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Discussions

Interactive comment on "Effects of relative density and accumulated shear strain on post-liquefaction residual deformation" *by* J. Kim et al.

Anonymous Referee #2

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The authors describe a series of Ko-consolidated, undrained cylindrical torsional tests performed on a sand to evaluate the combined effects of residual shear strain and residual volumetric strain. Although this topic is valuable to practicing engineers when assessing the consequences of liquefaction-induced lateral spreading, the manuscript is poorly written and hard to follow. Firstly, the manuscript needs to be thoroughly edited for English usage and grammar. Poor grammar and usage made it quite difficult, in many instances, for the reviewer to understand what the authors were trying to say. Furthermore, the experiments themselves are poorly explained and the effect of Ko consolidation is not illustrated clearly. As a result, the reviewer recommends that the paper be declined for publication. My detailed comments below are keyed to page number/line number.

2/5. Residual shear strains only accumulate if some slope or free-face exists at a site. Under level-ground conditions, residual shear strains do not accumulate. Thus, this study only applies to sites that are susceptible to lateral spreading. This is not clearly articulated by the authors.

2/12-13. In the 1980s, Dobry and his co-workers developed a very similar testing device that allowed stress-controlled cyclic torsional loading to liquefy a soil followed by stress-controlled monotonic loading. The device could apply equal all-around or Ko consolidation stresses. This device was used extensively to study the hydraulic fill materials from Lower San Fernando dam, as well as other sandy soils. Thus, the authors' device and test method is not novel, and the authors should review the work by Dobry and his co-workers to put their own work into proper perspective.

2/13. What is "Ko drain"? Are the authors referring to reconsolidation?

2/15-16. Why would a Ko condition exist during cyclic loading that generates excess porewater pressure (leading to liquefaction)? Ishihara and his co-workers have shown that the coefficient of lateral stress, K, approaches unity as the excess porewater pressure approaches the effective vertical stress (i.e., as a soil approaches level-ground liquefaction). Therefore, it's not clear why it is relevant or appropriate to maintain a Ko condition during cyclic loading, post-cyclic monotonic loading, and reconsolidation.

2/20. What are "restoration behaviors"?

2/22. What are "structure restoration characteristics"?

3/16-22. Bray, Dashti, and their co-workers clearly have shown that a large percentage of shaking-induced settlement (including liquefaction-induced settlement) occurs during shaking because an undrained condition is not maintained for most soils during shaking. Post-shaking reconsolidation certainly does occur, but this may be a smaller fraction of the total shaking-induced volumetric strain. The authors appear to have missed this key aspect of soil behavior in their work.

3/25. Stewart and his co-workers were focused on settlement of unsaturated soils during shaking. How does this relate to the current study?

3/26-27. Again, this statement is not consistent with the work done by Bray, Dashti, and co-workers.

4/11. What are “online testing techniques”?

5/11. At what rate are the liquefied specimens drained? Does the drainage rate affect the development of residual shear strains?

5/15-17. This statement is poorly written, and I don’t understand what the authors are trying to say. Why would the occurrence of lateral deformation be prevented under undrained conditions for horizontally stratified soils?

5/19. What are “indoor test programs”?

5/23-25. As noted above, residual shear strains only accumulate in sloping ground or near a free-face. As a result, the residual shear strain is a function of both the number of cycles as well as the static shear stress. That is, for a given number of cycles, the higher the static shear stress, the larger the residual shear strain. The authors appear to have neglected this key variable.

Figure 1. This figure appears to illustrate an annular specimen. However, the authors earlier stated that the specimen is cylindrical. Please clarify the text and/or modify the figure accordingly.

6/6. The shear strain per pulse will be a function of the specimen height. Most commonly, this rotation per pulse is reported in radians (followed by a shear strain per pulse corresponding to a particular specimen height).

6/11. Again, this should be rotation, not shear strain.

Figure 2. (a) needs a scale and an indication of the orientation of the photo. (b) is not needed.

6/24. Ko is the coefficient of earth pressure at rest, while K is the coefficient of lateral stress. Please clarify.

Figure 3. This figure is very difficult to follow since the authors don’t include the cyclic loading history and measured porewater pressures with the plots. Does cyclic loading begin at t = 0 sec? If this is the case, a Ko condition is not maintained during cyclic loading as the authors indicated in the abstract. Clearly, during both sets of tests Ko approaches unity (presumably indicating liquefaction). That is not a constant Ko condition (nor should it be). Furthermore, on page 7, the authors indicate that liquefaction occurred completely (what is complete liquefaction?) at 12500 sec. This needs a porewater pressure plot (or some other clear data) to illustrate.

7/10-11. This statement is not clear. The “. . .vertical control method is [adopted] to eliminate the effects of the Ko control method.” According to the authors, isn’t the vertical control method one type of Ko control. And I’m still not clear on how the authors are controlling K in this test.

7/15-17. This statement is not clear. How is the vertical stress changed to produce a constant total stress? Do the authors mean the “mean stress” is constant? This must be clarified.

8/11. What is “pre-consolidation”?

9/15. Figure 7 indicates that the cyclic load is applied at a rate of about 8 or 9 cycles / 1000 sec, or less than 0.01 Hz. What loading case is this intended to simulate. Because the load cycles are so slow, there is ample time for local void ratio change and creep to occur between load cycles because of the high permeability of the sand specimens. So, even though the global void ratio is unchanged (i.e., global undrained conditions are maintained), it is highly unlikely that local void ratio is unchanged. This could dramatically affect the test results.

Table 3. This is a key issue for this paper – six tests are FAR too few to recognize

significant trends when there are multiple variables involved!

Table 4. This is another key issue for this paper – the values of Ko used for testing are not reasonable for the sand. The table indicates that the consolidated relative densities range from 38% to 59%, yet the Ko values range from 0.53 to 0.57 (and do not show any trend with Dr). Using Jaky's equation, Ko values of 0.53 to 0.57 correspond to drained friction angles of 28 to 25°. These friction angles are NOT consistent with sand specimens with relative densities of about 40 to 60%. Reasonable friction angles for this sand and these relative densities should be on the order of 33 to 36°, corresponding to Ko values of 0.46 to 0.41. How do these unreasonable Ko values affect the authors' interpretations?

11/1. Rate of strain, not strain speed.

Figure 10 and related text. This figure shows that the excess porewater pressure builds more SLOWLY as the relative density increases, it does not build more QUICKLY as stated by the authors.

11/16-17. Again, the excess porewater pressure ratio does NOT increase with increasing relative density. Figures 10, 11, and 12 clearly show that the rate of excess porewater pressure generation decreases with increasing relative density.

12/16-18. This statement is not well-supported. How is this shown in the figure?

Conclusions. As a result of the numerous inconsistencies and questions throughout the study, it is impossible to comment on the reasonableness of the conclusions.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 1579, 2013.