

To,
Prof. Federico Rossetti,
Editor, Solid earth

Sub: Resubmission of Revised Manuscript (SE-2022-11)

Dear Prof. Rossetti,

Thank you for the review reports on our manuscript entitled “**Mechanical compaction mechanisms in the input sediments of the Sumatra Subduction Complex- insights from microstructural analysis of cores from IODP Expedition- 362**” written by Lahiri, S.; Milliken, K. L.; Vrolijk, P., Desbois, G.; and Urai, J. L. “Minor Revision” was recommended based on the review report. **Reviewer#1** (Dr. Dave Dewhurst) stated regarding the manuscript that, ‘this manuscript is vastly improved and pretty much ready to go with just the clay mineralogy clarification to go along with some minor cosmetic tidying up. Really good work and great images’. **Reviewer#2** (Dr. Bernhard Schuck) commented that “the manuscript is well-written and address a topic relevant to solid earth”. However, the reviewers raised a few questions regarding clay mineralogy, bulk rock mineralogy and also suggested some minor changes.

We appreciate the constructive review by the reviewers, and in the revised manuscript we have addressed all the issues raised by them. *In the revised manuscript, all the changes based on suggestions of the reviewers are highlighted in green color (reviewer#1, Dave Dewhurst) and yellow color (reviewer#2, Bernhard Schuck).*

I hope that the revised version will be accepted for publication in the *Solid Earth*.

Thanking you,

Yours sincerely

Sivaji Lahiri

(For the authors)

Comments by Dave Dewhurst

Comment 1: My main concern with the manuscript last time was that the description of the clay mineralogy of the sediments was somewhat confusing and this unfortunately is still the case. As one of the senior authors (Milliken) is currently visiting my office in CSIRO in Perth, I have discussed this issue with her, and it seems that this can be clarified. I will still write my comment here however so that the authors can address it in the final version.

The text from lines 131-144 states initially that smectite, illite, kaolinite and chlorite are present in the samples. However, it then goes on to discuss mixed layer illite-smectite, which doesn't appear in table 1 at all. I know that the XRD data is not the crux of this manuscript and comes from the Rosenberger paper, so I went and checked that too, but it isn't completely clear in that paper either. So, what I am left wondering is whether the "smectite" quoted in table 1 is pure smectite or whether it's mixed layer illite-smectite (or both)?

From the Rosenberger paper looking at the diffractograms presented there and how things are worded, I would guess that the "smectite" quoted is all mixed layer illite-smectite and that there isn't any pure smectite in these sediments. Or if that is incorrect, is all the I-S hiding under the "illite" number given in table 1, or is that just illite and all the various micas? I have seen a few shales which do have pure smectite, mixed layer I-S and illite and these are routinely specified as separate phases when they all occur together in the XRD work we do here. Hence, I have ended up a bit confused. I gather that the XRD was done in two batches, one shipboard for bulk and one onshore for detailed clay mineralogy. Perhaps a bit of clarity around the different techniques used and different timings would be helpful so the reader can clear understand what has been done and how the quantitative determinations were made.

The final thing I am a bit confused on (I'm having one of those days I think!! 😊), is on lines 137-139, where it is stated that "There is a weak increase in the expandability of I-S with depth" but "no change in the amount of illite in mixed layer I-S". Presumably the increase in expandability with depth results from increasing smectite in I-S with depth (which I gather is from increasing volcanogenic components deeper in the sequence, might be useful to note in the discussion maybe?), so if smectite is increasing, shouldn't illite be decreasing? Sorry if I am missing something obvious.

Response: Further explanation of the data reported in Rosenberger et al. is added in Lines 136-147. Basically, the amount of illite/smectite in samples, which are smectite-rich (highly expandable), are first defined in terms of the smectite used in the reference samples for quantification. A separate analytical step on glycol-saturated samples was performed to determine the amount of illite in mixed-layer I/S (defined in terms of Expandability). As now stated, the accumulation of clay mineral data and how they are combined into a comprehensive understanding should now be clear (line number 132 to 156 in the new version of the manuscript).

Comment 2: Lines 64-66 and 70-74, the previous investigations quoted have been divided up into two groups, "lab experiments" and "natural samples". However, some of the lab experiment references quoted were actually performed on natural samples, which makes things a bit unclear. Perhaps this could be better phrased as (1) experimental compaction in the laboratory and (2) in situ compaction with depth, or some such. Need to rephrase at lines 70-74 as not all the lab tests were done on artificial clay packs.

Response: *DONE*. Line number 65 to 73 in the new version of the manuscript.

Comment 3: L117-118, Units I to VI are not all visible in Figure 1c as the figure seems to be chopped off at the bottom only getting to unit IV.

Response: *DONE*. We have modified Fig.1c.

Comment 4: L230 and other places through the manuscript, the magnification is written out in two different ways, in places for example with x 10,000 while in other spots 10kx. I would stick with writing things out at x 10,000 for consistency. I have noted a few spots in the marked-up version that should be made consistent.

Response: *DONE*. Line number 244, 245 to 284 in the new version of the manuscript.

Comment 5: L285, the authors are discussing figure 3b and one observation that should be noted is that although there is a correlation between the two porosity measurements, there is a large difference in the absolute values and that this difference increases with increasing porosity. This observation becomes useful to support interpretations later in the discussion. I have written this comment at L215 on the marked manuscript but decided it would be a better fit at L285.

Response: DONE. We have added the line. Line number 302 to 304 in the new version of the manuscript.

Comment 6: L320-325, I think the figure numbers are incorrect in this paragraph.

Response: DONE. Corrected. Line numbers 341-345 in the new version of the manuscript.

Comment 7: L337, this supplementary data figure number also appears to be incorrect.

Response: DONE. Line number 359 in the new version of the manuscript.

Comment 8: L430-432, in regard to the previous studies noting mismatches between MAD and BIB-SEM, what did they actually conclude were the causes?

Response: DONE. Line numbers 460-463 in the new version of the manuscript.

Comment 9: L442, the correlation coefficients quoted don't seem to match what is on the figure.

Response: DONE. Line number 473 in the new version of the manuscript.

Comment 10: L469, A comment could be added here stemming from point 5 above that the results are also consistent with the difference between the porosity measurements getting larger as porosity increases.

Response: DONE. Line number 501-502 in the new version of the manuscript.

Comment 11: There are a few typos and grammatical suggestions which are on the accompanying marked manuscript. For the main text, the references are correct with one exception at the bottom of page 3 (Hippchen and Hyndman is not in the list). However, there are a number of references in the supplementary text which are not in the list and these are on the marked manuscript.

Response: DONE.

Comment 12: On figure 3a, the 1:1 line is not in the correct place. Both porosity axes also need the units displayed.

Response: DONE.

Comment 13: Figures 12a and 12b also need the porosity units on those axes. What do the different coloured symbols represent in Figure 12a?

Response: DONE. Green, orange and blue colored symbols represent samples from Unit-I, II and III respectively. To clarify this issue, we have described the color codes in the Fig.12a and also mentioned in caption.

Comment 14: Perhaps the legend to table 1 could note what Pc/pc+g is as a column title. Perhaps also add in that bulk measurements were shipboard, and that the clay mineralogy was done on the < 2 micron fraction onshore on a different instrument.

Response: DONE. A small description is added in the caption of the table.

Comment 15: In Figure S1, the very ends of the axes need reversing for kaolinite/chlorite and illite content, otherwise they are reading as higher than total clay content.

Response: DONE.

Comment 16: The supplementary written documents have a lot of typos and some referencing issues. These are on the marked manuscript.

Response: DONE.

Comments by Bernhard Schuck

Comment 1: Referring to “comment 2” and “comment 6” in my review of the initial manuscript: I highly appreciate that the revised manuscripts is substantially more specific about the samples’ mineralogy and the methods used to analyse it. However, while lines 273/274 specify that EDX analyses revealed the presence of K- and Na-feldspar, results of XRD analyses presented in table 1 only give the presence of plagioclase. This difference should be at least explicitly mentioned or discussed.

Response: DONE. Line numbers 291 to 294 in new manuscript.

Comment 2: The reference McNeil et al. (2017; <https://doi.org/10.14379/iodp.proc.362.102.2017>) actually only refers to the methodology applied to study the bulk rock composition. Page 10 of this methods report does not only provide values to assess the errors in XRD analysis but also states that “the method [of XRD] described is semiquantitative and results should be interpreted with caution”. The authors should provide these information on errors and limitations of the performed XRD analyses in the manuscript.

Response: DONE. Line numbers 156-158 in the new version of the manuscript.

Comment 3: Furthermore, the supplementary material providing bulk rock compositions has a doi (<https://doi.org/10.14379/iodp.proc.362supp.2017>) different from the one indicated in the manuscript. Therefore, I suggest to use McNeil et al., 2017a and b as references.

Response: DONE.

Comment 4: Using two digits to present “quantitative” bulk rock compositions suggests an accuracy of the results which is not justified – especially considered that the reference (McNeil et al., 2017b) only uses one digit to present bulk rock composition. For the same reasons I recommend to not provide any digits at all to present the bulk rock composition in table 1.

Response: DONE.

Comment 5: Referring to “comment 4” in my review of the initial manuscript: As far as I remember the Zeiss Supra series uses a field emission gun. There I suggest to modify lines 157 – 161 as follows to clarify the infrastructure used for microscopic analyse:

“Samples have been prepared using Ar-ion cross-section polishing and analysed using a scanning electron microscope equipped with a field emission gun (BIB-SE technique). “The first sample set (33 mud samples; depth 1.24 to 1300 mbsf) was prepared and analysed at RWTH Aachen University, Germany. The second sample set (22 samples; depths 6.25 to 1493.30 mbsf) was prepared and studied at the Bureau of Economic Geology (BEG) at the University of Texas at Austin.”

Response: DONE. Line number 171-175 in the revised manuscript.

Please note that the fact that the Zeiss is a FE-SEM implies that minor corrections to lines 327/328.

Response: DONE. Line number 350 in the revised manuscript.

Comment 6: Referring to “comment 10” in my review of the initial manuscript: Please add the following sentence after the first sentence in line 305:

“From this section four samples have been analysed in Aachen as well as in Austin, respectively.”

Response: DONE. Line 324-326 in new version of the manuscript.

Comment 7: Referring to “comment 11” in my review of the initial manuscript and to “comment 1” of this review: please modify the reference “McNeil et al. 2017” to “McNeil et al. 2017b (<https://doi.org/10.14379/iodp.proc.362supp.2017>) to make it easier for readers to also obtain the data used.

Response: DONE.

Comment 8: Referring to “comment 12” in my review of the initial manuscript: I still do not see how Figures 2a & b (and Table 1) support the statement that “Shallow samples from Unit-I are richer in smectite content than the deeper samples” (line 466). Both, the figures and tables show that the indeed smectite content decreases below Unit I. However, below approx. 400 mbsl smectite content increases / is elevated compared to the section between 28 and approx. 400 mbsl (see also lines 136/137 and 631/632). Therefore, I suggest to modify the statement in line 466 and discuss this observation.

Response: DONE. line 497-498 in the new version of the manuscript.

While doing so, I strongly recommend to also taking Figures 2e & f into account, which might indicate increasing porosities towards the bottom of the drillhole. Given one of the major outcomes of the study presented (cf. lines 20/21) this observation should be discussed.

Response: DONE. line 373-378 in the new version of the manuscript.

Comment 9: Line 580: Please rephrase the sentence.

Response: DONE. line 613-615 in the new version of the manuscript.

Comment 10:

Figure 1:

- The line indicating the location of the seismic profile (b) on the satellite image (a) is yellow, not red. Please correct.

- Please provide the orientation of the seismic profile (i.e. “SW” and “NE”, respectively)

Response: DONE.

Figure 12g:

Still the “increase in preferred alignment of the long axes of pores” cannot be seen on the conceptual sketch, i.e. the pores’ long axes appear to already have a preferred alignment in stage one, which does not change in succeeding stages. The figure should be modified to emphasize the proposed increase in preferred alignment more clearly.

Response: DONE. We have redrawn the Figure.12g. (Fig.14g in new manuscript).