

Interactive comment on “Impact of the 2011 off the Pacific coast of Tohoku earthquake on a deep-sea benthic ecosystem: evidence from living and dead benthic foraminifera on the landward slope of the Japan Trench” by Akira Tsujimoto et al.

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Tsujimoto et al, have tried to understand changes in benthic foraminifera following a powerful earthquake, by studying temporal changes in foraminiferal assemblage, in sediments deposited pre- and post-earthquake. They have identified disturbed sediments by using radionuclides. Similar studies have been carried out from the same region. The only novelty in this work is that the authors have taken a few cores from deeper region. I've several reservations regarding the methodology as well as the down-core variations in parameters, across the cores.

Response: We would like to thank anonymous referee 2 for critical and constructive comments. Reviewer's comments were really helpful to improve our manuscript. Detailed responses to the review comments are below. All technical comments on supplementary file were corrected or incorporated. Added or improved sentences are written in red-letters in the manuscript.

1. The title of the manuscript needs to be rephrased. Please see annotated manuscript for details.

Response: We changed title based on the suggestions of referees 1 and 2 as "Impact of the 2011 Tohoku-oki Earthquake on the deep-sea benthos: Evidence from foraminifera of the Japan Trench slope".

2. The first sentence of the introduction should be reworded.

Response: We changed the first sentence of the introduction.

3. Page 2, Line 11, please specify which fauna? The reference is also missing.

Response: We added the species name (*Psammosphaera* spp.) and reference (Toyofuku et al., 2014).

4. Page 2, Line 21, replace 'Their' with 'Foraminiferal'.

Response: We corrected.

5. Can the authors explain the logic behind sampling during two consecutive years (2011 and 2012)?

Response: We added short description on sampling strategy in line 4 to 6 of page 3 and also limitations on consistent samplings between 2011 and 2012 in line 7 to 11 of page 3.

6. Page 3, Line 14, the authors state that samples were stored at -80C for foraminiferal studies. Such a cold temperature will results in breakage of foraminiferal tests due to

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thawing. The breakage will thus significantly alter benthic foraminiferal assemblage.

Response: In case biological samples, it is preferable to freeze at lower temperature such as -80dC or even lower, because freezing at -20dC or -30dC can cause cell destruction (e.g. McHatton et al. 1996, Appl. Environ. Microbiol. 62:954–958.) due to water crystal formation. We thus think that -80dC preservation may reduce these physical destructions even for foraminiferal test. However, we agree that freezing and staining with rose-Bengal afterward is not common protocol in foraminiferal study and may have biased our results. We noted potential effects of freezing in the discussion.

7. The authors followed a strange methodology for foraminiferal analysis. To preserve and identify living benthic foraminifera, the sediments should be stained with rose-Bengal, immediately after collection. Unfortunately, the authors stored the sediments at -80C, oven dried it, sieved by using a 63 um sieve and THEN STAINED THE RESIDUE. I think, it is absolutely wrong. The living assemblage will be significantly underrepresented. 8. Staining the samples for just one day is insufficient. A minimum of couple of weeks of staining is widely recommended.

Response: In this study, we followed the traditional procedure for rose-Bengal staining method by Takayanagi (1978, Manual of Microfossil Studies) and subsequent studies mainly by Japanese researchers (e.g., Tsujimoto et al., 2006; Nomura and Kawano, 2011). Also, we did NOT oven-dry the sediment before staining; we revised the text to make it clear. We understand the importance of the standard staining method as the reviewer pointed out, especially for the benthic foraminiferal monitoring studies (e.g., Schönfeld et al., 2012). However, although the living assemblage may be underrepresented, we mainly discuss temporal faunal changes based on total (i.e. living and dead) foraminifera. Furthermore, in the revised manuscript, we discuss more based on total fauna, not by “living” fauna which may represent underestimated numbers. We noted these methodological limitations in the discussion. Although these limitations on methodologies to study on “living” fauna, , temporal trend in total foraminiferal assemblage indicates the impact of large-scale seafloor disturbance on deep-sea benthic

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ecosystem together with concurrent sedimentological analyses, thus we believe these results are worth to be published in this journal.

9. The authors chose a odd mesh size (106 μm) to pick benthic foraminifera. The recommended mesh size for benthic foraminiferal study is 63 μm . A few authors have also used >125 μm fraction. The choice of these authors does not match with either of the widely used mesh size, thus making it difficult to compare their results with other studies.

Response: This size fraction follows our conventional procedure by Nomura (1995), Tsujimoto et al. (2013), and Takata et al. (2015), which studied deep sea benthic foraminifera. We added these references in the text. We aware that most ecological studies used either 63 μm or 125 μm sieves, and the differences in sieve size in our studies hampers direct comparison with such ecological or environmental monitoring studies. We noted these methodological limitations in our revised manuscript. However, even though there are such limitations when comparing with other studies, we believe that the temporal and spatial trends found in our study based on consistent taxonomical works are worth to publish with such notes.

10. Authors picked 200 or less foraminifera from each sample. Again, the recommended minimum number of specimens to be picked is 300. Therefore, I'm not sure whether the foraminiferal assemblage studied by the authors is a true representative of the natural assemblage.

Response: We had picked 300 specimens in some samples, but some samples contained individual numbers less than 200 individuals even if we analyses entire sediment samples (~ 12 ml). Full faunal list was available in the supplementary Table S1, however, for better understandings for readers, we added number of specimens and number of species in Table S1.

11. It is not clear, how did the authors calculate foraminiferal density? Did you pick foraminifera from a known weight of sieved fraction?

Response: We measured the wet weights of sliced subsamples before wet-sieving through a 63- μm sieve. We calculated the dried weights of sliced subsamples from the water content obtained from the process of water content analysis (see section 2.2). The foraminiferal density (i.e. number of foraminifera per gram of dry sediment) was calculated from the dried weights of sliced subsamples. We added this explanation in Method.

12. Page 4, Line 4, authors state that they used only those samples that contained >30 individual, for statistical analysis. Does this mean that several samples contained as less as <30 individuals? It is too small a number to draw any meaningful inference from foraminiferal parameters. Please provide the number of specimens picked from each sample.

Response: We added the number of specimens of each sample in supplementary Table S1. We performed statistical analysis only for total assemblage of core 4W-2012 because the others did not contain sufficient specimens. We recalculated Q-mode cluster analysis and diversity index based on the samples containing more than 50 individuals. We also determined the rarefaction diversity E (S100) (= the expected number of species in samples rarefied to 100 individuals) in addition to the Shannon Index (H) for total assemblage of core 4W-2012. The results from additional analyses showed no obvious change from the previous analyses.

13. Three out of the four cores have a nearly same mud profile, suggesting no evidence of disturbance.

Response: As the reviewer suggested, evidence of disturbance is not obvious from mud content of core 2W-2011 and 2W-2012, but detection of ^{134}Cs in the top 1.5 cm of the cores indicates that this interval was deposited after the FNPP1 accident. Moreover, relative low concentrations of ^{210}Pb in the top 1.5 cm of the cores 2W may indicate that deposition of older sediments derived from seafloor disturbance. The rapid decrease of water content and the very low ^{210}Pb concentrations in core 4W-

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2011 suggest that the surface sediments at this site were eroded prior to the sampling.

14. Can the authors provide the details of how far were the cores 4W-2011 and 4W-2012? Both these cores are very close and at nearly same depth (just a difference of <20 m). Why was the ^{210}Pb profile, so drastically different in so closely spaced cores?

Response: We added bathymetric profile at site 4W as supplementary figure (Figure S3). Although cores 4W-2011 and 4W-2012 were collected in the same area, which are ~ 550 m away each other, slight locational changes in the canyon should result in different sedimentation environments. Our new supplementary figure S3 showing the bathymetric profile along the two sites clearly shows that 4W-2011 was collected from a steeper slope ($\sim 4^\circ$) while 4W-2012 was collected from a gentle slope ($\sim 1^\circ$) of the same canyon (Fig. S4). Sedimentation rates can vary greatly even at local scale, in particular at such topographic and hydrographic heterogeneity environments. A flow such as a turbidity current was accelerated by an increase in slope gradient and eroded the surface sediment near 4W-2011. The flow was decelerated by a decrease in the slope gradient and deposited the new turbidite near 4W-2012. As the consequence, the lithology and profiles of radionuclides between the two cores are quite different. We added these description in the text.

15. The water depth of both the cores is nearly same, just 20 m difference. I do not agree with the authors that the slope is so different at these two locations, so as to bring such a big difference in geochemical parameters as well sedimentation rate.

Response: As mentioned above (reply to comment 14), bottom topography differs largely between 2011 and 2012 sites, thus the geochemical parameters and sedimentation rates. Indeed, we originally thought these two cores are collected from comparable environments to track foraminiferal faunal change between 2011 and 2012, because these sites are almost same water depths and located at same canyon axis. However, detailed bathymetric survey and sedimentological analyses revealed that such local topographic change can cause great differences in sedimentation environ-

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ments. We believe this is another important result from our study.

16. Page 7, Line 19. Authors report living benthic foraminifera at much deeper depths and provide a strange explanation? Why this argument is not applicable for the other stained benthic foraminifera? How does the authors rule out the possibility that it is a autochthonous living benthic foraminifera?

Response: As the reviewer pointed out, due to the limitation on staining methodologies different from standard protocols, we deleted these speculations from the text.

17. Page 7, Line 36-37. The authors speculate the effect of productivity on benthic foraminifera. Please confirm it with the Corg in the sediments. In its present form, it is just a conjecture.

Response: We added the TOC value of the 4W-2012 as supplementary table (Table S2), which represents very high TOC concentrations (~4%) down to 10 cm depth. This TOC concentrations are very high given the water depth is 3600m, strongly support the rapid sedimentation of fresh organic matters. We added these data and descriptions in the text.

Comments in supplement file

Page 1, Line 25. Not clear, please rephrase.

Response: We rephrased as “After the disappearance of many benthic foraminiferal species”.

Page 1, Line 29. Please specify, what you mean by faunal change.

Response: We specified as “the opportunistic species”.

Page 3, Line 2. Not clear.... did you collect a couple of cores at each station? If so, why?

Response: We collected single core from at each site, from each sampling year, mak-

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ing four cores in total (2 sampling periods at 2 sites). We made these explanation clearer.

Page 2, Line 11; Page 3, Line 2; Page 3, Line 39 - Page 4, Line 5; Page 4, Line 24 - Line 28; Page 5, Line 36 - Line 38; Page 7, Line 19; Page 7, Line 35 – Line 37

Please see above mentioned responses.

Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2018-237/bg-2018-237-AC2-supplement.zip>

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-237>, 2018.

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