

To,

Prof. Virginia Toy,

Editor, Solid earth

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

Dear Prof. Toy,

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. “Major Revision” was recommended based on the review report. Reviewer#1 (Dr. Dave Dewhurst) stated regarding the manuscript that, ‘this is a really an interesting manuscript with some fabulous SEM images of compacting clays and great insights into processes occurring during early mechanical compaction’. Reviewer#2 (Dr. Bernhard Schuck) commented that “the manuscript is well-written and address a topic relevant to solid earth”. However, the reviewers also raised several questions regarding clay mineralogy, bulk rock mineralogy, drying artefacts etc.

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)

Reviewer 1# (Dr. Dave Dewhurst)

Comment 1: My main concern with this manuscript is that the description of the clay mineralogy of the sediments is somewhat confusing and this requires significant clarification by the authors. The paragraph on page 4, line 113-121 notes a clay mineral assemblage of “illite with minor smectite and chlorite”, noting 33% smectite in unit 1 and 73% in unit III (Table 1), numbers which are not really minor (and are averages really useful in such thick units?). The illite percentages are then given followed by a discussion of illite-smectite (I-S) expandability. However, I-S is not documented at all in table 1, just presumably pure illite and smectite. How much mixed-layer clay is present, and is it being properly differentiated from the pure end members? What is the I-S composition? This is actually important for the whole manuscript since porosity corrections are required for smectite-rich sediments and this may impact significantly on results, especially as there seems to be 15-20% “missing porosity” as noted in the discussion between MAD and SEM porosity (although there are other possibilities for this). This lack of clarity over clay mineralogy also comes back in the discussion (p12, L385-388), where the numbers quoted in the text are not consistent with table 1. For me, I feel it would be really informative to have the clay compositions plotted up against depth and added to figure 2 for comparison. I’m sure IODP have the data and although the measurements wouldn’t be from exactly the same points as measured here, they would still provide clear trend information.

Response: We agree about the need for greater clarity on clay mineralogy. In the revised manuscript, we have modified Fig.2 and Table-1. In Fig.2, we added Smectite content vs depth, and Illite content vs depth plots for both drill holes (U1480 and U1481) separately. In Table-1, we added separate columns to depict bulk clay mineral composition corresponding to the analysed samples. In addition, we also added two new supplementary data (Figure S1 and S2 in Supplementary data) to emphasize on this aspect. In Figure S1, we represented depth wise variation of different component of clay for U1480 and U1481 drill holes. Subsequently, in Figure S2, we represented the variation of I/S expandability with depth. Nevertheless, to clarify this particular issue we have also modified the section ‘Geological Background and drilling’ and provided mineralogical description in details (Line number: 131 to 144). I hope, these modifications resolve this issue.

Comment 2: P2, L52-54, depositional environment is also a strong control on porosity evolution, compaction and diagenesis in mudrocks (e.g. Baruch et al., 2015, AAPG; Delle Piane et al 2015 MPG) as the initial clay and rigid grain compositions significantly affect both compaction (as this manuscript shows) and subsequent diagenetic alteration due to variations in composition.

Response: A line is added in the main text (Line number: 56 to 59).

Comment 3: Somewhere in the geological background and drilling section, there should probably be a mention of whether these sediments have been uplifted at all or are currently at maximum burial depth. I would assume the latter just from how things have been written but probably best to have a clear statement.

Response: A line is added in the text to clarify this issue (Line number: 147 to 148).

Comment 4: L137-147, somewhere in this paragraph there needs to be a description of how the samples were freeze dried as this is absolutely critical in very soft clay bearing sediments when trying to get quantitative data from pore characterisation techniques, whether that be via SEM or gas/mercury porosimetry. Need to know things like the method employed, temperatures achieved, how long they were frozen for before drying, how long the drying took etc etc. This goes to understanding the quality of results and I’m a little surprised there is no discussion of this. There was work done on this going back to the early 1980s looking at fabrics and microstructure of soft clays and the impact drying can have on porosity measurements and particle preferred orientation. There should be some consideration of this work and later work for Example in the Microstructure of Fine-Grained Sediments compilation quoted in the Bennett et al reference in the manuscript. Given the “missing porosity” between MAD and SEM quoted later in the paper, could the drying methods account for that? This is a really important topic where the authors need to convince the reader that the data quality is good. A few of the early references may prove helpful and these (plus younger ones that could be found through citations) could help formulate the discussion further. I’ll come back to this point below.

Response: For Aachen sample sets, we have added a section in the main text describing the condition maintained for drying the samples at IODP repository, Japan (line number: 165 to 168). For BEG sample set, the drying condition and after drying effect is described at line number: 220 to 222.

We agree that drying artefacts can significantly affect the MAD porosity vs BIB-SEM porosity plot. We are grateful to the reviewer for identifying this problem in our old version of the manuscript. In the revised version, we have added a section in the discussion describing this aspect to clarify this issue (Line number: 453 to 469).

Comment 5: P5, L169, The PPR and REA section needs some discussions around the assumptions, errors and limitations of determining porosity and pore sizes from 2D images. Pores are non-spherical 3D objects that are cut through in random orientations and at random points in a 2D thin section, so measurements taken on the images have associated assumptions and errors.

Response: *This is clarified in the main text (Line number: 206 to 215).*

Comment 6: P6, L195, introduced the shipboard MAD measurements in the results section. Again, even though done elsewhere, a brief description of the methodology is warranted in the section above as to how this was performed and then point the reader to the appropriate reference for further details. These data are absolutely critical to the manuscript, so the details are sorely needed. In addition, Figure 2 shows some data in yellow points as measured at Aachen and BEG; were these done by the same method as shipboard or different? Again, a description is warranted.

Response: *DONE. A brief description of the methodology employed for generating MAD data set is added in the main text (235 to 238).*

Blue points are describing Shipboard MAD (Moisture and density) porosity vs depth data for mudstone samples recovered from Sites U1480 and U1481. Yellow points are describing analysed samples at Aachen and BEG. To bring clarity, we modified caption of Fig.2 (967 to 969).

Comment 7: L238-239, the correlation coefficient between the two porosity measurements should be shown either in the text or on Figure 3b.

Response: *DONE. (Fig.3b and line number: 285 in the main text).*

Comment 8: P11, L341, in these plots it should be stated which points are used to calculate the D values. Is it all points or just the points that avoid the artificial tail offs from sampling bias/resolution issues at either end of the lines? Probably should be the latter but some of the trend lines do not lie through the straight-line section of the points.

Response: *Yes, we have considered the points that avoid artificial tail off due to resolution issues. In the diagram we plotted the line corresponding to the average 'D' value (i.e. the average of highest and lowest estimated 'D' values). However, we have again rechecked each plot and corresponding D value for each sample.*

Comment 9: For me, the whole discussion is a bit weak and lacking in detail. There are some interesting points made and comparisons, but they are not really built on very much and tend to be almost standalone points based on a couple of sentences. There also seems to be potential background discussion items that probably should be in there but are currently not referred to. For example, the soil mechanics community has done a lot of work around natural clays and remoulded clays (similar to the Mondol/Fawad type work) but this is not even discussed although thoroughly relevant to the behaviour noted here (e.g. classic papers by Burland, 1990, Geotechnique; I think also Lupini et al 1991, Geotechnique, which is currently quoted in the reference section but isn't in the text). The references I note above in point 4, although old, also discuss topics alluded to here in terms of pore collapse with compaction, the very topic of the paper. Perhaps a bit more background in the introductory sections would provide a good lead into the discussion of the results obtained. Supplement 15 should also be in the discussion as it provides some details that are sorely missing at the moment.

Response: *We are thankful to the reviewer for suggesting this. In the revised version of the manuscript, we have modified and improved a section in the introduction (Line number: 75 to 93) to discuss previous studies, and also subsequently improved discussion section (line number: 531 to 548). In addition, we have also reviewed recent literatures on mechanical compaction and added a new Document S1 in Supplementary data to describe these studies in details.*

Supplementary data-15 (according to old version) is added to the main text (line number: 493 to 511)

Comment 10: P12, L400-405, it would be better to show the supplementary data 14 in the text rather than outside the manuscript. This is actually a pretty important result and consistent with the topic of the paper.

Response: *DONE. We have incorporated it in the main text.*

Comment 11: P13, L415-417, this is a good point here about the smectite content potentially resulting in over-estimated MAD porosity and one of the things I was getting at above in point 1. However, until there is some better clarity about the clay mineral composition, it is a moot point. I suspect this is a contributor for sure but again this needs a more detailed discussion as to why this is the case and appropriate supporting references quoted.

Response: *We have modified Fig.2 for clarification on the clay mineralogy (details mentioned in the response of Comment 1). We also clarified this issue in the text (Line number: 466 to 469).*

Comment 12: P15, L506, the authors discuss “intra-crystalline” slip as a mechanism for particle re-arrangements during compaction. This term has a very specific meaning, usually around things like dislocations and the like within crystals, calcite being a classic example at relatively low temperatures and pressures. One thing the authors haven’t really discussed is the flocculation and aggregation of clay minerals (although flocculation first makes an appearance in the conclusions section on P17, L545 and aggregate in the discussion). The results of these processes can be clearly seen in the SEM images the authors show, often with multiple clay platelets stuck together in aggregations, but this is not described in the images clearly. The intra-crystalline slip quoted here appears to be more like intra-aggregate slip of particles or inter-platelet slip and these would be better ways to describe these phenomena. Again, there is a lot of background work done on clay fabrics in compacting clays with well established terminology which isn’t referenced here, I think some in the references I have noted above for the early work and plenty in the Microstructure of Fine-Grained Sediments compilation as starting points. The manuscript would really benefit from a brief introduction to typical microstructural fabrics of clays which then naturally leads into the discussion.

Response: *We are aware of the important role assigned to flocculation and other mechanisms of particle aggregation in clay-particle deposition. The image regions we have termed “clay-rich domains” appear as somewhat discrete masses because they are distributed between the larger silt particles, but there is no clear way to ascertain if particular particles within the observed clay-rich domains were ever joined together as a discrete floc prior to deposition. We have endeavored in the revision to be consistent in using the less interpretive term “clay particle” or “clay-rich domain” and to generally avoid the term “aggregate” or any reference to flocculated structure, as our focus is on the post-depositional modification of the pore structure, regardless of the depositional mechanism. The terminology intra-crystalline slip is changed to ‘Intra-particle slip (Line number: 27 and 593 of revised manuscript). It’s also important to note that IODP coring may not capture the precise sediment-water interface and is not ideal for examining flocculation. Box cores might be preferred.*

Comment 13: As an overall comment, there is a lot of good data in the supplementary data that would really back up the observations made in the text. I have already mentioned 14 above, but 11 and 12 to me are also no brainers for supporting the hypothesis put forward by the authors. To be honest, I don’t think tables 2 and 3 bring anything to the paper, they are not things that can be quantified, and these for sure should be supplementary data as figure 8 covers their input adequately. If one was going to include a table of useful data in the paper, I would pick supplement 7, perhaps a slimmed down version with some columns removed and the full table still provided as a supplement. This would provide much more useful and quantifiable data to the reader.

Response: *We have added Supplementary data-14 to the main text (Fig.12 in revised manuscript). We deleted Supplementary data-11 (number from old version).*

We have removed table-2 and 3 (number is from old version of manuscript) from the main text.

Comment 14: Following on with the discussion on supplementary data, the large pore outlined in the image of the deepest sample in supplement 10 looks like a plucked grain to me. One can see the compacted clays around the edge of the hole outline and within the hole. I would use a better image than this if you have one. The other two images in this supplement for the shallower samples are clearly geologically realistic and relate to the interactions between rigid grains.

Response: *DONE. Now we have modified Supplementary data 10 (according to old version of manuscript) with a new image for the deepest sample. This is represented in Figure S9 in Supplementary data in revised version.*

Comment 15: For figure 1b, the abbreviation HANP needs spelling out or removing if not relevant to the study.

Response: *DONE. HANP is removed from Figure 1b.*

Comment 16: In figure 2, b and c are the wrong way round. This plot would benefit from clay mineralogy data (e.g. illite vs depth, smectite vs depth, clay content vs depth, I-S vs depth etc) on a single plot (or added to the porosity plots).

Response: *DONE. We have modified Fig.2 and incorporated depth vs Smectite, and depth vs illite for all samples. Moreover, we also added a new Figure S2 in Supplementary data for I/S expandability vs depth for the analysed samples.*

Comment 17: Figure 3 would benefit from having a 1:1 line for easy reference for the reader. Also, the Boom Clay symbol colour on the graph doesn't match the legend.

Response: *Done*

Comment 18: The references are a real mess, many not in the text (all the ones with red ticks on the marked manuscript were ones I could find, including in all the supplementary data), many dates are inconsistent between text and reference list (marked in manuscript) and many in the text but not in the reference list. To be honest, this is much more than a reviewer should have to deal with and tidy up!

Response: *Done.*

Comment 19: There are a bunch of typos and suggested edits also in the marked-up manuscript supplied.

Response: *DONE.*

Reviewer2# (Dr. Bernhard Schuck)

Comment 1: Although quite prominently presented in the abstract (lines 17 – 21), the change of deformation mechanism from rotation and realignment of clay-platelets dominating in the shallower section (0 – 28 mbfs) to bending and subsequent sliding / fracturing of clay particles dominating in the deeper sections is not that evident from the observations presented. Maybe a figure presenting evidence for both mechanisms next to each other would support the statement. In addition, the manuscripts describe the change of deformation mechanism observed at 28mbfs but does not elaborate on the underlying reason. Maybe the authors could discuss this aspect.

Response: *A new figure-S12 in Supplementary data is added to clarify both mechanisms next to each other.*

At the shallow depth above 28mbfs, due to greater porosity, particles get enough free space for rotation to align parallel to the bedding plane. However, below 28mbfs depth, as porosity decreases, space problem arises which causes particles to deform by bending and subsequently fracturing with increase in compactional strain (Line number: 602 to 606 in revised manuscript).

Comment 2: In addition to information on clay mineralogy (lines 113 – 121), additional information on bulk mineralogy should be presented, too. What is the bulk composition (not only the clay mineral assemblage)? In this context I have to admit that the information which should be transported by Supplementary data-1 is not clear to me – especially at the position where it is referred to (line 114).

Response: *Bulk composition of the analysed samples are documented in Table-1.*

Supplementary data-1(according to old version of the manuscript) is defining the size of different clay minerals present in the samples. It is shown in the table that the mica (both illite + chlorite) particles observed in the size range both <2 μ m (similar to clay size particles) and >2 μ m (similar to silt size particles). In the new version of the revised manuscript the table is represented as Table S1 in Supplementary data. To clarify this issue, we have also modified caption of the table.

Comment 3: The fact that site U1480 consists of several holes, whose samples have been used for this study, should already be mentioned in line 94 instead of line 131.

Response: *DONE. (Line number: 111 in the new version of the manuscript)*

Comment 4: Section “sampling and methods”: based on the information given, I would assume that sample preparation and analysis in Aachen and Austin was done by Ar-ion cross-section polishing succeeded by SEM investigations (i.e. BIB-SEM) with the main difference between both universities having been the instruments used (BIB polishing device: JEOL vs. LEICA; SEM: Zeiss vs. FEI). Is this correct?

Response: *Yes, this is correct.*

Comment 5: MAD measurements already performed should be mentioned earlier than in the “results” section.

Response: *DONE. It is also mentioned in the ‘methodology’ section (Line number: 206 to 215).*

Comment 6: Has the mineralogy presented in lines 227 – 231 been determined based on EDX element map / point analysis or is it the outcome of XRD bulk measurements (as presented in lines 113 – 121)? Qtz, Fsp, Cc, Mica und Ill are only five phases – what is the sixth phase mentioned in line 227? How was the detrital origin of the clay-size particles determined (l. 228)? Assuming that information in this paragraph are based on EDX analyses (i.e. no diffraction patterns), I wonder how it was possible to conclude that clay-size particles are dominantly illite, given that XRD analyses reveal the presence of smectite and illite/smectite.

Response: *Mineralogy of the samples are analysed using two techniques; 1) ship-board XRD analysis, and 2) EDX elemental map and point analysis on BIB cross sections. Using EDX elemental map or point analysis, it is observed that six mineral phases are dominant in the samples; these are- Quartz, feldspar (K-feldspar, Na-feldspar), calcite, pyrite, micas (muscovite, biotite, and chlorite) and clay. From ship-board XRD analysis we came to know that clay-size particles are dominantly Illite in composition. To clarify this issue we have modified the main text (Line number: 272 to 274 (in revised version of the manuscript)).*

Comment 7: Given that segmentation of SE2 images only gives pores without additional information on the kind of pore, I wonder how the amount of intergranular pores (>99%) could have been determined and if this value can be considered to be representative or just a qualitative assessment (lines 241 – 242).

Response: *This is a qualitative assessment, based on the rarity of intragranular pores.*

Comment 8: Exchange (1) and (2) in line 254 to be consistent with Fig. 4 and lines 251/252 and 255, respectively.

Response: *DONE. The line number in the revised manuscript is 293 to 294.*

Comment 8: Contacts “EE”, “EF” and “FF” should be highlighted in Figures 5, 7 and Supplement 13.

Response: *DONE.*

Comment 9: To be consistent with the information given in lines 281 – 283, a similar information should be given at some point in lines 259 – 267.

Response: *DONE. (Line number in revised manuscript 305)*

Comment 10: Unfortunately, I was not able to extract the bulk density of the sediment from the reference McNeill et al. (2017) to calculate the vertical effective stress (cf. line 373). Maybe the authors could provide these values in the manuscript / the supplement?

Response: *DONE. Ship board Bulk density values for the analysed samples are collected from the IODP website (Line number in revised manuscript 408 and 409).*

Comment 11: The statement given in lines 416/417 that “shallow depth samples are richer in smectite compared to the deeper samples” should be somewhat adjusted, because Table 1 shows that it is only true with respect to the difference between Unit I and Unit II, not with respect to Unit III.

Response: *To clarify this aspect we have modified Fig.2 where we added smectite content vs depth plot for the analysed samples.*