Biogeosciences

Discussions

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Interactive comment on "DISCOL experiment revisited: Assessing the temporal scale of deep-sea mining impacts on sediment biogeochemistry" *by* Laura Haffert et al.

Anonymous Referee #1

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This paper covers an important subject, which should be of interest to the readers of Biogeosciences. The major finding of this study is the differences in recovery time of deep sea sediments between two methods that can be used during deep sea mining. The authors used an extensive dataset and model porewater and solid phase depth profiles with numerical a numerical model.

In general I think the structure of the paper is a bit confusing. There is an introduction section (section 1) but within this section there is a long discussion/introduction of the general biogeochemistry of the Peru Basin (section 1.1). I think this would be more in place in the methods section as a site description. The results section contains a lot of

discussion where I think it should be better to first clearly describe all the data, which is used in this study. The discussion section is rather short and reads as a conclusion part. I think the discussion and conclusion part can be combined. I think the manuscript is highly under referenced.

Abstract: Line 19: "reduces seafloor nutrient fluxes over centuries" - I think based on this study the authors can only draw conclusions for oxygen. Otherwise the authors should include more data from their modelling study on the effects of these two different sampling techniques. Now the only information given in the discussion (and figures 10 and 11) are oxygen fluxes. I think it would be a good addition to also show other nutrients fluxes from the sediment (for example ammonium/nitrate), which are already in the model I guess. - Can the authors also give some information about the changes/recovery of macrofaunal communities.

Introduction In my opinion section 1.1 is a bit out of place. This section describes processes that are important in general (degradation of organic matter P2 lines 55-59). I would move this to the introduction and make a new section 2.1 with a site description where you discuss all the processes that are important for your study. Also I think this part is under referenced.

In lines 39-41 you describe there were studies that focused on characterization and distribution of macrofauna. I think it would good to shortly explain what was found in these studies in this section because disturbance of macrofauna is important as you also describe in you abstract. I think the last paragraph of this section is a bit out of place or should be better introduced.

Results: It is quite difficult to understand to which plots you refer in this section. Add more figure numbers behind your statements. Also the order of discussing data is a bit strange. You start with porosity but this is plotted in figure 5. You should start with describing figure 3 or change the order of figures.

There is a lot of discussion in section 3.1. I think it would be better to first describe

the porewater and solid phase profiles (in order of the figures) before you start your discussion. Section 3.3 Most of the interesting statements in this section are based on Vonnahme et al submitted or at visual observations. There is not a good link with the data shown in figures 4-6. I think it would be better to compare these instead of discussing visual observations or "repeating" findings presented in Vonnahme et al. submitted.

Conclusions I suggest to strongly shorten this section and only discuss the main conclusions found in this study and not discuss conclusions from other studies.

Figures: The resolution of the figures could be improved.

Detailed comments: Line 17: "(micro)-biological data". Is this in place? There is no microbiological data in this manuscript.

Line 18: how was the depth of this reactive surface layer quantified.

Line 19: "reduces seafloor nutrient fluxes over centuries". From sediment towards the water column? Maybe say benthic release of nutrients.

Line 39: col in recolonization should be in capitals similar to dis in disturbance

Line 43: introduce abbreviation of ROV

Line 47: What do the authors mean with a "state of the art geochemical dataset". I think it would be more appropriate to call the data set extensive, which is indeed the case when taking into account the amount of profiles. However, I would suggest to also include porewater iron as this variable is discussed multiple times throughout the manuscript.

Line 61-63: how deep is the layer where there is organic matter degradation? Does this mean there is no iron of sulfate reduction?

Line 64: which depth is the bioturbation depth approximately?

Line 65: "Oxygen is depleted at a sediment depth of 5 to 15 cm, " Do you mean in general for deep see sediments? Then add a reference.

Line 65: "with the DEA region being on the deeper end". Add reference.

Line 66: "In the slightly shallower region of the Peru Basin seawater-derived nitrate and nitrate from oxic respiration is consumed within the bioturbated upper 20 cm of sediment". Add reference.

Line 70: It is a bit confusing to also refer to this layer as reaction layer as you also have a surface 'reaction layer'. I would use a different name iron-rich layer for example.

Line 70: "formed during glacial times". Approximately how long ago?

Line 70: add reference to :" This reaction layer was formed during glacial times, when organic matter deposition was strongly increased and potential electron acceptors such as oxygen, nitrate and manganese oxide were exhausted closely below the sediment surface. "

Line 79-85: Is this one of the main research questions? This is not described in the abstract.

Line 89: "to artificially disturb the". This means without removing the reactive surface layer?

Line 92: what does less disturbed mean. The sediment depth of disturbance?

Line 97: I would give this information directly when you describe "artificially disturbance".

Line 115: 20 mictoliter of HCl was added to how much sample? 1 mL?

Line 116: "Additional sediment samples ". Do you mean a separate sediment core or the sediment leftovers after porewater extarction?

Line 119: you also measured show manganese data so you should add this to the list.

Line 119-124: Can you add the sample sizes to this paragraph or add this in Table 2.

Line 154: Can you add equation numbers to all equations? That makes it easier to refer to them in the text.

Line 176: shortly describe how the gridsize is distributed.

Line 178: describe that you use organic matter with different reactivities (Multi G approach) and give references for this approach (Jørgensen, 1978;Westrich and Berner, 1984; Middelburg, 1989).

Line 195: I do not see a sharp drop in porosity in figure 5.

Line 195: You should also give the reference to the figure.

Line 195: "and is rich in manganese". Can you add a concentration to this statement, or a range of concentrations.

Line 200: "Organic carbon content in the DEA region oscillates about a mean value of 0.5 to 0.75 wt% in the upper 50 cm,". refer to figure.

Line 202: "With depth the organic carbon content decreases steadily to about 1 wt% at 10 m.b". Add figure reference. Maybe I'm not looking at the right figure but the organic carbon content in fig 3 decreases to almost 0 and is never above 1 wt%.

Line 203-207: This part seems more suitable to the discussion. I think it is better to first discuss the data you show in figures 3-6.

Line 208-210: This is discussion, focus on presenting the data first.

Line 217: "Dissolved manganese is strongly redox sensitive and is absent in the upper oxic zone." Add reference to such a statement.

Line 226-228: Where can I see this Fe data? If you don't want to add this to the manuscript you can add it in an appendix.

Lines 231-235: I would add this part to the methods.

C5

Line 238: There should be a dot after inhomogeneities, Not a comma.

Line 241: Can you further explain why you choose these sites?

Line 248: "which suggests that secondary reactions, such as a semi-labile Fe(II)-oxygen reaction front, additionally shape the oxygen profile:" Why is this reaction not added in the model?

Line 260: "relevant information" is a bit vague.

Line 264-266: Is this data compared to the reference stations?

Line 268: "signs of bioturbation are lacking". how would you see these and are these sign visible at the reference site?

Line 278: Can you give macrofaunal density (add a number).

Line 292-295: These lines are repetition. This is already discussed in the introduction.

Lines 300-304: For me it is difficult to understand how these changes were modeled. Can this be explained in the method section in more detail?

Line 305: "It is assumed that bioturbation is inhibited immediately after the impact with a linear increase to undisturbed reference bioturbation intensity within 100 to 200 years". Where is this assumption based on, can you give a reference?

Line 311: "Upper centimetres". Give a number.

Line 321: For me it is quite surprising that there is such a large difference in such a short period.

Line 346: "overall surface fluxes vary". Where can I see the all the fluxes besides oxygen?

Line 367 (Discussion): first paragraph does not give any new information or does discuss the results found in this study. The discussion reads as a summary/conclusion section.

C6

Line 370: "The associated microbial growth". Add references

Line 379: "..infauna new material for additional degradation and microbial colonization, close the causal cycle." Add reference

Lines 386-388:" through changing the physical characteristics of the sediment surface, i.e. by exposing more compacted sediments with increased shear strength, and removing manganese nodules as hard substrate habitats." Add reference.

Line 389: Where can I find the biological data?

Table 4: - "Number of points in the numerical grid (uneven)". How are they distributed? - "Rate constant for reaction of Mn2+ and NO3". This equation is not given in table 3 - How do the rate constants (last column) compare with values used in the literature? Are they in the same range? I think it would be good to add a column which described the ranges of rate constants used in the literature (with references). - there is no rate constant describing the third secondary reaction (oxidation of Fe2+) - "Monod constant for SO42- reduction". This reaction is not in table 3. If you add this reaction you should also add Fe reduction coupled to organic matter.

Figure 8: - Total Co org. Not a very good fit. You could add more refractory OM deposition during the glacial periods. Also I would like to see how the three fractions are distributed with depth - For me it is unclear why/how the selection of stations is made to fit the model. The stations are both inside and outside the DEA region.

Figure 10 - FeII2: Why not show the steady state? I think it would be better to use the same type of line for impact in all the plots.