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Interactive comment on "Faunal carbon flows in the abyssal plain food web of the Peru Basin have not recovered during 26 years from an experimental sediment disturbance" *by* Tanja Stratmann et al.

Anonymous Referee #2

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General Comments:

This manuscript represents an important contribution to scientific knowledge of the possible impacts of seabed mining on deep-sea communities. The work conducted is original and timely, and the aims of the manuscript are clear and focussed. Analyses conducted are generally of a high standard, and the quality of figures and tables is satisfactory.

However, the manuscript could be strengthened by some additional simple analyses

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(detailed in the 'Specific Comments' section), as well as greater recognition of the limitations of thee data sets analysed.

In particular, I feel the naming of sites within the DISCOL experimental area that were not directly ploughed as 'undisturbed' misleading. Although not ploughed, such sites will still likely have experienced disturbance in the form of settlement of re-suspended sediments; a projected impact of deep-sea mining noted by the authors on page 2, line 26 of the manuscript. This is an issue which should be discussed in the manuscript but is not presently recognised.

Related to this, the authors describe the DISCOL experiment as a 'simulated small-scale deep-sea mining experimental disturbance' (Page 2 lines 7-8). However, no nodules were removed during the DISCOL experiment, and in this way, amongst others, DISCOL was not a perfect simulation of disturbance caused by deep-sea mining. How the results of this study may differ if nodules are removed from the sediment deserves discussion.

A glaring omission to the LIM analysed in this manuscript is the lack of microbial and meiofaunal data. It is explained in the manuscript that this is because of insufficient data (page 3, lines 10-11). This is understandable, but the impact this lack of microbial and meiofaunal data may have had on the analyses conducted deserves discussion in the 'Model limitations' section of the manuscript.

Another limitation of this study is the lack of baseline sampling – a 'Pre-Disturbance' time point. Such a time point would give a better indication of the 'undisturbed' ecosystem state against which all post-disturbance time points could be compared (especially PD0.1). Clearly it is not possible to obtain this data now, but this lack of baseline data requires discussion. At present it is only noted in the legend of figure 6.

Throughout the manuscript, the high values of community metrics obtained for the PD3 time point are noted frequently – highest biomass, highest faunal C ingestion, highest respiration, highest macrofaunal contribution, lowest faeces contribution to total C outflow etc. However, no attempt is made to explain this observation. Similarly, on multiple occasions, 'natural variability' is noted amongst observations. I find it surprising that no attempt has been made to identify the variable(s) that may be driving this variability, even qualitatively. If quantitative analyses to this effect are not possible, the manuscript would still benefit from greater discussion of this natural variability.

Whilst the use of Jumars' (1981) paper to structure the discussion of this manuscript is an excellent idea, I feel that the quality of this discussion could be improved. For example, on page 11, line 28 it is stated that "...Jumars' (1981) predictions for sub-surface deposit feeders could not be tested...". In the 'Specific Comments' section, I have given some suggestions of simple analyses which could be used to test Jumars' predictions more effectively.

Finally, whilst the quality of language used in this manuscript is satisfactory, many sentences, particularly in the discussion section could be re-written to improve the flow of the manuscript (e.g. the first sentence of section 4.1). There are also a number of grammatical errors, but I have not detailed these in full.

Specific comments:

Title

The title is rather long and could be improved to increase the impact of the manuscript. I suggest something like 'Abyssal plain faunal carbon flows remain depressed 26 years after a simulated deep-sea mining disturbance'.

Abstract

It is not immediately clear upon a first read-through of the abstract that LIM were produced for all points in the time-series, rather than just at PD26 (as the title may suggest). Please make this clearer.

Percentages – is it possible to give some sense of variability around these values?

At nearly 400 words, the abstract would be improved by more a concise wording.

Introduction

The description of the DISCOL experiment (page 3, paragraph 1) could be clearer. Perhaps a figure illustrating the areas ploughed/not ploughed would help. Also, I feel it would be better for only a short introduction to the DISCOL experiment to be included in the Introduction section, with a more detailed description being reserved for the 'Methods' section.

Page 3, paragraph 2: It is good to introduce the basics of LIM in the Introduction section. However, the current text is perhaps a little too technical for this section. I suggest moving the more technical aspects of this paragraph to a new paragraph in the Methods section.

I would like to see the aims of this study, and perhaps some hypotheses (e.g. based on Jumars (1981)), stated more explicitly at the end of this section.

Methods

Page 4, paragraph 1: It would be useful for the reader to know how many box cores were collected from 'disturbed' and 'undisturbed' sites at every time point, not just the PD26. This could be easily detailed in a table, which could also detail surface area of seabed surveyed for megafauna. Alternatively, the locations of all box cores collected over the 26-year study period could be plotted (colour-coded for date of collection) on the figure I proposed above illustrating the DISCOL experimental mining disturbance regime.

Page 4, line 8: Only three box cores were collected from disturbed sites for PD26. This is a very low level of replication, and something which should be discussed in the 'Model Limitations' section.

Page 4, paragraph 1: It seems strange that such an effort was made to analyse the same number of images for 'disturbed' and 'undisturbed' sites for megafauna, but there was no corresponding effort to analyse the same number of box cores for ‘disturbed’ and ‘undisturbed’ sites. Was an effort made to standardise megafaunal sampling effort for the other post-disturbance time points to that of PD26?

Page 4/5: Conversion of biomass into carbon content; I would like to know exactly what the conversions used were, if possible. These could be included in a supplementary materials file.

Page 5, line 20: No details are given of the conversion used for cnidarian/ poriferan biomass to carbon content. The authors should elaborate on the use of the Tilot (1992) paper.

The section detailing biomass to carbon content conversion would be made clearer by greater consistency in the use of the term ‘biomass’ to mean either the total weight of individuals, or the total carbon weight of individuals.

Page 6, lines 4-9: What literature was used to determine the coarse feedings guilds assigned to other taxa?

Page 6, lines 7: It is stated that “...a further detailed classification of the macrofaunal polychaetes...” was made. However, this further detailed classification seems only to additionally subdivide deposit feeding polychaetes into surface/subsurface categories. Could this division not be made with a little effort for all invertebrate macrofauna and megafauna?

Page 7 line 23 to page 8 line 9: Why was ‘Hedge’s d’ used here rather than t-tests or their non-parametric equivalent? This should be clarified.

Results

Page 8, lines 18-21: It is interesting that the minimum and maximum biomass values occur at same PD time steps for both ‘disturbed’ and ‘undisturbed’ treatments. That the lowest biomass for ‘undisturbed’ sites was observed at PD0.1 may suggest that these sites were actually disturbed to an extent. The fact that both disturbance treatments

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reach maximal biomass at the same PD time point may also suggest a shared recovery trajectory following disturbance.

Page 8, lines 21-23: This comparison of the change in % biomass difference between disturbance treatments from PD0.1 to PD3 is verging on discussion. I suggest moving it to the Discussion section.

Page 8, lines 24-26: Why is ‘absolute weighted Hedge’s d |d+|’ reported here when on page 8, lines 6-9, Hedge’s d is explained in a different form? This is confusing for the reader as page 8 lines 6-9 suggests that values greater than $\sim$0.8 represent strong effect sizes, but the results given on page 8 lines 24-26 report small values associated with the metric and the authors describe these as ‘indicating a strong experimental effect’.

Discussion

Page 9, line 28: Suggest changing “...compared to the undisturbed sediment after 26 years” to “...compared to the undisturbed sediment 26 years after experimental mining disturbance”.

Page 10, lines 1-23: I feel the authors are somewhat underselling the conclusions of their manuscript by placing a model limitations section so early on in their Discussion. This could be moved to later in the manuscript, perhaps to just before the conclusions.

Page 11, line 7: I am confused why the authors are discussing changes in fish respiration over long time periods (3 years) at undisturbed sites. The predictions of Jumars (1981) would be better tested by considering changes in respiration at disturbed sites very soon after the disturbance (e.g. PD0.1). I note that no fish were detected at disturbed sites at PD0.1, so simply put, this hypothesis cannot be tested with this data set.

Page 11, line 28: “Hence, Jumars (1981) predictions for sub-surface deposit feeders could not be tested...”. Indeed, it would be easier to test Jumars’ predictions if a PD0 time point was available. However, would it not be possible to test whether there is a significant difference in the density of subsurface deposit feeders at PD0.1 between the disturbance categories? Under Jumars' predictions, we would expect the density of sub-surface deposit feeders to be much reduced at 'disturbed' sites relative to 'undisturbed' sites at this time point.

Page 11/12, lines 30-6: The authors state Jumars' prediction that surface deposit feeders will be more drastically impacted by mining activities than sub-surface deposit feeders. However, they do not test this prediction, instead comparing deposit feeder ecosystem functioning to that of 'omnivores, filter- and suspension feeders and carnivores'. Please explain why. It would be possible to investigate the relative changes in surface and sub-surface deposit feeder contributions to ecosystem functioning between 'disturbed' and 'undisturbed' sites.

Page 12, lines 15-16: "After 26 years, the relative difference in the filter and suspension feeding respiration rate was still 80%". I assume that this refers to the difference in respiration rate of filter and suspension feeders between the disturbance categories? The current text is ambiguous and could be interpreted as the difference in respiration rate between filter and suspension feeders. It is also unclear whether an 80% difference means that respiration rates at 'disturbed' sites were 80% lower than at 'undisturbed', or that respiration rates at 'disturbed' sites were 80% of those at 'undisturbed'.

Page 12, lines 18-19: "...indicating a slow recovery of this feeding group". I'd argue that compared to Jumars' predictions this apparent recovery rate is relatively fast!

Page 12, lines 23-24: The authors complain here and elsewhere about natural variability in values making it difficult to isolate disturbance-related trends. However, the authors make no effort to identify or even simply discuss the key environmental factors which may be driving this variability.

Page 12, lines 20-28: This summary paragraph is unnecessary.

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Page 13, lines 5-6: “In contrast, filter and suspension feeders did not recover at all...”. This sentence is too strongly worded. The authors state on page 12, lines 13-15, that “Directly after the initial DISCOL disturbance event, the respiration rate of filter and suspension feeders at the disturbed sediment was only 1% of the respiration rate of this feeding type at the undisturbed sediment” and on page 12, lines 15-16, that “After 26 years, the relative difference in the filter and suspension feeding respiration rate was still 80%”. Whilst I agree that the respiration rate of filter- and suspension feeders is still clearly depressed at ‘disturbed’ sites relative to ‘undisturbed’, even 26 years post-disturbance, there clearly has been some recovery - perhaps even more so than might be expected! Please re-word this sentence to soften your conclusions.

Page 13, line 7: The authors state that “...[ecosystem functioning] has not recovered 26 years after the experimental disturbance”. However, there is clearly some evidence of recovery. Please could the authors change this statement to “...[ecosystem functioning] has not fully recovered 26 years after the experimental disturbance”?

Table 1

Please explain what ‘n’ stands for. Is this the number of taxa analysed, or the number of individuals used to estimate taxon-specific biomass etc.?

Figure 1

There is a lot of information on this figure, and the overlap in error bars make it especially difficult to read. One option would be to plot each group separately, although this would result in a large number of graphs. Alternatively, this information may be more clearly presented as a table (as per Table 2). Why are there no error bars for the PD26 bars?

Figure 4

Why are there no error bars on figure 4a? Are they simply too small to see?

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Abstract

Page 1, line 27: I, and most others, consider fish as megafauna. Please explain why they are treated separately to the other megafauna.

Introduction

Page 2, line 12: The word ‘occasionally’ is used twice in same sentence. Page 2, line 20: Yttrium is typically considered a rare earth element. Page 2, line 25: One could argue that there’s not really such a thing as food-rich surface sediments on the deep seafloor. Page 3, line 2: ’10.8 km2 large circular area’ – ‘large’ is not required.

Methods

Page 4, line 21: ‘could’ should be used, not ‘can’.

Discussion

Page 9, line 23: Suggest changing ‘evolution’ for ‘change over time’. Page 9, line 27: Put in comma after “...role of the various feeding types in the carbon cycling differs”, and after “...was significantly lower”. Page 11, line 28: “Hence, Jumars (1981) predictions...” should be ‘Hence, Jumars’ (1981) predictions...’. Page 11, line 34: “...deposit feeders seem to have advantages during the recovery from the DISCOL disturbance experiment...”. Relative to whom?

Figure 1

‘Figure 1’ is actually referred to in the text after ‘figure 2’ is. Swap around the order of these figures – i.e. ‘figure 2’ should be renamed ‘figure 1’, and vice versa.

Figure 2

Inconsistent spelling of faeces here and throughout the manuscript. Figure legend line 7 “...yellow-dashed arrow indicate...” should be ‘...yellow-dashed arrow indicates...’. Incorrect use of ‘due to’ here and throughout the manuscript. Please change to ‘because

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of' or 'as a result of'.

Figure 5

Is it possible to subscript the x-axis post-disturbance times – e.g. PD0.1, for consistency with the rest of the manuscript?

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