Megafaunal composition compared to the terrain (Figure 5-86, 5-87 and 5-88) is qualitative and should be investigated based on several numerical descriptors of the terrain (e.g., slope, depth,

	curvature) with a redundancy analysis (RDA). - Megafaunal composition was not assessed based on nodule density which can have significant influence on biological communities (Simon-Lledo et al. 2019b).
Megafauna references	Simon-Lledó, E., et al. (2019b). "Ecology of a polymetallic nodule occurrence gradient: Implications for deep-sea mining." Limnol Oceanogr 64(5): 1883-1894. Simon-Lledó, E., et al. (2020). "Multi-scale variations in invertebrate and fish megafauna in the mid-eastern Clarion Clipperton Zone." Progress in Oceanography 187. Simon-Lledó, E., et al. (2019). "Megafaunal variation in the abyssal landscape of the Clarion Clipperton Zone." Prog Oceanogr 170: 119-133. Durden, J. M., et al. (2021). "Megafaunal Ecology of the Western Clarion Clipperton Zone." Frontiers in Marine Science 8. Zhang, D., Zhou, Y., Yang, J., Linley, T., Zhang, R., Lu, B., ... & Wang, C. (2021). Megafaunal community structure from the abyssal to hadal zone in the Yap Trench. *Frontiers in Marine Science*, *8*, 617820. Gooday, A. J., Lejzerowicz, F., Goineau, A., Holzmann, M., Kamenskaya, O., Kitazato, H., ... & Wawrzyniak-Wydrowska, B. (2021). The biodiversity and distribution of abyssal benthic foraminifera and their possible ecological roles: a synthesis across the Clarion-Clipperton Zone. *Frontiers in Marine Science*, *8*, 634726.
388	The underway observations state that observations were taken at specific times per day. Therefore, this data is of limited use, and not

	sufficient to make any conclusions about seabirds, turtle or marine mammals in the area. Normal practice for MMO/biodiversity is to have dedicated observers on the bridge. There is no reference to the very comprehensive PAM study in the executive summary or elsewhere in the report, despite the study showing a "rich biodiversity in the area". Which contradicts the statements from the scanty underway observations that biodiversity is low.
405-406	This section is just providing textbook knowledge and also contains significant mistakes (microprofiling quantifies DOU - not TOU). The other way round with 'benthic incubators' (if that means benthic chambers or enclosures). Obviously, no observations have been performed as part of the baseline studies. This is a major shortcoming as the oxygen uptake (as the EIA also states) is a key variable to assess overall community activity (function). SCOC correlations with water depth as that of Zheng et al. 2023 do not help at all to replace baseline measurements of oxygen consumption that post collector test measurements can be compared to. Also information on other biogeochemical constituents (e.g., pore water nutrients, carbonate system) are largely missing (see also sediment parameter overview in tables starting on page 720 and 726, respectively - which do not list biogeochemical data (e.g., nutrients, chlorophyll/pigments etc.) obtained from benthic samples. Also potential impacts on seafloor biogeochemistry and biogeochemical functions are not addressed in part 6.
407	No bioturbation studies were conducted in the contract area. No community oxygen consumption study either.

Chapter 6	
413-414	The section lists all impacts that are connected to mining. Why is it not specific to the planned activity (i.e., leaving out tailings discharge / spills, riser system etc.? At least there should be a note in all cases where issues are listed that do not apply to the planned activity.
413	A Finite Volume Coast and Ocean Model alone does not account for sediment redisposition or settling. Please give more detailed information on how sedimentation is represented in this model.
419	There is a wide range of particle sizes in deep-sea sediments, and it is therefore surprising only one particle size was used in the model, with one settling speed therefore too. Please repeat the modelling efforts and explore the effect of particle size and settling speed. Further, provide the 95% confidence interval, not just the average of the results. It is unclear whether the flow rate included is horizontal or vertical. It is unclear how the collector plume flow rate as initial momentum was incorporated into the model, if at all. Please clarify.
429	What is meant by that the selected area does not have 'any critical habitat in it'? How can nodule removal have no significant effect on the physico-chemical environment?
430	The description of the fate of suspended material should be also referring to the modeling presented in the EIA and not only to other studies from the literature.
436	How can the impact of a removal of nodules, i.e., all settling surface available to sessile organisms that require hard substrates be 'non-significant'?

Chapter 7	
437-438	The section lists all impacts that are connected to mining. Why is it not specific to the planned activity (i.e., leaving out tailings discharge / spills, riser system etc.? At least there should be a note in all cases where issues are listed that do not apply to the planned activity. In general, the lack of specific studies on certain types of impacts is generally used in the text as an argument of absence and/or low effect. However, this must be observed much more carefully, and the conclusions must be more modest. Furthermore, some assumptions seem to consider the benthic community as being uniform and with very similar responses, however this does not reflect what we know to date in relation to responses and proposals for mitigating these impacts in benthic communities.
438	Studies e.g. Stratmann et al. 2018 (Limnology & Oceanography) have shown that even mobile fauna were affected by a small-scale sediment disturbance event. Hence, the collector test will impact even mobile bentho-pelagic species.
439	For the benthic environment, only the compaction of sediment and sediment disturbance is mitigated, no other identified impact on the physico-chemical environment. No mitigation strategy for mid-water layer and the mitigation strategy for the epipelagic is not really a mitigation strategy but complying with established international regulation.

445	Which studies have reported an increase in microorganisms after sediment disturbance and compaction. Revisiting the Benthic Impact Experiment DISCOL after 26 years, Vonnahme et al. 2020 (doi: 10.1126/sciadv.aaz5922) found microorganism abundances still reduced in the most disturbed areas.
446	The deduction that due to the 0.25km size of the region, the negative effects on gene flux, community structure and connectivity would not be serious, is in no way supported by evidence.
447	Very nice that first results from MantaI impact are shown (July 2023). How do these abundance values correlate to the pre-impact abundances? How were samples collected? It would be excellent to get more information.
447	It is unclear how many samples per station were taken and used in analyses. The meiofaunal abundances seem very low, about one order of magnitude lower than for example in the CCZ. What about areas closer to the BPC site, how do they compare to these results?
449	The cited studies were not conducted as part of MIDAS or MiningImpact 1, but were conducted in the context of oil and gas exploration in Norway. The coral species that this EIA refers to are not comparable to the coral species living at the test site.
449	The environmental conditions at ~1500m water depth are very different from ~4000-5000m water depth and the species living at the specific sites are adapted to these conditions. Therefore, one cannot directly

	infer the effect of 1cm sediment addition at 1500 to 4000/5000m water depth. Citing a conference abstract and not a peer-reviewed study from a completely different deep-sea environment is not supporting the argument that no asphyxiation will be caused in macrofauna by this experiment.
451	Please present this 'unpublished data'.
462	The literature cited for the ecotoxicological effects/ accumulation of metals in the food web is 35-40 years old and a lot of new studies have been conducted since then. Please check also these newer studies for potential accumulation of metals into organisms (e.g., studies conducted within the MIDAS project).
464	What is the total and organic carbon concentration of the sediment in the test site?
464	"However, the extent to which warming during commercial mining will affect the growth, reproduction, metabolism, and other processes of organisms in the mining area is unclear and requires further study." For this same reason, there is not enough evidence to state that the impacts on community structure, gene flow, etc. cannot be considered low, as assumed on page 446.
449	The section mixes macrofauna and meiofauna, a large variety of areas and experimental settings and a very limited amount of data collected as part of baseline studies to conclude that 'the likelihood of this test causing depths of benthos due to asphyxiation is low'. This is not fully

	convincing. Also limiting plume sedimentation impacts on sediment infauna to potential oxygen limitation is most likely too narrow. What about changes in sediment structure and a possible reduction in organic matter in the new surface layer as the original active and comparably organic matter 'rich' surface layer got buried?
466	Maybe not a “major” fishery area but it could be used. Recommend contacting the North Pacific Fisheries Commission to determine use of this area and potential interactions.
466	It may be right that the relatively small scale of the experiment will limit the effect of fauna. However, in the end the authors base this assumption on this extremely complicated topic (spatial patterns and connectivity of benthic organisms in nodule ecosystems) on one old publication (Foell et al., 1992) stating that organisms in nodule areas are widely distributed.
466	Due to a very limited understanding of the connectivity between species and populations of fauna living in and around the study site, the statement about the wide distribution of macro- and megafauna species in the deep-sea is wrong. In fact, most deep-sea specimens found in areas where deep-seabed mining might happen were singleton or occurred in extremely low numbers. Hence, we do not know whether deep-seabed mining will lead to extinctions of species locally or even globally.
466	What do you mean by ‘lost to post-mining restoration as a habitat’?

Chapter 9	
477	Conditions under which the project would be stopped/altered are not stated. It is likely that some factors may become obvious or be identified in the data that prevent or alter plans, examples include, unsuitable surface conditions (storms) or finding of particular vulnerable species in mining area (e.g. migration event).
481	Are there indications from modeling that particles can reach the seamounts within the time frame of 'the collection tests of collector components period of this project'? If yes, monitoring of a possible plume impact on the seamounts makes sense and longer-term studies of potential effects on the seamount fauna would need to be included in the post collector test phase. Installing turbidity meters for longer-term recordings at the seamount may make sense to monitor elevated concentrations during but also after the test
485	There are no plans mentioned to do observations of sediment community oxygen demand although the relevance of TOU and DOU are discussed in section 5.3.2 and oxygen uptake is also requested as monitoring parameter in ISA guidelines and recommendations (ISBA/25/LTC/6/Rev.3, ISBA/27/C/11)
490	It is unclear how observations with AUV sub-bottom profiler, sediment profile cameras, laser finder and scanners shall be referenced to get absolute values of deposition thickness and the depth of the mining track. After sediments are redistributed, brought into suspension and

	resettle there will be no reference layer anymore that post-test observations can be compared to. The deposited layer will probably not be discernible in sediment profile camera images and if time series of sediment images are collected during the test, the camera will create its own depositional environment. Sediment collection boxes will only provide a few distributed snapshots of the amount of deposited material.
499	Table 9-5: environmental baseline survey workload in 2024: There is an insufficient amount of pre-test monitoring. A sample of n=1 is not sufficient for any form of analyses! pre-test monitoring: PRZ: 1 mooring, 2 box corer, 2 multicorer 1 CTD, 1 vertical plankton net, 1 multinet, 1 lander, 1 AUV CTA: 1 mooring, 2 box corer, 2 multicorer 1 CTD, 1 vertical plankton net, 1 multinet, 1 lander, 1 AUV Plume impact area: 6 box cores and 6 multicores. These are also insufficient samples to determine effects of plumes.
500	Figure 9-22: this is too low resolution to see what samples are taken where. Please increase the resolution of the figure.
504	Monitoring during the phases: no information is given on e.g. how many multicores or box cores will be collected.
505	How can trace metals in organism and isotopes be compared when they are not measured before the test? No information is given on how/if this will be assessed in I-1?

505-507	Dissolved oxygen is mentioned as a parameter to assess from cores. As also mentioned in ISBA/27/C/11 ex site oxygen profiles or core incubations are not considered state of the art, especially in deep waters. Hence, in order to reliably assess oxygen fluxes as a key parameter of ecosystem function (sediment community oxygen consumption as proxy for organic matter remineralization), in situ profiler and / or chamber measurements need to be added to the monitoring plan.
506	Will eDNA be measured in I-1? Trace metals and food-web studies in I-1? There is only one table given on Phase I-1 with no information on details (e.g. if eDNA, ecotox, foodweb-studies will be included). The low amount of samples to be collected questions if all parameters indeed can be accessed. The severe critique, on the fact that there is no replication planned for I-1, was already mentioned.
509	The ultimate point of the bait/trapping experiment is not obvious. How will you compare to untainted animals of the same species? If you see differential gene expression, how is it of value? Baseline surveys should evaluate heavy metal loads in nodule field fauna and in post test mining in the text plot.
501	Fig 9-23 indicates a NW flow direction and AUV flights “downstream”. Yet Figure 4-136, which I believe to be in the CTA, shows an alternating tidal current E-W. At any one time, the plume may be moving thus eastward, no?
Chapter 10	

512	Blue eye actions quote: what is the purpose of this quote in this EIS?
Appendix I	
550	It is good that environmental objectives are included in the EIS. However, these objectives are vague. Ideally objectives should be more SMART (specific, measurable, achievable, relevant and time bound).
560	BPC's Environmental Management System presents some important priorities in developing an EIA. Unfortunately, many of these are not followed in the EIS. Developing thresholds is thought to be an important approach for management and monitoring and their development is stated as being a priority. However, only thresholds for noise are presented in this document. Threshold values for suspended sediment, sediment deposition etc would help management and monitoring.
561	Section 4.3 has some good ideas for effective environmental management actions that are not implemented here, for example “high-frequency (real-time or near-real-time) assessments of environmental impacts should be conducted, with timely warnings for situations that exceed habitat thresholds and prompt adjustments to operational plans. Combine digital and visualization systems to assess environmental impacts at high frequency (real-time or quasi-real-time), provide early warning of exceeding biohabitat thresholds, and adjust the plan of work in time. Mitigation, restoration, and compensation measures should be taken to reduce environmental impacts, and the effectiveness of these measures should be evaluated.” Implementing these ideas during test

	mining provides the foundation for continual improvement and experience in such assessments that can be taken into the development of full mine plans.
Appendix II	
572-576	It is unclear what the concentration was used in the experiments and how this was translated to the model inputs. More details on the experiments need to be added as the set up can quite easily affect the results, making them less representative of open ocean environments. It is unclear how the settling speed was determined. Please clarify.
578	Figure 6: the correlation seems incredibly weak, and it would be helpful to see a confidence interval and R^2 value of this analysis. The red line suggests a trend, but given the scatter around the line it is hard to believe the correlation is statistically supported. This is worrying as settling speed should be proportional to the particle diameter^2, even if flocculation occurs. Using these data in the model is questionable at a minimum.
Appendix III	
672	List of species: seabirds turtles, mammals – an inventory but no info on location, abundance; good is that danger is given

674-700	Zooplankton: A species abundance list for all samples should be provided. A list with no information on where or when the sample was collected is not very informative, especially not within the context of an EIS. The list is very extensive.
700-705	List of benthic species: A species abundance list for all samples should be provided. A simple list (with no information on where or when the sample was collected) is nice but not very informative, especially not within the context of an EIS. The list is compared to the zooplankton not extensive. Please see comments earlier on quantity of data. Further notes: Porifera not identified, only 2 Foraminifera, Annelida only on family level, very extensive nematode list, only 4 harpacticoid species (this is for sure an underrepresentation; id only on order level), Isopoda (id on order level and 12 “species”). Add all raw data.
706	list of data obtained at stations (sediments). Please indicate what samples were taken in IRZ and PRZ. Add all raw data.
720	list of data obtained at stations (marine chemistry): Please indicate what samples were taken in IRZ and PRZ. Add all raw data.
720&726	The metadata presented in tables starting on page 720 and 726, respectively show that no biogeochemical data (e.g., nutrients,

	chlorophyll/pigments etc.) have been obtained from benthic samples so far. This is considered as a major shortcoming as no information on baseline conditions and their natural variability are available to compare to post-impact data.
726	List of data obtained at stations (biology): Please indicate what samples were taken in IRZ and PRZ. It is very difficult/impossible to get the important information of how many samples were taken in IRZ and PRZ. Add all raw data.

Feedback please send to: pioneer_eis@sina.com