

## Final author responses

### Referee #2

This manuscript is embedded within in a set of recent scientific investigations and cruises dedicated to elucidate the environmental impact of polymetallic nodule recovery from the seabed in the Clarion-Clipperton Zone (CCZ). New data (pore water and solid phase) are presented from deep seafloor tracks affected by deliberate disturbance (e.g. benthic sledges), and these are compared to data from undisturbed reference sites (which are unfortunately presented elsewhere). To investigate the long-term effect of disturbance, the data are supplemented with reactive transport modelling. The manuscript is generally well prepared, and uses a clear language. The manuscript clearly adds an original contribution to the existing literature regarding the CCZ, and puts the current work into perspective with appropriate referencing to previous work. However some important problems and shortcomings need to be resolved.

#### Major comments

[1] The approach to determine the disturbance depth (Section 2.3) is highly unclear. In my guess, the authors use a shifting window approach, whereby the depth profile of the disturbed site is shifted down compared to the reference site. Starting from a zero shift, the shift is gradually increased. At each shift, the Pearson correlation coefficient  $r$  is calculated. Somehow the best fit is determined (but is not explained how this is done – maximum  $r$  value?). Given that this method sustains one of the important conclusions (solid phase Mn can be used as to determine the disturbance/removal depth), I would expect more details on how it actually is done and its robustness. A figure of how the Pearson correlation coefficient varies as a function of the shifting depth seems to be a critical inclusion.

**Author's response: In line 246-248, we explain that “the highest positive linear correlations of solid-phase Mn contents ( $r_{Mn} \sim 1$ ) between the disturbed sites and the respective undisturbed reference sites (Table 1) were used to determine the depths of the disturbances.” Thus, the maximum  $r$  value is used.**

**The robustness of the disturbance depth determined with the Pearson correlation using solid-phase Mn contents is verified by applying the same correlation/disturbance depth on the TOC contents as described in line 248-252. As this correlation of the TOC contents yields to Pearson correlation coefficients between 0.73 and 0.91, the estimates for the disturbance depth based on the correlation using solid-phase Mn contents may be supported as written in line 336-339.**

[2] The optimal shifting window approach is now derived only from the Mn profile, and then verified with the TOC and porosity profiles. What if one combines the data from Mn, Toc and porosity profiles to obtain a weighted optimum to determine the “best disturbance depth”. Would this provide a more reliable measure?

**Author's response: The idea of primarily using solid-phase Mn contents as a tracer for sediment removal is that it could be used during the monitoring of industrial-scale mining activities in the area of the CCZ (as pointed out in line 558-560). We do not see how a combination of solid-phase Mn contents and TOC contents will obtain better estimates for the disturbance depth as we are already applying the correlation on both parameters. Furthermore, we do not feel that a disturbance depth could be derived from the correlation of porosity as the data does not show any variation over depth (see figure 4).**

[3] Replication is fundamental to proper scientific investigation. The current analysis lacks such replication. The comparison of Mn and TOC profiles appears to be based on the comparison of a single core within the disturbed track compared to a single core within the reference area (some 5 kilometres away). Although I am aware that core retrieval is not obvious in deep sea conditions, replication of sediment cores is essential for the conclusion reached (solid phase Mn can be used as to determine the disturbance/removal depth). This can only be done if the variation within the disturbed area and the reference area is sufficiently small. In order to obtain

an uncertainty on the estimated disturbance depth one should compare replicate cores within the disturbed track and reference area.

**Author's response:** We agree with the referee's objection that more sediment cores from adjacent undisturbed sites and duplicate/triplicate sediment cores from the disturbance tracks would certainly increase the robustness of our study. However, due to the fact there was a large interdisciplinary scientific team participating during cruise SO239, it was not feasible to use more MUC sediment cores for geochemical investigations. Furthermore, retrieving undisturbed/natural sediment cores adjacent to the disturbance track is only possible using ROV-operated push cores with visual control of where the cores are taken (in this case: to be sure to not hit the disturbance track), which can only be performed with a very limited amount of push cores (due to the storage space on the ROV during the dive).

Although, we certainly would like to have more analytical data from replicate sediment cores in order to strengthen the study, geochemical data of pore-water and sediment generally give an integrated signal at least over several tens of meter – especially in deep-sea environments at sites distal to seamounts, so that we are confident that our data is representative for the area and the impact of the disturbances.

[4] The text speaks throughout refers to “mining-related removal” of sediments, while in these seabed trials it is in fact the “mining-related displacement” of sediment, as trenches are made and sediment is simply pushed aside (with a limited resuspension that carries sediment far away, it appears). The bio geochemical consequences of the deposition are not studied (and not discussed). Somehow the aspect of removal is considered more important than the side way deposition part. Moreover, how relevant is this “removal operation” for the envisioned mining approaches? I guess that for actual mining operations, filtering and sorting of polymetallic nodules will happen right at the seafloor (5-20 cm of sediment removal would imply huge volumes to be transported), and so the deposition part will also equally crucial (sediment will be deposited behind the nodule collection vessel?)

**Author's response:** As described in Chapter 3.1, based on the visual impact inspection, the sediment surface was scraped off during the small-scale disturbance experiments and then indeed pushed aside/piled up next to the freshly exposed sediment surface. We only have taken/studied sediment cores from the freshly exposed sediment surface and not from the pushed-aside sediment pile for several reasons: (1) biological sampling also focused on the freshly exposed sediment surface, and thus, we ensure that we can compare/match biological and geochemical data sets. (2) “mining-related sediment removal” may be the more likely/important scenario for the impact of large-scale industrial deep-sea mining activities than “mining-related sediment displacement” (in the sense of pushed aside/piled up sediments), because current mining technology aims at sucking up nodules and sediment. As mining-related activities will disturb a much larger seafloor area on the scale of km<sup>2</sup>, “mining-related sediment displacement” in the sense of pushed aside/piled up sediments will most probably not play a significant role as the mining equipment will probably disturb these pushed-aside/piled-up sediments again. However, as mentioned in line 73-84, there is no clear consensus on the most appropriate mining techniques for the industrial exploitation of manganese nodules so far, and thus, the prediction of the environmental consequences including the displacement/removal/re-suspension of sediments is difficult. To our knowledge, current literature on mining-related environmental impacts cited in the presented manuscript does not consider “mining-related sediment displacement” in the sense of pushed aside/piled up sediments as a major impact.

We certainly agree that the transport of re-suspended sediment after the disturbance and during industrial filtering/sorting as well as bottom blanketing during/after the disturbance is key for the evaluation of mining-related environmental consequences and especially for biogeochemical conditions and processes in the sediments. However, during the studied small-scale disturbance experiments probably much less sediment was re-suspended than what is to be expected during large-scale industrial deep-sea mining activities. The studied old benthic impact experiments do not allow

**studying of this important environmental aspect. We are currently working on this topic as part of the new JPI Oceans project “Environmental Impacts and Risks of Deep-Sea Mining” (MiningImpactII), which aims at independently accompanying an industrial mining trial.**

[5] The description of what model simulations are performed and how these simulations are carried out is lacking in the methods section (or alternatively at the start of section 3.4). So the reader does not know what is actually simulated in Figs 5 and 6. In essence, one first fits steady state depth profiles to the data depth profiles of the undisturbed site (how is this done? E.g. what is the goodness of fit criterion?). Secondly, one removes the top L centimeter in the model domain, keeping the boundary conditions constant. Subsequently, one performs a transient simulation until a new steady state is reached. This simulation procedure should be clearly described.

**Author’s response: We will add the description of the model simulations in section 2.4. as follows:**

**“We have applied a transient transport-reaction model for the sites in the BGR-RA and IOM areas (Table 1). These sites were chosen due to distinctively different sedimentation rates and OPD (Table 2). We have adapted the code of the steady state transport-reaction model, which was originally presented by Volz et al. (2018) and used pore-water oxygen, NO<sub>3</sub><sup>-</sup>, Mn<sup>2+</sup> and NH<sub>4</sub><sup>+</sup> data as well as TOC contents of GC sediment cores from the same study as undisturbed reference data (Table 1; Table 2). Thus, the model parameters and baseline input data used for the transient transport-reaction model are the same as presented in the study by Volz et al. (2018).”**

**Furthermore, we will add a sentence in the figure caption of figure 5 that the analytical data shown in the graph represents the undisturbed/baseline data as following:**

**“Figure 5: Model results of the transient transport-reaction model adapted after the steady state transport-reaction model presented in Volz et al. (2018) and fit of the simulated profiles with the analytical data for undisturbed sediments at current steady state geochemical conditions (add profile) and for the new steady state geochemical system after the disturbance (purple profiles) for (a) the EBS disturbance in the German BGR-RA area after 15 cm have been removed from the sediment surface, while the boundary conditions and (b) the IOM-BIE disturbance in the eastern European IOM area.”**

[6] Line 214. Wrong definition of porosity. The porosity is always defined as the volume of the pore water (including dissolved salts) over the total volume, and hence not the salt-free volume fraction. Moreover, the porosity cannot be determined gravimetrically in a direct fashion. One determines the H<sub>2</sub>O content of the sediment gravimetrically as well as the solid phase density. From this and salinity one can calculate porosity (accounting for salt content of pore water).

**Author’s response: We will adapt this part as follows:**

**“The mass percentage of the pore water was determined gravimetrically before and after freeze drying of the wet sediment samples.”**

Model comments:

Figure 6 is a zoomed-in version of figure 5. Why is the data not plotted on figure 6 (as in figure 5)? Even if the data is off, it is highly instructive to include the data in the plot.

**Author’s response: We will add the analytical data in figure 6.**

Sup Fig 1. Why is the porosity data fitted in such a poor way? Panel a: Why use an exponential if the data is constant? Panel b: the value at infinity should be 0.71 and not 0.65.

**Author’s response: As pointed out in line 283-289, the porosity is commonly following an exponential decrease in transport-reaction models indicative of the compacting sediment. However, we agree with the referee’s objection that the porosity fit could be improved. We decided against changing the already published porosity parameterization presented in Volz et al., 2018, DSR I for two reasons: (i) The transient model results simulating the removal of the surface layer presented here should be directly comparable to the previously published steady state model results for**

undisturbed/natural sediments and (2) while the porosity fit towards deeper sediments appears to be 'off' by about 0.03 porosity units, the difference will not alter the overall outcome of the model.

Figure 6. In both panels a and b, the final steady state does not return to the initial state (difference between red and dark blue curves). Explain better what the initial state is, and why this happens.

**Author's response:** As indicated in the legend of figure 6, the red profile represents the simulation results for 1 day after the surface sediment layer has been removed (see Table 3 for disturbance depth). Thus, the red profile does not indicate the initial steady state geochemical conditions in undisturbed sediments. In order to clarify this, we will add another profile for the simulation of pre-disturbance steady state geochemical conditions and alter the legend accordingly.

In Fig 6 panel b (IOM-Bie site), the organic matter decreases almost linearly with depth. This is rather strange behavior (should be an exponential decrease)

**Author's response:** As explained in line 290-292, organic matter was treated as three reactive fractions (3G) in the transport-reaction model including labile, degradable and refractory fractions. The exponential decrease of simulated organic matter contents in the top sediments (due to the presence of different reactive fractions) and below is common during diagenetic modelling (see Arndt et al., 2013, ESR, <http://dx.doi.org/10.1016/j.earscirev.2013.02.008>; Mewes et al., 2016, EPSL, <http://dx.doi.org/10.1016/j.epsl.2015.10.028>; Volz et al., 2018, DSR I;). As only refractory organic matter is left at depth, the curvature might be difficult to see.

Why are there no pore water NH<sub>4</sub> data collected? This would provide a strong model constraint on the N cycle (which is now unconstrained, as only a depth profile of NO<sub>3</sub> is available)

**Author's response:** We agree with the referee's objection and we have tried to measure pore-water ammonia onboard (flow-injection method) using fresh untreated pore-water splits as well as in the home lab using frozen samples (-20°C) with the ortho phtalaldehyde (OPA) method in a QuAAtro continuous segmented flow analyzer (Seal Analytical) for low concentration ranges of 0–5 µM. Unfortunately, the concentrations of pore-water ammonia were mostly below detection limit. We assume that ammonia is immediately oxidized to nitrite/nitrate under oxic conditions and therefore it cannot be detected using these methods.

Terminology:

Why are the disturbances referred to as "small-scale disturbance"? The areal coverage of the disturbance is not relevant for the disturbance depth and recovery time, only the vertical impact matters. The disturbance to the sediment geochemistry (and recovery time) is equally large when doing a single track than when doing a whole area covered by multiple tracks. The use of "small-scale" throughout the ms seems to suggest that large-scale application would have different impacts and recovery times (which should not be the case).

**Author's response:** We do not agree with the referee's objection. It is important to point out that the presented study focuses on the impact of small-scale disturbances because it can only be considered an approximation to the environmental impact of industrial-scale mining activities. Coming back to major comment [4] by referee #2, the presented small-scale disturbances scraped off the sediments, which were then pushed aside and piled up next to the freshly exposed sediment surface. As described before, this "mining-related sediment displacement" will most probably not be the case during industrial-scale mining activities. As mentioned before, presumably much less sediments were re-suspended during the small-scale disturbances presented in this study than what is to be expected by industrial-scale mining activities. While the effect of bottom blanketing by re-suspended sediments on biogeochemical processes within the small-scale disturbance tracks can be mostly neglected, it certainly has to be considered when investigating industrial-scale mining activities. In addition, in the presented study, we show that porosity is not affected by significant sediment

compaction from the weight of the disturbance device. Industrial mining equipment is expected to be much heavier than the disturbance devices used during the small-scale disturbance experiments presented in this study. Also lateral effects (e.g. transport of solutes, easier recolonization by fauna and resulting bioturbation) play some role for the single (2-m wide) tracks compared to large-scale industrial disturbances with widths of hundreds of meters to kilometers.

For these reasons, we would prefer to keep the distinction of “small-scale disturbance” throughout the presented manuscript.

The state of the sediment is referred to as “equilibrium”, and this is not a proper word choice. Equilibrium refers to a closed system that does not change. Steady state is the proper term for an open system that does not change. Sediments are open systems.

**Author’s response: We will replace “re-equilibration/equilibrium” by “the system will return to (new) steady state geochemical conditions” accordingly.**

Other comments:

The title is not so descriptive for the results presented. May well be changed to “Disturbance depth and recovery time of sediment geochemistry in Clarion-Clipperton zone after surface disturbance”

**Author’s response: We do not agree with the referee’s objection. Indeed, we are determining the disturbance depth as it represents a crucial input parameter for the application of the transport-reaction model. However, as described in line 406-412, the determined disturbance depths as part of this study are well within the range of previous estimates for the disturbance depths, and therefore, it is not a new finding. Using the transport-reaction model, we present estimates for how long it takes until the sedimentary geochemical system reaches steady state conditions. As pointed out in line 126-128, our second main goal is to assess the consequences of the disturbance on redox zonation and element fluxes, which are presented in Figures 7 and 8.**

For these reasons, we would prefer to keep the title.

L1 Title: “element fluxes” -> no element fluxes are determined

**Author’s response: Fluxes of oxygen, nitrate and ammonia are presented in Figure 7.**

L26 conceived -> understood

**Author’s response: We will change this accordingly.**

L49 sediments -> sediment

**Author’s response: We will change this accordingly.**

L51 millennium-scale

**Author’s response: We will change this accordingly.**

L51 It does not matter whether these experiments are small-scale -> the areal extent does not influence the recovery time

**Author’s response: We do not agree with the referee’s objection. It is important to point out that the presented study focuses on the impact of small-scale disturbances because it can only be considered an approximation to the environmental impact of industrial-scale mining activities for reasons pointed out in the first comment of referee #2 in Terminology.**

L81 delete ref to ms under review

**Author’s response: We will change this reference to Hauquier et al., 2019.**

L96 estimates

**Author’s response: We will change this accordingly.**

L132 - The sampling description is unclear – both the disturbed and undisturbed sites were visited on the same cruise. The way the text is written confounding and suggests otherwise.

**Author’s response: We do not fully understand the referee’s objection. In line 132-142, we clearly state that MUC and GC cores are taken from undisturbed reference sites while**

video-guided PC cores were taken within the disturbance tracks. In addition to the sampling description, we present all studied sediment cores with the information of type and age of the disturbance for the PC cores in Table 1. Therefore, we feel like the sampling description is clearly presented.

L158 ...with Mn nodules varying in size and spatial density...  
**Author's response: We will change this accordingly.**

L189 repetitious  
**Author's response: We will delete this sentence.**

L194 the mass of an object (expressed in kg) is independent of depth or pressure  
**Author's response: We will delete "under normal atmospheric pressure" accordingly.**

L238 What is negligible? Less than ?? %  
**Author's response: The line number seems to be incorrect - we are not sure what is meant here.**

L247 subscript i is missing for x and y symbols in formula  
**Author's response: We will insert subscript i for x and y accordingly.**

L261 delete double ref  
**Author's response: We will delete the double reference.**

L266 We have adapted the model...-> adapted in what way?  
**Author's response: The code was adapted from steady state to transient, while the model parameters are same as presented in Volz et al., 2018, DSR I. We will add this in the model description in section 2.4.**

L268 It is unclear how the steady model was fitted to the data. See comment above.  
**Author's response: We will add to the model description in section 2.4 as follows:**  
"We have adapted the code of the steady state transport-reaction model, which was originally presented by Volz et al. (2018) and used pore-water oxygen, NO<sub>3</sub><sup>-</sup>, Mn<sup>2+</sup> and NH<sub>4</sub><sup>+</sup> data as well as TOC contents of GC sediment cores from the same study as undisturbed reference data (Table 1; Table 2). Thus, the model parameters and baseline input data used for the transient transport-reaction model are the same as presented in the study by Volz et al. (2018)."  
**Simulated profiles were fitted to the data by adjusting the fitted parameters listed in the Supplementary Table 2.**

Eq (5) : the assumption in the model is steady state compaction (porosity not dependent on time) – should be mentioned  
**Author's response: It is mentioned in line 283-285.**

L293 Eq(8) + is missing in formula  
**Author's response: There is no Eq. 8 throughout the manuscript so we are not sure what is meant here.**

L333 Total bulk Mn contents in the upper 25 cm -> strange term. I guess one implies "Mean solid-phase Mn content in the upper 25 cm"  
**Author's response: We will adjust this sentence as follows:**  
"Bulk Mn contents in the upper 25 cm of the sediments at the disturbed sites are between 0.1 and 0.9 wt% (Fig. 3)."  
**We are giving the range of Mn contents in the sediments, not the mean value.**

Fig 1. Mention in caption what the white areas denote. Add black line around green areas for clarity.  
**Author's response: We will change this accordingly.**

Table 2 POC flux -> mg C m<sup>-2</sup> d<sup>-1</sup>  
**Author's response: We will change this accordingly.**

Table 3.  $r_{Mn}$  : is this the maximal value? How is this value determined – see comment above  
**Author's response:** In line 246-248, we explain that “the highest positive linear correlations of solid-phase Mn contents ( $r_{Mn} \sim 1$ ) between the disturbed sites and the respective undisturbed reference sites (Table 1) were used to determine the depths of the disturbances.” Thus, the maximum  $r$  value is used. We will add this in the table caption as follows:

**“Table 3: Calculated Pearson correlation coefficients  $r_{Mn}$  and  $r_{TOC}$  for the determination of the disturbance depth of various small-scale disturbances investigated in the framework of this study (compare Table 1). For both correlations, the highest positive linear Pearson coefficient for solid-phase Mn contents ( $r_{Mn} \sim 1$ ) between the disturbed sites and the respective undisturbed reference sites was used.”**

L392 “regional phenomenon” -> what is meant by this: that it is only local or that it common across a broad area of the CCZ? How big is the region?

**Author's response:** We will change this sentence accordingly to:

**“The fact that the solid-phase Mn maxima in the surface sediments appear to be a regional phenomenon across the CCZ area as it has been observed throughout the different exploration areas studied in the framework of this study (Volz et al., under review)”**

L413 “were removed” -> but also placed just on the side of the track

**Author's response:** We will add “...and pushed aside”.

L468 “Mining-related removal” Is this really mining-related? Is it removal? As it placed just on the side of the track...

**Author's response:** We will delete “mining-related”. As we are studying only sediment cores from the (freshly) exposed sediment surface after the sediment has been scraped off (line 312-314), we feel confident that we can talk about sediment removal.

L473. Which organisms are the dominant bioturbators? This is crucial information to provide insight into their return time? Large motile surface-dwellers (e.g. sea cucumbers) may return faster...

**Author's response:** We agree with the referee's objection that it would be important to know which organisms are the dominant bioturbators in sediments of the CCZ. To our knowledge, data/studies on this topic are very rare for the deep sea and do not exist for the CCZ. Therefore, we have pointed out in line 295-304, “due to the lack of data on the re-establishment of bioturbation, i.e. the recovery of the bioturbation ‘pump’ after small-scale disturbance experiments, we have tested the effect of different bioturbation scenarios in the transport-reaction model. For the different post-disturbance bioturbation scenarios, we have assumed that bioturbation is inhibited immediately after the disturbance with a linear increase to undisturbed reference bioturbation coefficients (Volz et al., 2018). Based on the work by Miljutin et al. (2011) and Vanreusel et al. (2016), we have assumed that bioturbation should be fully re-established after 100, 200, and 500 yr. As the modelling results for the different time spans were almost identical, we only present here the model that assumes bioturbation is at pre-disturbance intensity 100 yr after the impact (Volz et al., 2018; Supplementary Table 2).” Thus, this is the best approximation of the re-establishment of bioturbation for the application of a transport-reaction model.

Moreover, is there a point for the bioturbators to in return, if the food stock (organic matter) has not yet been replenished? Figure 5 shows that it takes over 10.000 years to get the organic matter back up in the surface layer.

**Author's response:** As elaborated in the previous response, due to the lack of data on the re-establishment of bioturbation after disturbances, we have tested different post-disturbance bioturbation scenarios in the transport-reaction model based on the work by Miljutin et al. (2011) and Vanreusel et al. (2016). Thus, the time span after which bioturbation is re-established is based on actual biological studies on disturbances in the study area.

Although we are not experts in biology/bioturbators, we think that bioturbators not only return once the surface layer is fully re-established but that they can already return once the surface layer is only partly re-established. At this point, some “fresh” organic matter has been deposited, which is probably a point for bioturbators to return. Vonnahme et al., revised, Science Advances show that microbe abundances may recover on decadal scale, i.e. before the reactive surface layer has been redeposited. Hence, fauna is likely going to graze on those microbes and thus recolonize on decadal time scales as well.

L487 Unclear why denitrification would increase when OPD gets deeper -> explain better

**Author’s response:** We are not saying that denitrification increases once the OPD shifts deeper into the sediment, we are actually implying the opposite: denitrification is weakened in the oxic environment due to the fact that aerobic respiration is favored over denitrification. Therefore we are writing in line 486-490 that “interestingly, during the transition time (at which point solute profiles slowly shift towards their pre-disturbance shape) when oxygen is still present at depth but aerobic respiration in the upper sediments has already begun to pick up,  $\text{NO}_3^-$  concentrations are strongly elevated in the BGR sediments (Figs. 5 and 6). This is due to the fact that  $\text{NO}_3^-$  is not consumed during denitrification or the Mn-annamox reaction in the presence of oxygen (Mogollón et al., 2016; Volz et al., 2018).”

We do not feel that this needs clarification.

L554 (and elsewhere) equilibrium -> steady state (see comment above)

**Author’s response:** We will replace “re-equilibration/equilibrium” by “the system will return to (new) steady state geochemical conditions” accordingly.

Fig 7. Plot all times on the x-axis

**Author’s response:** The timescale will be added to the figure.