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Interactive comment on "Impact of small-scale disturbances on geochemical conditions, biogeochemical processes and element fluxes in surface sediments of the eastern Clarion-Clipperton Zone, Pacific Ocean" *by Jessica B. Volz et al.*

Anonymous Referee #2

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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.

[2] The optimal shifting window approach is now derived only from the Mn profle, 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?

[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

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

[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 that 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 deposited behind the nodule collection vessel?)

[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.

[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_2O 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).

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 is the data is off, it is highly instructive to include the data in the plot.

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

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.

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)

Why are there no pore water NH_4 data collected? This would provide a strong model constraint on the N cycle (which is now unconstrained, as only a depth profile of NO_3 is available)

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).

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.

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"

L1 Title: "element fluxes" -> no element fluxes are determined

L26 conceived -> understood

L49 sediments -> sediment

L51 millenium-scale

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

L81 delete ref to ms under review

L96 estimates

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.

L158 . . .with Mn nodules varying in size and spatial density . . .

L189 repetitious

L194 the mass of an object (expressed in kg) is independent of depth or pressure

L238 What is negligible? Less than ?? %

L247 subscript i is missing for x and y symbols in formula

L261 delete double ref

L266 We have adapted the model . . .-> adapted in what way?

L268 It is unclear how the steady model was fitted to the data. See comment above.

Eq (5) : the assumption in the model is steady state compaction (porosity not dependent on time) – should be mentioned

L293 Eq(8) + is missing in formula

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"

Fig 1. Mention in caption what the white areas denote. Add black line around green areas for clarity.

Table 2 POC flux -> mg C m-2 d-1

Table 3. r_Mn : is this the maximal value? How is this value determined – see comment above

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?

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

L468 "Ming-related removal" Is this really mining-related? Is it removal? As it placed just on the side of the track . . .

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. . .

Moreover, is their 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.

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

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

Fig 7. Plot all times on the x-axis