Detailed response to reviewers' comments

<u>Andre Hüpers' comments (external reviewer):</u>

General Comments

Reece et al. conducted a comprehensive set of geotechnical tests on resedimented claystones recovered from IODP Site C0011, which they blend with silica silt. The methods are well explained and the presented data of Atterberg limits, consolidation characteristics, permeability, grain size and micro fabric analysis are of high quality. The authors provide summaries of key parameter in tables and figures such that it is easy to compare them with other data on physical properties and/or employ them for numerical models. In the light of pore core quality during IODP EXP 322, the applied re-sedimentation technique is an important approach to assess consolidation properties with a small number of minor corrections that should be completed prior to publication.

1. Accuracy and completeness of technical (scientific) content:

In general the reported data sets are complete and easy to read and comprehend. However, there are two topics which should be solved before publication:

a) It remains unknown how many samples went into the artificial mixtures. Did you use one sample from Unit III and one sample from Unit IV? In case most of the samples originated from Unit III, the resulting mixture would also be more representative for Unit III. Thus, it is important to know how many samples were used (and from which depth/units) and how much volume/mass of the samples went into the mixture. Please provide a sample list with information.

We absolutely agree with the reviewer. We used 4 samples of 13 kg in total of Unit III and 5 samples of 10.6 kg in total of Unit IV with one sample that contains material from both units and weights 1.3 kg; so almost equal amounts of both Lithologic Units. We added a new Table T1 with a detailed sample list to the manuscript. This moves all table numbers up by one digit.

b) The authors describe a change in compression indices with decreasing values for higher effective vertical stresses but only report value for 5-21 MPa. Please provide also Cc (and Ce) for the low stress range such that the reader can assess the magnitude of change.

This is a good point. We determined Cc values for the low stress range (0.2 to 5 MPa) and reported the numbers in the text and added them to Table T6. Ce was incorrectly stated to change with vertical effective stress. The magnitude in change is negligibly small. Also, we only unloaded the specimens to an OCR of four = 5 MPa, so we do not have unloading data from 0.2 to 5 MPa in order to compare with Cc. Thus, we deleted the part of the sentence that said Ce would change with vertical effective stress.

Embedded comments in PDF:

Abstract:
You should use the term Claystone to be consistent with lithological description of the EXP 322 preliminary results and proceedings

> ***Edited as suggested throughout the entire manuscript.***

Introduction:
In addition to this very broad benefit, how does NanTroSEIZE benefits from the data report? 1 or 2 sentences how the data could be used in the NanTroSEIZE framework would be nice ...

> ***We added two sentences on how our data is important for NanTroSEIZE or other convergent margins.***

You may want to cite also Tobin and Kinoshita (2006) who summarized NanTroSEIZE goals

> ***We added Tobin and Kinoshita (2006) as reference.***

Citation needed

> ***We added Underwood et al. (2009) as citation here.***

What is the benefit of using resedimented samples? It is mentioned in the methods section but it may be appropiate to present it here.

> ***We agree with the reviewer. We moved the statement about benefits of resedimentation to the end of the introduction expanding a bit on its purpose for systematic studies to understand fundamental sediment behavior.***

Laboratory Testing Methodology (sample handling and preparation):
It is unclear how many samples were used and from which Units. A sample list should be provided

> ***We agree with the reviewer's comment. A total of 10 bags with material from various cores were collected. The total mass was 24.9 kg. We added a sample list as Table T1 with details on mass, core origin, and lithologic Unit of each bag.***

Do you actually "pulverized" the sample? If so, it may be necessary to explain why...

> ***We ground the bulk material in a ball grinder in order to destroy aggregates and start from an unstructured fabric. We added this detail to the manuscript.***

Do you used the same amount (either mass or volume) from all samples to form the batch?

> ***No, in order to have the maximum amount of material for the single batch allowing several geotechnical experiments, we mixed all 24.9 kg together in one single batch ensuring homogeneity. Thus, we did not use the same amount of each bag or core or unit. This detail is now added to the description of sample preparation.***

Laboratory Testing Methodology (Sample description):
Citation of methods is needed here.

> ***Based on Michael Underwood's comments we added some details on the method and also added two citations (see response to comment further below).***

Laboratory Testing Methodology (Particle Size Analysis):
What surface?

> ***We clarified that we meant the air – suspension interface.***

Laboratory Testing Methodology (Resedimentation):
add (G_s)

> ***Edited as suggested.***

R/V Chikyu instead of JOIDES Resolution

> ***Edited as suggested.***

Laboratory Testing Methodology (Constant rate of strain consolidation testing):
add 'base' between excess and pore pressure

> ***Edited as suggested.***

add 'pore' between base and pressure

> ***Edited as suggested.***

The equation looks odd as it is although is not false. You may want to rearrange the equation to sigma'v = sigmav-uc -2/3*delta_u because you substract chamber pressure from the applied vertical stress and then substract 2/3delta_u (cf ASTM for CRS tests eq. 12,18,21).

> ***Edited as suggested.***

Results (Resedimentation):
It would be nice to have all Cc in the table 4, too.

> ***We agree with the reviewer's comment. We included the Cc values from resedimentation to Table T5 (previously Table T4).***

Results (Consolidation Testing):

Not consistent with Table 5 (1.63-0.98)

> ***The reviewer pointed a mismatch out. We updated the numbers in the text with the correct values from the Table.***

What is the eff stress range from which you determined Cc?

> ***This is a valid question as we state that Cc changes with stress. We added to the sentence that the determined Cc values of 0.36 to 0.24 are determined over the stress range between 5 MPa and 20 MPa.***

If you want to make this statement you also should provide some numbers. What is Cc at low stresses? One possiblility is to determine Cc from 100kPa to 1000kPa and add results to table 5. Similar problem with Ce

> ***We agree with the reviewer's comment. We determined Cc values for the stress range between 0.2 and 1 MPa and added these numbers to the text and Table T6 (previously Table T5).***

Figure captions:

Figure 1: add (A-A')

> ***Edited as suggested.***

Figure 1: What is the red star?

> ***Red star indicates earthquake in 1944. We added this description to the figure caption.***

Figure 2: Remove minerals from table which have 0%. What is the error of the method (in general or for a specific mineral)?

> ***Entries with 0% are removed from Table T1 (now Table T2). We added errors for each mineral to Table T1 (now Table T2).***

Tables:

Table T1: remove 0% entries

> ***Edited as suggested.***

Table T3: in mass-%

> ***Edited as suggested.***

Michael Underwood's comments (co-chief):

First, for some reason, my copy of the report did not include any of the tables. I do have Tables T1 and T3 from Andre's edit.

The tables were for some reason not included in the original submission; instead, they were emailed to IODP later. That might explain the missing tables. However, all tables were submitted.

Abstract. Make it clear from the beginning that the so-called Nankai "mudstone" is actually homogenized from a large number of discrete specimens. As I recall, we threw lots of "junk" into the bucket to provide Peter with enough sample volume for these mixtures. Nowhere in the report does that really come across. You might also wish to link the grab-all term "mudstone" to the more technical designations of "silty clay" to "clayey silt", as used during the shipboard descriptions. Finally, silt-sized and clay-sized should be changed to silt-size and clay-size, both in the abstract and throughout the text.

Edited as suggested.

Introduction. The first sentence is out of place. I would move that sentence to paragraph 4, and then follow Andre's suggestion to explain succinctly how this study actually benefits NanTroSEIZE. In addition, for this particular data set, I see no reason to get into the details of the units and facies designations. You're just running tests generic Nankai "mudstone", so the facies (which were defined and separated by the presence of other interbedded lithologies: sand, ash/tuff, etc.) shouldn't matter.

We moved the first sentence of the introduction to the beginning of paragraph 4 and deleted some of the details on lithologic units and facies.

Sample handling. You need to specify exactly where (mbsf) all of the individual specimens are positioned in the stratigraphic column. As Andre noted, you should specify the amount (by weight or by volume) from each depth interval that got blended into the mix. You should also provide a more thorough characterization of the commercial silt. The range of particle sizes for "silt" extends from 63 to 4 (or 2) microns. Is the commercial material coarse, medium, fine, poorly sorted, well sorted silt, or what? I think this is important because geotechnical properties at the coarse end of the silt spectrum will differ quite significantly from properties at the fine end. That size distribution is shown in F5, but some description in the text would be even better.

Based on co-chief's and the reviewer's comment, we added a new table with a sample list that describes the amount by mass from each depth interval.
We agree that we should state more details on the commercial silt. We described the silt as a fine, ground, poorly sorted (well graded) silica. We also added numbers for coefficient of uniformity and coefficient of curvature to better quantify and describe the grain size distribution of the commercial silt.

Sample description. The XRD methods used by your commercial outfit need to be described in some detail. Sample preparation is particularly important, including the size split at 2-microns and surface preparation (spray-painted, filter-peel, oriented, random, etc.). There should be some analysis of error here, both precision and accuracy. I also think it would be useful to compare the wt-% values for your homogenized mixture with the range of values that we calculated from the shipboard XRD measurements of bulk powders. At first glace, the numbers seem to be pretty close. Similarly, it can't hurt to compare the clay-size fraction with the range of values documented in the Underwood & Guo data report. I distributed those data to the entire scientific party last year when the report was submitted, but if you can't find the data, I will send you a copy of the entire report. The method for determining %-expandability also needs to be specified. In looking over Table T1, I noticed that %-chlorite in the bulk power is less than %-chlorite in the clay fraction. That's impossible, of course, although it's understandable if viewed within the error bars of both measurements. I don't think any of those wt-% values should be reported to the nearest 0.1%. Finally, Part B of Table T1 is labeled as "clay fraction." What you really mean, I think, is clay minerals in the clay-size fraction. I guarantee that there is some quartz (and probably some other minerals) included in the <2-micron spherical equivalent, and that will affect XRD peaks for the clay minerals even if you don't extend the scan to include the quartz peaks.

We added details on the XRD methods such as sample preparation, analysis, method for expandibility, and errors.
The %-chlorite in the bulk powder (4%) is not less than %-chlorite in the clay fraction (3%). We agree that wt.% values should not be reported to the nearest 0.1%. We accordingly rounded the values up or down to the nearest 1%.
We corrected the title of Table 1, part B to "Mineralogy of clay minerals in clay-size fraction (< 2microns) expressed as relative weight percent". We also added a comparison of our wt.% values to the values that were calculated from the shipboard XRD measurements of bulk powders (Underwood et al., 2009) and to the values published by Guo and Underwood (2012) in the IODP 314/315/316 data report.

Particle size analysis. This part is a bit confusing. Why did you run the size analyses AFTER squeezing the mudstone down to a hockey puck? That must have required some sort of disaggregation after consolidation. If so, you need to describe. Was a dispersant added to the suspension to prevent flocculation? If so, specify the composition and the concentration. Finally, these types of methods generally assume a perfectly spherical shape, whereas natural clay minerals and other fine-grained silicates are usually far from perfect spheres. So, the settling rate (Stoke's Law) mimics the behavior of a perfect sphere, even though the maximum and minimum dimensions are much different that the so-called "diameter". I bring all of this up because of your outstanding SEM images, where visual estimates of "size" might differ substantially from the spherical equivalent settling behavior shown by grain-size data.

We performed the hydrometer analyses after squeezing the samples because we wanted to get the grain-size distribution of the exact specimen that was resedimented and uniaxially consolidated. We did look at grain-size distributions before and after compression though and found no difference in composition indicating no mechanical effects on the grain-size distribution. We made this point clear in the data report. We also added details on disaggregation and dispersing agent.

We agree with M. Underwood's comment that the outstanding SEM images could be used to estimate particle sizes, which might substantially differ from the spherical equivalent settling behavior shown by grain-size data. However, we used a well-recognized repeatable approach to characterize the material and stuck to it.

Resedimentation. It is not entirely clear to me how the wt-% values were determined for the proportion of "mudstone" to "silt". Are the proportions based on dry weight before adding the water and sea salt? Is there a correction for the salt that must have been included in the interstitial water before the homogenized mix of mudstone was dried? In the calculation of OCR, it is not clear to me how you determined maximum past effective stress. Is that just equal to 100 kPa, as described in the previous paragraph? Seems true, judging from the e-logP curves, but please clarify.

The proportions of Nankai claystone and silica are based on dry mass before adding water or sea salt.
We accounted for the salt content that was included in the interstitial water before homogenizing and drying the Nankai claystone by assuming an in situ salt content of 35 g/L and in situ water content of 27% based on moisture and density measurements averaged over the appropriate depth range. This results in 26 g/L of sea salt that needed to be added to the slurry to bring it to in situ conditions.
The maximum past effective stress is 100 kPa, therefore, the samples are unloaded to 25 kPa in the resedimentation tests.
We made all these points clear in the text.

Note: In this section, and elsewhere, I noticed some unnecessary switching of verb tense from present tense to past tense, and back again. That style will probably be annoying to many readers. I suggest sticking to one tense or the other.

We edited this section to make it consistently in past tense.

Index properties. Please specify the temperature of oven drying, for reasons that become obvious later. I would also avoid referring to the "pure" mudstone, when it's actually an artificial, homogenized mixture. Did you make a correction for salt content in the pore water when calculating water content? If so, cite the method.

As suggested, we specified the temperature used for oven-drying. We also deleted the word "pure" at all locations in the manuscript. We agree that it is misleading. We did not make a correction for salt content in the pore water when calculating water content.

CRS testing. A brief description of the instrument needs to be added. It's probably the same type of rig as we have at here MU, at Rice, and at Penn State, but no one else will know that. You should also specify the load limit and the ring diameter. That diameter is especially important because of edge effects. If I'm not mistaken, the computation of intrinsic permeability from any particular value of hydraulic conductivity (which is what you actually back out of the test results) requires some knowledge of the permeant properties. Therefore, you need to specify the values of temperature and viscosity and unit weight for the permeant (i.e., your equation 5).

We added specifics on the consolidation equipment such as manufacturer and load capacity. The ring diameter was already mentioned in the text; we just moved it further up, closer to the load frame specifics. We also added information on the fluid properties such as temperature, viscosity, fluid density, and unit weight of water.

Results. Atterberg limits. Turning the crank on the Casagrande cup is fun. It's quite likely, however, that the consistent difference between air-dried and oven-dried is due to loss of interlayer water from smectite during oven drying, which perturbs values of water content. That's why you need to specify the oven temperature, which I'm guessing is 105°C. You should at least bring this caveat to the reader's attention. Better still, you might wish to calculate a correction to the water-content value based on the average %-smectite (in bulk mud) and an assumption of 2 layers of interlayer H2O. With high percentages of smectite in the bulk sample, this artifact can be rather significant.

We came to the same conclusion that the difference between air-dried and oven-dried samples is due to the loss of interlayer water from smectite during oven-drying. However, we did not make this clear in our initial submission. We added this comment to our manuscript and listed the oven temperature in the methods section. We appreciate M. Underwood's suggestion to calculate a correction to the water content values based on the average %-smectite in the bulk mud. However, we did not find this as necessary in an IODP data report. We did highlight the problem associated with the loss of interlayer water from smectite during oven-drying and the resulting implications though.