

## Final author responses

### Referee #1

The work constitutes one of the first of its kind, documenting the sedimentary biogeochemical impact of simulated disturbances that are likely to arise from deep-sea mining. Overall the study is very well designed, scientifically sound and well executed. The report is well written and contributes to an international framework of projects dealing with the deep-sea impacts of mining. Looking from a purely scientific/geochemical angle, the results are not surprising from a sediment diagenesis point of view, but I find it very important that the early diagenetic framework is able to explain and guide (to some extent) these type of impact assessments.

There is a couple, relatively moderate shortcomings which can be addressed in the final version:

- It would be great to see more of the undisturbed/baseline sediment geochemical profiles. I understand there will be a companion manuscript on this, but reproducing more of those results here maybe can be possible. Especially undisturbed OPD – TOC profiles would be interesting to see in this paper as well.

**Author's response: We understand the referee's objection. However, we would like to point out that in fact we are showing undisturbed/baseline data (published in Volz et al., 2018, DSR I) in the present manuscript:**

**In figure 4, TOC contents are shown for undisturbed reference surface sediments for the different study sites, which are taken from Volz et al., 2018, DSR I (as stated in the figure caption for figure 4). Furthermore, TOC contents as well as ex-situ oxygen, nitrate and dissolved manganese concentrations for undisturbed deep sediments are shown in figure 5, which are used as baseline input for the transport-reaction model of the benthic disturbances. As this seems not to be clearly described in the present manuscript, we will adapt the methods section in Chapter 2.4 about the geochemical model setup and reaction network as following:**

**“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 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 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.”**

- I find the discussion very substantial and objective. However I was wondering if more input to policy/blue economy can be given with a few more sentences. As it stands the message is that the uppermost organic-rich part is removed and a lot more oxygen seeps into the sediment. What will the policy side make of this, may sounds like a positive result? What is really the impact here? The fact that the system comes back to original steady state is one thing, but deep-seafloor can be disturbed naturally, through different means.

**Author's response: Deeper OPD means lower flux of reduced substances (NH<sub>4</sub>, Mn etc) to the seafloor and more CO<sub>2</sub> production from POC degradation (i.e. lower pH) but from our current knowledge it is difficult to quantify this impact due to changed biogeochemical fluxes and also if this impact is good or bad. It will certainly change the**

microbial community structure and possibly meio/macro/megafauna communities. It is also difficult to assess at the moment whether this change may trigger a tipping point in combination with natural changes that are ongoing (e.g., bottom water warming, acidification, changes of carbon flux to the seafloor etc).

Therefore, based on our current knowledge, we can't give further advices for policy/blue economy.

Several line-by-line suggestions:

L46 and many more places: Please consider the repeated use of (re)equilibration of the study system. Rather than chemical equilibrium, I think what is meant here the system becomes steady-state over the course of the study's simulation timeline.

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

L115 A more specific verb can replace 'influencing' here, maybe 'increasing'?

**Author's response: We will replace "influencing" by "increasing" accordingly.**

L396 This can be confusing if not elaborated a little more, as it rapidly switches the discussion to glacial-timescales. First, how sure are we on the lower bottom O<sub>2</sub> levels in the LGM, when the seawater was somewhat colder with increasing mixing? If O<sub>2</sub> was still lower, I do not think it would be to a hypoxic extent that it is going to compress all the redox zones. But then how Mn delivery to LGM deep waters, given Mn was is the tracer used in this study? Please elaborate or I would recommend removal of this sentence.

**Author's response: We will elaborate more on this (see paragraph below) as the occurrence of the solid-phase Mn maximum in surface sediments throughout the CCZ is important regarding the use of solid-phase Mn contents/depth profiles as a tracer for the removal of sediment/disturbance depth during the small-scale disturbance experiments, and thus, potential future mining-related activities. We have studied and intensively discussed the diagenetic formation of solid-phase Mn peaks as a result of lower glacial oxygen concentrations in the cited paper Volz et al., under review, EPSL. This manuscript will be accepted and published soon. It also presents all relevant studies regarding the mechanisms and indications for lower oxygen concentrations in the glacial deep Pacific Ocean.**

The paragraph will be adjusted as following:

**"Volz et al. (under review, EPSL) have suggested that the widely observed solid-phase Mn enrichments in CCZ surface sediments formed in association with a more compressed oxic zone, which may have prevailed as a result of lower bottom-water oxygen concentrations during the last glacial period than today. Strong indication for lower glacial bottom-water oxygen concentrations throughout the eastern Pacific Ocean have been provided by a number of independent proxies (e.g., Anderson et al., 2019 and references therein). As a consequence of the condensed oxic zone, upward diffusing pore-water Mn<sup>2+</sup> may have precipitated as authigenic Mn(IV) at a shallow oxic-suboxic redox boundary in the upper few centimeters of the sediments. After the last glacial period, the authigenic Mn(IV) peak was continuously mixed into subsequently deposited sediments by bioturbation causing the observed broad solid-phase Mn(IV) enrichment in the surface sediments (Fig. 4; Volz et al., under review, EPSL)."**

Figure 8 - the timescale is missing, was this deliberately omitted?

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