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Interactive comment on "Observations of deep-sea fishes and mobile scavengers from the abyssal DISCOL experimental mining area" *by* Jeffrey C. Drazen et al.

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My coauthors and I would like to thank the reviewer for their constructive comments and criticisms of our paper. They carefully read our paper and caught a number of things that our internal reviews missed, raise some good questions which we address and our manuscript is much improved as a result. Our responses to each of the points raised by the reviewer are given below. The line numbers in our responses below refer to the clean revised version. We hope that you will find our responses complete and that the paper is ready for publication.

Reviewer 1 Overall, I think this manuscript is very well-done and deserves to be pub-

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lished. The authors very carefully integrated previously-published and new data to give a comprehensive view of fish community recovery following experimental manganese nodule mining disturbance. The data are sound, and the manuscript is well-written. I have some minor and some medium comments, listed below.

Line 29: “relatively diverse” – in comparison to what? Is 16 taxa an average level of diversity for the abyssal Pacific? Line 29 - Changed this to “The abyssal fish community included 16 taxa and was dominated by Ipnops meadi.”

Throughout: Please be consistent with English spelling. There are instances of “plow” and “plough” in the manuscript, so the authors should choose one and stick with it. Corrected

Line 30: “Several years” is ambiguous, so please state the number of years. Now specified as “at 6 months and 3 years”

Line 31: The authors state that fish density increased because of changes in regional environmental conditions, but they did not measure any environmental parameters in the present study. I expect this was a finding of the oft-cited Bluhm (2001) paper, but either way, it leaves the reader expecting environmental data throughout the paper. I would just remove the phrase “due to changes in regional environmental conditions” from the abstract. This phrase now removed.

Line 31: This is incredibly nit-picky, but it is improper to begin a sentence with digits. Converted to text instead of digits.

Line 33: I’m not convinced that a lower abundance of the dominant species in disturbed areas by itself means there was only partial fish community recovery. My experience with disturbance-recovery studies is largely based on benthic invertebrates, which have very distinct community-level changes over time post-disturbance. There is early dominance by opportunistic species followed by an increase in species richness and finally a return to the pre-disturbance state. Is there any similar framework for fish? If so, I

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would love to see the authors cite previous fish disturbance-recovery studies to back up their claim of partial recovery. This could go in the discussion but be mentioned in the abstract as well. We respond to this comment below under line 310 – which reiterates this comment here.

Line 102: Please name the research vessel these cruises were on. We have now added that they were on the RV Sonne

Line 115: The OFOS has been updated several times throughout the last 10 years, so please add in parentheses the manufacturer of the camera and lasers used. It would be also helpful to know which flash was used. The camera is (iSiTEC, CANON EOS 5D Mark III) and the lasers (iSiTEC, custom built). These details are now added on lines 118-120.

Line 130: Please give a measure of the spread (i.e. standard deviation) of the photo area. The standard deviation and interquartile range are now given on line 133-134.

Line 141: This transect-elimination method is confusing. It sounds like you assigned each photo a habitat type, then grouped all the photos from a single transect by habitat type, and if there weren't enough photos that you were likely to see at least one fish, you eliminated the transect-habitat type group? This paragraph could use some revision to be clearer. The reviewer is correct in both their understanding of the paragraph and the recognition that the paragraph was poorly written! We have rewritten the paragraph and hopefully it makes much more sense now. We define "habitat transects" and our method for eliminating those that were too small to include statistically. Note this was also a comment for reviewer #2.

Line 162: Why were there so many variations in the baited camera specs? We were not a part of the older cruises during which the baited cameras were deployed. As we understand it, the variation in times of deployment were simply due to scheduling around many other cruise activities. They targeted about 700 images per deployment (limits to film and batteries) so the image interval varied based on deployment time.

The baiting protocol changed when they decided they wanted to capture some of the invertebrates (and it turns out a fish too!). Regardless of this variability the results are unique, the ISA is mandating scavenger studies in the CCZ and so these results are important to publish for general comparisons as we have made.

Line 163: Give a measure of spread (i.e. standard deviation) for the number of images per deployment. This has been added and also in Table 1

Line 165: Time of first arrival is said to have been measured, but it is not presented in the text of the Results. What information does this metric give about each of the scavenging taxa? We use the persistence values sparingly and now compare the time of first arrival values between the reference and disturbed areas (line 254-255, 299) finding that they are similar. Due to their sparing use we have moved them to a supplementary table.

Line 165 and throughout: The singular of “taxa” is “taxon.” Corrected throughout

Line 171: Why not use PERMANOVA to evaluate differences in community structure for the 1989 data as above for the 2015 data? PERMANOVA is more robust than ANOSIM. We were comparing community data and initially thought that we didn’t need the compare actual distances (PERMANOVA) but rather the rank order of differences (ANOSIM). Given the comments of both reviewers we have changed our statistical treatment to PERMANOVA. The result is unchanged. The test is described now on line 174-175 and the result has been amended on line 253.

Line 176: You should pick a consistent terminology for transect-habitat type groups. Define it in the paragraph at line 141 (along with a clear explanation of how some were eliminated), and use the term again here. This sentence has now been corrected to be consistent with the language in the methods (original line 141). We also made some changes to the paragraph starting on line 197 to maintain consistent and clear terminology as requested.

Line 203: The first two sentences of this paragraph belong in the discussion As written the reviewer is correct but this is also data that we analyze in this paper and want to include it in the results. Thus we have revised this paragraph so it reads clearly as results rather than discussion.

Line 212: A significant interaction means that change over time was only significant in one habitat type. This sentence should be revised. Line 213 - This statement is not exactly correct. It means that the changes over time differed between the three habitat types in figure 5. We revised the sentence.

Line 212: Some information is missing from this sentence – which habitat had lower fish density? Line 215 - The ploughed habitat. Revised to clearly specify this.

Line 218: Can Bluhm be contacted or original data accessed? 2001 was not that long ago. Unfortunately Bluhm has left ocean science. He has not uploaded the data which was used for the 2001 paper. As part of the JPIO Oceans project the raw seafloor images collected during the cruise have been digitized in 2016. These images will be made available via https://portal.geomar.de/ in the near future.

Line 292: “Neighborhood” is an ambiguous measure of scale. Please replace it with something more concrete. Range of attraction is baited camera studies is much debated hence our use of a general term. We don’t want to indicate a specific or concrete value here but we have removed the offending word and made the important point that attraction of fish could have come from as far away as the reference area.

Line 310: The fish community at large appears to have recovered from the disturbance because there is similar density and community composition of fishes, so the only finding for “partial recovery” is the low density of I. meadi over disturbed tracks. Again, I’m not convinced that this one result is sufficient to justify the conclusion of “partial recovery” of the fish community. If anything, I think it indicates “partial” or lack of recovery in the benthic infaunal community, which is I. meadi’s food source. The fish are there – they’re just hanging out where the food is. I think the authors should add

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additional justification for their conclusion of “partial recovery,” potentially framing their findings in the framework of other fish community disturbance-recovery studies. We have evaluated this comment in some detail because these are semantics that really matter. Our use of the term “partial recovery” is a bit ambiguous and has likely led to the comment. Ipnops meadi is the numerically dominant fish in the system and it has not recolonized the tracks. We agree that they are found very close nearby and we don’t think their population has declined regionally. It is our argument that their lack of occurrence in disturbed habitat is due to a lack of food. The dominant fishs’ distribution is affected. So the fish community is still affected by the DISCOL experiment 26 years later, even if population abundance may be regionally unaffected. The importance of this finding is that the DEA is very small and a mosaic of disturbed and undisturbed habitat. At an industrial scale, mining will affect 100’s km2 per year and potentially extirpate some of these fishes for decades even if they can move out of the way of mining vehicles. This will have population consequences due to the large spatial and temporal scale of effect. These consequences could extend beyond I. meadi. We have removed our term “partial recovery” from the abstract and discussion in relation to the fish community we measured. We also clarify the above point on lines 318-322 and 364-373, in the abstract and in the concluding paragraph. We evaluated comparing our results with other disturbances of fish communities. There is a literature on trawling but in this case the fishes are targeted and removed so the comparisons are not very easy to make without delving into the subject beyond the scope of this paper.

Line 382: The authors should cite one or more CCZ studies to back up the statements in the previous 3 sentences. These were cited in Table 3 which these sentences referred to. It is appropriate to also cite them in the text which we now do (line 397).

Section 4.3: What are the implications of the fish community overlap between the CCZ and DISCOL areas? Do the broad geographic ranges of fishes imply mined areas could be recolonized from elsewhere? Is it even known how far a given fish might swim to colonize a new area? The first paragraph of section 4.3 discussed why such com-

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parisons are important. Then we were purposely circumspect in this regard. Indeed, much of this section of the discussion talks about the fact that we can't speak that carefully about community overlap because often we are dealing with genera or higher taxa from photographic identification. But we understand the reviewer's point that readers will want some statement of conclusion. Thus, on line 403 we added the text "Rather it is likely that there are some species from this community, such as those that occur in both the DISCOL and CCZ regions, with broad distributions that could recolonize a mining license area if extirpated by mining. The extent of such conclusions must be made with caution because the overlap between the two areas may be artificially high."

Tables: consider adding a few horizontal lines to aid visualization of the data, for example between titles and data and between different table sections Lines added as per suggestion

Table 1: It would be helpful if blank cells were filled with zeros so the reader could better keep track of the rows Zeros added

Table 2: Persistence and time of first arrival data are not discussed at all in the manuscript. What is the importance of these metrics, and what information do they provide? As indicated above we use these variables sparingly and have now moved them to a supplementary data table.

It would be helpful to have a table of PERMANOVA results. We have included the PERMANOVA results for the two factor (habitat and time) evaluation of fish density in a supplementary table and cited this on line 213. The PERMANOVA test between reference and disturbed areas from the baited cameras was univariate and not significant so we have not included those results.

Fig. 1: Add an inset giving the location of the study site in the eastern Pacific. Figure 1 had now been updated as suggested.

Fig. 3: Use colors consistent with Fig. 1 for the different habitat types Colors now

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match

Fig. 3, 4, 5: Add tick marks on the outsides of axes for better visualization and consistency with Fig. 7 Done.

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