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Interactive comment on "Scars in the Abyss: Reconstructing sequence, location and temporal change of the 78 plough tracks of the 1989 DISCOL deep sea disturbance experiment in the Peru Basin" *by* Florian Gausepohl et al.

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General comments

This manuscript provides legacy data from previous cruises and new data from a recent research cruise from the Disturbance and Recolonization Experiment area (DISCOL) in the Peru Basin, SE Pacific. In 1989 an area of about 11 km^2 was ploughed using a plough harrow to simulate Mn nodule mining in this area. The data used in the provided study include ship-based multibeam bathymetric data (MBES), video data

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from deep-towed instruments as well as MBES, side-scan sonar and video data from an Autonomous Underwater Vehicle (AUV). The authors digitized and geo-referenced the old data, matched different data types (bathymetric, side-scan sonar and optical data) with different resolution and investigated the disturbance intensity of this area including sediment suspension and re-settling. Major findings of this study are (1) old data with lower resolution and lower position accuracy can principally be used for comparison with modern high-resolution, high accuracy data provided a number of anchor points such as bathymetric features or sampling footprints are present, (2) there is an initial impact given through the mixing (ploughing) of the top 20 to 30 cm of the sediment and the related suspension of sediment into the bottom water (3) there is a secondary impact characterized by re-sedimentation of the initiated sediment plume and (4) the settling of the plume sediment is rapid in the immediate vicinity of the disturbance and causes high sedimentation rates which will be harmful to the benthic community which is not adapted to such high sedimentation rates.

Author's response: The listing of the major findings to your understanding here is very valuable since it shows that some of the key findings and intentions of the paper need to be better emphasized. Major findings of this study are: (1) to present a most accurately georeferenced acoustical and optical data set of various data layers from the DEA including the best possible location of the plough marks created in 1989. The intention was to create one accurate data set which other scientist can use, now and also in 20 years time. (2) In this respect, the study also presents the sequence of origin of single or groups of plough marks. This is essential information for detailed interpretation of observations and particularly sediment samples for geochemical analyses as (3) the grade of disturbance in terms of thickness of resettled sediment differs distinctly between earlier and later created plough marks. (4) An additional objective was to follow the development of one track over 26 years with respect to re-settlement of organisms and sediment cover. Only accurate geo-referencing enables such direct comparison. (5) Finally we wanted to show that the impact of the resettled sediment plume just after/while the disturbance is much higher than the sedimentation over the following 26 years. This is to point out that impact of a possible sediment plumes-inducing mining scenario is much higher than the low-sedimentation-rate-adapted deep sea environment is used to. We see the two major findings of point (2) and (3) listed of the reviewer more as fundamentals, which were named to define our terms used in this manuscript to distinguish between the two disturbance types. We hopefully managed to clarify and emphasize the key findings and objectives of the paper in the new version (see chapter 1.3).

Specific comments

Referee comment: The manuscript is well written and contains relevant references. Especially the methodology is well documented and convincing. However, I still have a number of issues the authors might take into account: The description of data processing, i.e., how to match the old and new data, covers the largest part of the manuscript, whereas the discussion of the results and their implication (especially point (4) above) is rather short. A more in-depth discussion of the results is needed. Moreover, there are a number of repetitions mainly in chapters 1.3, 2.4 and 4.2 so that the manuscript should be shortened by removing these repetitions. This is already obvious in the abstract which mainly contains methods for data processing but no results!

Author's response: The core content of this study is the presentation of the geo-referenced optical and acoustic data sets from the DISCOL area and the location of the plough marks created in 1989, since this information was lacking before that due to insufficient navigation accuracy back then. This is why the description of the data and the data processing take a large part of the manuscript. The presentation of the workflow and how data were acquired is supposed to be indicative for other studies dealing with the topic of environmental impact studies related to deep seafloor mining. It is shown that highly detailed information in respect of mapping and exact localization of sample positions is needed to facilitate a correct analysis of the data. We realized that the abstract was lacking in the presentation of the successful age sequencing of the plough tracks, which is also core content of the manuscript, since it leads to interpretations and results presented in sections 3.3, 4.3 and 4.4. Therefore in line 18 ff. this content was added. The results of the sediment blanketing studies are first applications to show the importance and the potential of this accurately geo-referenced data set for sample interpretations. Moreover in the mentioned chapters we do not see major repetitions. To our understanding the contents differ clearly in the different sections, summarized as followed: 1.3: Data acquisition during SO242; Motivation and content of study 2.4: Methodology of plough track identification and succession 4.2: Discussion of possible sources of error and evaluation of methods that best display the tracks themselves and best reflect the age succession (acoustic vs. Optical data) Nevertheless some repetitions within section 4.2 could be eliminated, since they are not essential to follow the text here and were mentioned before (in section 2.4), as the referee suggested: Line 509-514.: sentences have been deleted Lines 536 ff.: The plough tracks could be reconstructed with the highest amount of certainty for the very first and second set of disturbance tracks (PFEG 2 and PFEG 3). The uncertainties within the sequence regarding the absolute ages especially with later sets of tracks (PFEG 4-11) increase since they are mainly based on statistical information and logical method of elimination (see section 2.4).

Referee comment: The paper must critically review the fact that the DISCOL disturbance approach is very different to real nodule mining since no nodules were removed and sediments were only ploughed and not sucked into a device and subsequently dispersed a few meters above the seafloor as it would be done during real mining and as it has been done with the DSSRS. The manuscript does not say anything in this direction. Moreover, it should be discussed in this respect how the results of this study can be transferred to a real mining situation.

Author's response: This very important linkage to a possible real mining scenario was discussed in section 5 (Conclusions) Lines 655-616. It has been mentioned that this experimental setup is not unconditionally transferable to mining operations and that the amount of re-suspended material will be much higher, which in turn will influence the behavior of the sediment plume. This exactly links to the problem that all existing sediment plume behavior estimations are assumptions since no “real scenario” experimental setups were possible in the past but would be necessary for significant predictions in this respect.

Referee comment: During reading I wondered about the significance of the age sequence of the disturbance tracks and why the authors put so much effort into it. . . .It became clear to me in the lower part of the manuscript, i.e., to be able to differentiate between short-term settling of plume sediments with high sedimentation rates and natural sedimentation with low rates. Maybe it would be helpful if the authors present some clear objectives of their study within the introductory chapter.

Author’s response: We considered this valuable indication and inserted respective objectives within section 1.3.

Referee comment: As I already said above, this paper is mainly about the methods of data processing in order to compare old and new data with different quality. Some of these methodological approaches have been repeated a couple of times throughout the manuscript. I suggest that the authors should present a better separation of the method and the results of their study. In this respect they should provide a more in-depth interpretation of their results. For instance, they could discuss in detail the maps provided in figure 11.

Author’s response: We tried to eliminate some of the repetitions through the manuscript (e.g. see specific comments #1 above). In some cases we find that some things need to be mentioned again to keep on track with the explanations. Lines 581-584 were shifted to section 2.2, since we agree here with the referee that this rather should be mentioned in the methods part (section 2). To our understanding the maps in Figure 11 are sufficiently discussed in section 4.3. For more detailed discussion we think more complex calculations are needed including more influencing parameters and also more detailed sample analysis would be essential for a more detailed verification of the maps (discussed in section 4.3). As said in the manuscript, the disturbance intensity has been calculated based on assumptions and simplifications (section 2.5) to indicate trends and to give an idea about the distribution of the sediments that were re-suspended during the DISCOL experiment.

Referee comment: I also suggest that the author might provide suggestions how precise navigation during Mn nodule mining impact studies should be and how this navigation accuracy could be realized, e.g. through the installation of a transponder array on the seafloor within which all instruments used on or above the seafloor should navigate.

Author's response: As good as ever possible! Modern USBL and LBL systems linked with DVL and INS navigation on ROVs and AUVs can result in an absolute location accuracy of < 5m. This should be the aim, . . . and better if possible. We added this to the conclusions section in line 646.

Technical corrections

Referee comment: Apart from these comments, there are a number of special issues which I address below: Line 36: References Kuhn et al., 2011 and Oebius et al., 2001 are missing in the reference list.

Authors Response: The missing references have been added to the Reference list.

Referee comment: Line 59: Reference OMI; EC, 2013 is unclear and missing in the reference list.

Authors Response: EC,2013 is in the wrong place here and has been removed. OMI is the abbreviation for the named company Ocean Mining Inc.

Referee comment: Line 176: explain the abbreviation OFOS.

Authors Response: The abbreviation explanation has been added.

Referee comment: Line 208-209: What was the accuracy of the USBL system during the different cruises?

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Authors Response: 1989 - SO061 (Thiel and Schriever, 1989): USBL (RS904) in conjunction with GPS. The GPS position varied considerably with navigation errors >55m. The USBL system was used when no GPS coverage was available. The position was then manually calculated relatively to the pre-determined center of the DEA resulting in a mean estimated standard error of less than 100 meters. 1989 - SO064 (Schriever, 1990): Acoustic transponder navigation system (SONATRACK III) in conjunction with AMF ATNAV transponders. Four transponders were deployed to get a reliable position fix. After an initial calibration, a relative geodetic system was established for DEA with a standard error of approximately 18.5 meters within the array. However, additional measurements using GPS and the associated SONATRACK positions revealed an offset between the relative and absolute geodetic system by 151 meters in 345 degrees true north and all positions had to be corrected for this error. After good initial results, the SONATRACK system failed and could not be used for the major part of the cruise, so that most of the underwater positioning was again recorded using the RS 904 system. 1992 - SO077 (Schriever and Thiel, 1992): No information is given regarding the underwater positioning of the then used OFOS/EXPLOS system. 1996 - SO106 (Schriever et al., 1996): No information is given regarding the underwater positioning of the then used OFOS/EXPLOS system, but since it was basically the same system as in 1992, most likely a similar setup was also used to determine underwater positioning during this cruise. The recorded signals had to be edited and smoothed manually to minimize the impact of faulty signals.

Referee comment: Line 224: the reference Devey et al. is missing in the reference list.

Authors Response: The missing reference has been added.

Referee comment: Line 225: ....between 4300m and 3850 m...

Authors Response: This has been corrected.

Referee comment: Lines 220 – 234: Please provide some information about slope angles.

Authors Response: The reasonable requested information about slope angles within the working area was implemented in line 223, 226, 234.

Referee comment: Line 250 – 255 / Figure 3: How do the authors know that the NNW-SSE striking structures are ripple structures? To me they look like small grabens filled with sediment as well? On the east side of DEA these structures seem to be bent at their southern ends. Are these natural or artificial structures? Authors should discuss those obvious structures on the seafloor.

Authors Response: The origin of these features is not clarified and was descriptively named ripple structures because of their appearance. These 'ripples' follow the local morphology parallel to the broader slope and terminate in depressions. We downloaded Parasound data from Boetius and Roessler, 2015 from PAGAEA and analyzed one profile that more or less perpendicularly crossed these 'ripples'. There are no indications for fault structures down to ∼70m sediment depth that run parallel with the ripple-orientation (Figure 1); however the resolution of the ship-based Parasound data was not high enough to resolve the individual 'ripples' (opening angle=4°-4.5° => footprint of ∼286m). Figure 1: Section of a sub-bottom profile (below) crossing some of the channel structures within the DEA (above). Red arrows indicte the crossing of such a structure. Because of no detected indications of faults in the shallow sub-seafloor and the wave-like undulations, which are not typically observed with faults we postulate that the ripples are not caused by faults or tectonic activity but are derived from sedimentary processes. The orientation of the structures follows the predominant NNW current direction in this area. We assume that bottom currents are channelized through the local trough around the rising terrain towards the NE and may cause turbulent flows which eventually cause furrowing. The side scan sonar data show that the 'ripples' are channel structures filled with softer sediment. The process of sediment furrowing has been described for the deep ocean seafloor where strong dominantly unidirectional bottom currents from 5-20 cm –s occur (Flood, 1983), which is the case here (see this manuscript, section 2.5. We took the advice of the referee and included a short discussion about the structures to the respective section (section 2.2, lines 255 ff.). We also changed the misleading term “ripple” to furrow channels” or short channels (line 255).

Referee comment: Figure 4: The contours shown in Fig. 4 are based on the ship-based MBES? Why? Why the authors didn’t take the AUV-based MBES for the contours? If the latter is the case, please correct the figure caption.

Authors Response: The plotted contour lines are the ones originated from the ship-based MBES to illustrate the accordance of both MBES data sets after the geo-referencing and depth correction that was applied (lines 270-278). To clarify this we added a sentence for the explanation (lines 279-281).

Referee comment: Line 283 – 285: There is no information about the accuracy of the USBL sampling positions in sect. A2. . .

Authors Response: The link “A2” in line 285 was erroneous and has been corrected to “Appendix D”.

Referee comment: The way how USBL position was detected is described in Appendix D, instead. But no information about accuracy is provided. Since USBL was probably run in transponder mode, accuracy is normally around ±0.2% of slant range, in this case, water depth (4000 m), i.e., accuracy should be ±8m. Is this correct?

Authors Response: Exactly this is discussed in Appendix section D, lines 745-747.

Referee comment: Lines 286 – 294 (Fig. 5): What is the accuracy of the position of the sampling locations which act as anchor points?

Authors Response: The USBL accuracy of the sampling positions has been discussed in Section Appendix D. The sampling positions visible in the photo mosaics georeferenced based on the hydroacoustic layers (lines 271-285) show a mean difference of 14 m (line 495) in comparison with the USBL positions, which is considered as acceptable range of deviation in this water depth.

Referee comment: Line 349: Were current measurements being carried out during the DISCOL experiments in 1989? Or how do the authors know the overall long-term current speed and direction?

Authors Response: In 1989 long-term current measurements were carried out in the DISCOL area in February and in March, documented in Thiel&Schriever, 1989. The results are reported in lines 375-384.

Referee comment: Line 360-363: The capability of particles to flocculate is very important to consider (see also Guillard et al., 2019).

Authors Response: We considered and indicated flocculation of the plume particles within the assumptions made for the disturbance intensity calculation, as mentioned in lines 364 ff. in section 2.5. The mentioned literature of Guillard et al. (2019) presents latest results in the field of sediment plume behavior, which supports the statements in the manuscript and therefore has been cited in the respective section 4.3.

Referee comment: Line 400, formula 1: Provide reference for the application of this exponential function.

Authors Response: The exponential function used to generate the disturbance map of the DISCOL area was chosen to account for the size-dependent particles settling within the assumed distances, as stated in section 2.5. In line 398 ff. the function is shown and explained; it is a very simplified function but considering the numerous uncertainties which influence the sediment plume distribution we consider this approach as a sufficient way to indicate trends of sediment blanketing qualitatively. We are aware of that for the true assessment of the impact this equation is not accurate enough as it does not considers the specific sediment settling parameters as particle sizes, density of particles and water turbulence. A respective comment was added in the manuscript lines 573-574 in section 4.3. However, given the setup of the experiment and the observations from deep-sea photographs and videos we believe that the majority of the impact in this case occurred in close proximity to the disturbance tracks (see section

2.5); this has recently also been indicated by Guillard et al. (2019).

Referee comment: Line 470: Figure Caption of Fig. 11: Explain the abbreviations in the figure or give reference to where the reader can find this explanation.

Authors Response: The Figure caption refers to the literature where the different sectors were defined. The explanation of the indications “C” and “P” has been added as suggested.

Referee comment: Line 578: “. . ..to the disturbance map of this study.”

Authors Response: The suggestion has been implemented.

Referee comment: Line 610: There is no other N-S running track to the east of track V02 in Fig. 12A. But there are some other tracks running either E-W or ENE-WSW which were crossed by the OFOS stations during the different years.

Authors Response: Lines 614-615: Thank you for this indication. Indeed, there is no vertical track running east of V02. It should have been “west”. The sentence was corrected and clarified. Track V02 was chosen for analysis due to the low occurrence of vertical tracks in a larger radius. The close neighborhood with another vertical track west of V02, which has been crossed by all OFOS-surveys as well, allows an exact determination of V02 within the video footage. The more frequent occurrence of horizontally running tracks in consideration of the poor navigation accuracy of the OFOS tracks especially during the first cruises to the DEA impedes a definite identification of one track in different OFOS surveys. This explanation is implemented in the Figure caption of Figure 12 (lines 613-617).

Referee comment: Line 625-630: Is this conclusion supported by the disturbance intensity map presented in Fig. 11?

Authors Response: The disturbance intensity map indicates high disturbance in and in close vicinity of the cross section, since the track itself is set as highest disturbance (initial impact) and adjacent to the tracks the disturbance is also very high because of the resettled sediments ("secondary impact", see section 2.5). So within the cross section a difference between the two tracks is not visible within this map but it is very distinct within the images in Figure 13.

Referee comment: Lines 910 and 917: Reference Sharma & Nath, 1997 occurs twice.

Authors Response: The duplicate has been removed and the order according to the publication year has been adapted.

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Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-348/bg-2019-348-AC1-supplement.pdf

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