Title

Closure to Assessment of Liquefaction Potential During Earthquakes by Arias Intensity by Robert E. Kayen and James K. Mitchell

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Authors
Kayen, Robert E
Mitchell, James K

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SALLOP: Simple Approach for Lateral Loads on Piles[a]

Discussion by D. Hossain,[2] Member, ASCE

The author is to be congratulated for his presentation of a "simple" approach for calculating the lateral loads on piles using preboring pressuremeter test (PMT) data. The method is based on a comparison of the results of twenty full-scale horizontal pile load tests with those predicted from PMT data. The discusser wishes to supply the following comments on the proposed method.

- Although the method is simpler than the earlier method from the Texas A&M University group, the discusser does not find it as simple as the title of the paper would suggest, because (1) it's use needs PMTs, which are not as simple or common as some of the other in situ tests used in routine site investigation, and (2) it involves adherence to nine steps involving use of a similar number of equations, in addition to the trials and interpolations (depending on the pile length and "end condition" and modifications for any group action). "Simplified PMT Approach for Lateral Loads on Single Piles" would have possibly been a better choice of title.
- Some of the suggested steps could have been made more "complete." For example, in step 3, the source of soil spring constant K needed for calculation of transfer length l_o, and hence zero shear depth D_V, should have been mentioned. Similarly, in step 6, guidance is necessary as to what should be done if the deflection calculated at $H_o/3$ through use of (13), (15), or (16) is not acceptable for the intended functioning of the concerned structure supported by the pile/piles.
- In Table 2, it appears that the EI values of concrete filling the pipe piles were not taken into consideration. This affected the calculation of l_o and D_V.
- The author is fortuitous to have achieved an r^2 value of 0.977 in his comparison of $H_{ou(\text{measured})}$ and $H_{ou(\text{predicted})}$ which is based on 20 piles differing in material (steel, timber, concrete), in degree of soil displacement (e.g., driven, timber, and precast concrete piles with large displacement through H-piles with small displacement to bored piles with no displacement), and in ground condition (i.e., clay, sand over clay, sand, etc.). However, the author got disappointed when he faced the large variations in the measured deflection y_o at ($H_{ou}/3 = H_{ow}$) for these piles which varied from 0.009B to 0.034B. The author suggests the reason for this apparently unexpected scenario to include the greater difficulty of predicting soil movement compared to soil capacity. However, in the opinion of the discusser, the important reasons are the different stress-strain characteristics of the soils at the relevant sites (due to difference in soil type, stress history, etc.) and the different degrees of modification brought about by the pile installation procedures into the surrounding soils. While intersite variations of soils depending on the location of a site in reference to the local geological setting is more easily appreciated, insite variation of soil type or response within short distances is also frequently encountered by geotechnical engineers, and especially when dealing with native ground sites. As Table 2 shows, the paper deals with native and man-made soils showing p_L values in the range of 100–730 kN/m^2, E_o values in the range of 1000–7250 kN/m^2, and E_o/p_L ratios in the range of 7.7–20.6. That soils with different composition and stress history are associated with different E_o/p_L ratios is recognized in the literature (e.g., Baguelin et al. 1978). The discusser's own experience with preboring PMT tests in a soft-to-medium, normally consolidated to lightly overconsolidated, sensitive sabkha clay and a stiff-to-very-stiff, moderate to highly overconsolidated brown clay from Obhor, Saudi Arabia, was presented in Hossain and Sabtan (1994). The E_o/p_L values of the former clay were generally in the range of 7–12 (average about 10), and that of the latter clay in the range of 8–25 (average about 15).

In the light of the above, the discusser is of the opinion that an examination of the PMT parameters at the author's reviewed sites, especially the E_o/p_L ratios, would have suggested the possible range of scatter of the deflection at $H_{ou}/3$. The close resemblance of the three curves on Fig. 7, representing piles 1, 2, and 3 from the same site with sand fill over clay, is encouraging in this respect. This suggests that it could be illuminating to examine the measured deflections at $H_{ou}/3$ for all the 20 piles, in groups representing (1) piles in sand fill over clay; (2) piles in sand; and (3) piles in clay/silt clay, etc., and in groups of (1) driven piles and closed-ended steel pipe piles; (2) H-piles; and (3) bored piles. It might be argued that grouping in the manner suggested above would result in a small number of piles in each group, which may not appear statistically representative. Yet, this would be more meaningful from a practical viewpoint. Furthermore, these groups could include some other piles from the sites listed in Table 2 of the paper (having records available in the database), though not pushed to a displacement of 0.1B, their H_{ou} possibly being extrapolated through comparison of their load-displacement curves with those of other piles at the same site.

Extending the same philosophy, different coefficients applicable to different soil and pile types in place of a single value of 2.3 could be suggested for use in (14) for estimating K relevant to $H_{ou}/3$. Alternatively, all analyses could be made to first determine the loads corresponding to a number of allowable deflections (e.g., 0.01B, 0.015B, 0.02B, etc.) and then check the corresponding factor of safety; this would make it possible to use the pile load tests that did not reach 0.1B deflection and allow determination of the lower limit of the resulting factor of safety.

An indication of the author's recognition of the above philosophy is understood from his use of markers for different soil and pile material types in Figs. 7–9, but without any discussion. It is considered that such a discussion, which would also include the effect of the different volume of soil displacement, would be enlightening.

The discusser cannot agree with the author's suggestion to ignore, without reservation, the beneficial effect of a crust. It may be more realistic to make an appropriate decision for such a situation after an assessment of the ratio of the strength of the crust to that of the underlying layer, the ratio of pile width to crust thickness, pile length, etc.

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[a]October 1997, Vol. 123, No. 10, by Jean-Louis Briaud (Paper 12280).
[2]Sr. Geotech. Engr., Soil Probe Ltd., 110 Ironside Crescent, Unit 20, Scarborough, ON, M1X 1M2, Canada.

Discussion by Claudio Cherubini[3] and Francesco Santoro[4]

Although the paper is really interesting, it seems useful to point out some statistical considerations:

1. Toward the comparison between measured and calculated deflection (Figs. 8 and 9), two determination coefficients have been obtained: $r^2 = 0.082$ and $r^2 = 0.35$. From a statistical point of view, only the second value is acceptable. This is because, for a number of measured data equal to 20, the necessary value of r^2 to reach a significance level of 10% must be equal to 0.3598, while to reach a higher level, that is, 1%, r^2 must be equal to 0.5368 (Gennaro 1975).
2. In order to overcome the known problems connected with the use of regression techniques (Troutman and Williams 1987)—which will not be summarized in this discussion—it is possible to analyze the set of the predicted value/measured value ratios (Cherubini et al. 1995). Once the above-mentioned ratios have been obtained, it is possible to calculate for them the mean value and the coefficient variation. These two entities allow estimation of the accuracy and the precision of the predicted values, where the words accuracy and precision have the following definitions:
   - A perfectly accurate method would be one that resulted in calculated values equal to the measured ones in every case (Tan and Duncan 1991). In a statistical sense, an accurate method would be one that yielded, for a set of n values of the predicted/measured ratio, a mean close to unity (Cherubini and Greco 1988).
   - A statistically precise method (Chapra and Canale 1988) would be one that, for the same set of n values of the predicted/measured ratio, resulted in very low scatter estimated; for example, by means of the coefficient of variation (Cherubini and Greco 1997; Cherubini, et al. 1995).

With regard to the paper discussed, we can therefore state:

- Regarding the ratio $H_{ou\ \mathrm{meas}}/H_{ou\ \mathrm{pred}}$, the mean value (equal to 1.06) leads to a good accuracy, while the coefficient of variation (equal to 28.4%) leads to a fairly good precision.
- Regarding the values $y_{\mathrm{meas}}/y_{\mathrm{pred}}$ (0.02 B method), the mean value (equal to 0.916) leads, again, to a good accuracy, while the coefficient of variation (equal to 54.7%) leads to a bad precision.
- A different behavior has been detected for $y_{\mathrm{meas}}/y_{\mathrm{pred}}$ (K method): the mean value (1.233) is higher than 1, with a high coefficient of variation equal to 54.05%. In this case, however, we can exclude from the whole set of the previously defined ratios the 20th value that is equal to 3.387. This value has to be considered, according to Chauvenet's criterion (Taylor 1986), an "outlier." Making this adjustment, the mean value becomes equal to 1.109 while the coefficient of variation shows a notable reduction, becoming equal to 39.04%.

To come to the point, via the elaboration of the predicted value/measured value ratios, it is possible to obtain useful information about the reliability of the prediction made by the theoretical model, in addition to the information possible using the regression techniques, which are affected by lots of conditions and limitations.

[3]Assoc. Prof. of Soil Mech., Technical Univ. of Bari, Via Orabona, 4-70125 Bari, Italy.
[4]PhD Student in Geotech. Engrg., Technical Univ. of Bari, Via Orabona, 4-70125 Bari, Italy.

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Comparison of Four Methods to Assess Hydraulic Conductivity[a]

Discussion by Nadim F. Fuleihan,[6] Member, ASCE, and Anwar E. Z. Wissa,[7] Fellow, ASCE

The authors have presented an interesting case history: They evaluated differing clay liner construction techniques implemented by four different contractors that built test pads with the same borrow source. Whereas the discussers do concur that the quality of construction accounts for the documented differences in test pad performance, the discussers believe that better construction techniques could have been employed, and some construction problems avoided altogether, if the contractors had been adequately forewarned about the importance of hydration time and moisture equilibration.

The authors also compared four different test methods used to measure the hydraulic conductivity of the clay liner in each of the four test pads. They concluded that (1) all three large-scale (so-called "field-scale") test methods yield similar results, and that (2) tests on small specimens are of little value and should not be used. A review of the data presented by the authors does not lead the discussers to the same conclusions with respect to the relative ranking and reliability of the four test methods.

CLAY LINER CONSTRUCTION

The authors present a comprehensive evaluation demonstrating the importance of hydration time and moisture conditioning. The discussers agree, but feel that the contractors ought to have been adequately forewarned about this important

[a]October 1997, Vol. 123, No. 10, by Craig H. Benson, John A. Gunter, Gordon P. Boutwell, Stephen J. Trautwein, and Peter H. Berzanskis (Paper 12512).
[6]Prin. Engr. and Vice Pres., Ardaman & Associates, Inc., Orlando, FL 32859-3003.
[7]Sr. Consultant and Pres., Ardaman & Associates, Inc., Orlando, FL.

issue. In other words, the specifications should have required the contractors to hydrate the soil and allow it to thoroughly equilibrate prior to compaction, especially since the "clay had been stockpiled for a number of years and was very dry. . . ."

The specifications required the contractors to place the clay at moisture contents between the Standard Proctor optimum water content and 3% wet of optimum. It would have been more appropriate to specify a moisture content range up to 5% wet of optimum [e.g., Fuleihan (1989)] to provide for better hydration and more effective kneading. The "significant scatter in optimum water content" was probably caused in part by the differing degrees of hydration. Hence the clay was not adequately hydrated throughout, and the liner was not consistently compacted wet of the Standard Proctor optimum water content in any of the four test pads (Fig. 3).

Because of the moisture variability, the authors should have performed additional hydraulic conductivity tests prior to reaching conclusions regarding the reliability of the various test methods. Moreover, it would have been appropriate for them to have discussed construction procedures and quality-control test frequencies implemented during construction of the 14-ha earthen cap, in light of the variability documented within the test pads–particularly since the authors conclude that "it *may* be incorrect to infer that a test pad and liner have similar hydraulic conductivity because they are compacted to the similar water content and dry unit weight." The discussers agree that the type of compactor used, hydration time allowed, and degree of mixing and homogenization, all *do* have significant effects on hydraulic conductivity.

RANKING OF TEST METHODS

The discussers agree with the authors that the sealed double ring infiltrometer (SDRI) typically yields reliable in situ hydraulic conductivity data. The SDRI method, however, is not readily adaptable to quality assurance or quality control testing during production installation, because test durations on the order of 50–100 days may frequently be required (see Fig. 6) and because the performance of numerous SDRI tests on each compacted lift of liner can be very costly. A downward trend in hydraulic conductivity is still evident in Fig. 6 after 50 days of testing (i.e., through the end of December 1992), indicating that equilibrium conditions had not yet been achieved at three of the test pads. In fact, the third of the last three 1992 measurements is typically 2–3 times lower than the first of these three measurements. Therefore, while SDRI tests are desirable to confirm that the hydraulic conductivity of the as-built liner complies with specifications, it is not practical to rely exclusively on SDRI tests for quality control testing during construction of each compacted lift.

The authors go to great lengths attempting to justify data discrepancies in the two-stage borehole (TSB) and large block (B) specimen tests. It appears that the authors had already made up their conclusion a priori in spite of the test data. For example, they seem to accept the range of hydraulic conductivity ratio K_{TSB}/K_{SDRI} as low as 0.29 and as high as 5.3 (Table 2) as being reasonable, and they attribute the high TSB hydraulic conductivities to environmental distress, even though the test pads were subjected to only one winter/summer cycle in Texas, and were protected by a polyethylene sheet covered with 0.3 m of sand. In the discussers' opinion, environmental distress due to freeze-thaw cycles in Texas under such conditions is highly unlikely. Moreover, environmental distress cannot account for the low K_{TSB}/K_{SDRI} ratios of 0.29 to 0.63 (Table 2).

As noted by the authors, the K_{TSB} standard deviations were high, and while the reported geometric mean (Table 2) may yield a closer approximation for the equivalent or special hydraulic conductivity across several lifts of liner (see e.g., Fuleihan 1989; Benson and Daniel 1995), the arithmetic average value better represents the hydraulic conductivity of any given compacted lift. Since only the geometric mean and logarithmic standard deviations were tabulated by the authors, arithmetic means could not be directly calculated. Nevertheless, the results suggest that arithmetic mean and individual TSB values in Test Pads B, C, and D could potentially be one to two orders of magnitude greater than the corresponding K_{SDRI} values. It is misleading, therefore, for the authors to conclude that the hydraulic conductivity measured with the TSBs are only "about twice the long-term hydraulic conductivities measured with the SDRIs, on average" and that either method is acceptable, because the gross average geometric mean value of all test pads, in this context, is meaningless. For example, even based on the geometric mean K_{TSB} values reported in Table 2, test pad B does not comply with specifications (i.e., $K \leq 1 \times 10^{-9}$ m/s), while test pad C does. The exact opposite conclusion is reached with the SDRI test data! The TSB data, therefore, suggest that (1) either the test methodology has serious shortcomings and should not have been used, at least in this particular application, or (2) all test pads exhibited significant variability and, contrary to the authors' conclusion, none was constructed in compliance with specifications.

The hydraulic conductivity was determined in the laboratory on one 0.30-m-diameter block (B) specimen from each test pad. The corresponding hydraulic conductivity ratios K_B/K_{SDRI} ranged from a low of 1.3 to a high of 12 (Table 2), with a gross average of 4.9. The authors proceed to disregard the highest of the K_B/K_{SDRI} values as an "outlier" because the specimen apparently "contained a macroscopic flow path." Even when the so-called outlier data point is excluded, the range of hydraulic conductivity ratio K_B/K_{SDRI} for the remaining three data points is 1.3–4.8, with a gross average of 2.5. The authors attribute the higher K_B values to (1) the use of backpressure, which results in a higher degree of saturation in the laboratory, and (2) the fact that the block specimens typically represent one compacted lift of soil, whereas the SDRI permeates multiple lifts of soil. While the discussers do not disagree with the authors' explanations for the documented discrepancies, isn't it rather fortuitous for each individual K_B value to be higher than the corresponding K_{SDRI} value, in light of the documented variability? Could disturbance or microcracks that may have developed during sampling or trimming explain some of the discrepancy?

The hydraulic conductivity was also determined in the laboratory on 70-mm-diameter thin wall sampling tube (ST) test specimens. One test from each pad was performed in October 1992 by a commercial laboratory, and four specimens from each pad were tested by the authors in August 1993. The corresponding hydraulic conductivity ratios K_{ST}/K_{SDRI} are tabulated below for each test pad and compared with corresponding K_B/K_{SDRI} and K_{TSB}/K_{SDRI} ratios. It is quite perplexing how,

TABLE 3. Hydraulic Conductivity Ratios

Test pad	Sampling tube, ST (12/92 and 8/93 data)	Block Specimen, B	Two-Stage Borehole, TSB	Pass/fail based on SDRI test data
	K_{ST}/K_{SDRI}	K_B/K_{SDRI}	K_{TSB}/K_{SDRI}	
(1)	(2)	(3)	(4)	(5)
A	0.93	1.4	0.63	Pass
B	1.02	1.3	5.3	Pass
C	0.15	12	0.29	Fail
D	0.26	4.8	1.0	Fail
Gross average	0.59	4.9/2.5[a]	1.8	—

[a]Gross average when "outlier" data from Test Pad C is excluded.

based on the data in the discussers' Table 3, the authors can conclude that the three large-scale test methods (i.e., B, TSB, and SDRI) yield essentially similar hydraulic conductivities, and at the same time conclude that small specimens (ST) should not be used for hydraulic conductivity assessment. The authors go on to explain that the observed ''differences in hydraulic conductivity are within the accuracy with which hydraulic conductivity generally can be assessed with any of the three large-scale methods.'' Hence, a discrepancy by a factor of up to 2.5 is apparently acceptable to the authors on a gross average basis, and a geometric mean discrepancy by a factor of up to 5 is apparently reasonable for each individual test pad. When it comes to the hydraulic conductivity determined on sampling tubes (ST), however, equivalent and even smaller discrepancies in hydraulic conductivity are judged excessive-!

As documented by Fuleihan and Wissa (1995), significant discrepancies occasionally exist between small-scale laboratory measurements and large-scale field measurements of hydraulic conductivity, but primarily for poorly constructed liners (such as the liner in Test Pads C and D). For well-built liners (such as in Test Pads A and B), wherein the clay is properly hydrated, the clods adequately kneaded, and the borrow homogenized, there is in general very good agreement between results from small-scale laboratory tests and larger-scale field tests. This conclusion is derived from a substantial data base (Table 4).

Even if one were to disregard all laboratory test data and all data on admixes, as advocated by Benson and Daniel (1995), there would still be at least 22 field measurements in the Fuleihan and Wissa (1995) database (not 4 data points, as inferred by Benson and Daniel 1995), of which 14 measurements correspond to hydraulic conductivities backfigured from very-large-scale field tests (i.e., leak rates from ponds and large test pits). The database referenced above indicates a ratio of large-scale to small-scale hydraulic conductivity of 1.0–2.5 for well-built liners. Hence, excellent agreement exists between field and laboratory data for well-built liners, particularly when one considers that the reported ratios are based on simple arithmetic averages not necessarily representing identical locations. The ratio of large-scale to small-scale hydraulic conductivity for poorly built liners is significantly higher, as expected—on the order of 5 to 15 or more.

TABLE 4. Data Base

Histories and Measurements (1)	Fuleihan and Wissa (1995) Compacted natural soils and admixtures (2)	Fuleihan and Wissa (1995) Compacted natural soils (3)	Benson and Daniel (1995) Compacted natural soils (4)
Case histories from different projects	19	16	31
Case histories in which authors were directly involved	19	16	7
Independent field (F) measurements			
Leak rate (LR) or underdrain (U)	17	14	9
Lysimeter (L), SDRI, or infiltrometer (I)	22	8	44
Total field measurements	39	22	53
Independent Laboratory (L) Measurements on Undisturbed Samples	876	798	—

The authors seem to be unaware of the fact that when small-scale hydraulic conductivity tests are used in quality control, all hydraulic conductivity test data in any given lift must comply with specifications at the 95% confidence level, not just the gross average geometric mean or spacial hydraulic conductivity. A poorly-built liner is expected to exhibit significantly more variability than a well-built liner and, hence, if the construction is supervised by an experienced practitioner and a sufficient number of small scale laboratory tests are performed, there is no doubt that a poorly-built lift can be detected in time and reworked prior to building subsequent lifts of liner. Florida requires performance of at least five hydraulic conductivity tests from each compacted lift within a test pad (and 1–2 tests per acre per lift during production installation). For each 0.9-m-thick test pad evaluated by the authors and consisting of 5 compacted lifts, 25 sampling tube test specimens would have been required (compared to only 4 specimens tested by the authors), and essentially all 25 hydraulic conductivity test data would have had to meet specifications for the test pad to be accepted. It is rather irresponsible, therefore, for the authors to make definite conclusions based on very limited test data, even if their conclusions were consistent with findings reported in other studies.

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Closure by Craig H. Benson,[8] John A. Gunter,[9] Gordon P. Boutwell,[10] Stephen J. Trautwein,[11] and Peter H. Berzanskis,[12] Members, ASCE

The writers agree that better construction specifications and techniques could have been used. However, the writers' objective in this study was to assess the hydraulic conductivity using different methods and to interpret the results of the hydraulic conductivity tests, especially the variation between pads evident in the large-scale test results. Preparing and implementing the specifications was beyond the writers' scope of work. Nevertheless, a recent nationwide study by Benson et al. (1998) indicates that the construction specifications and methods used at this site are typical of U.S. practice. Requirements for hydration time are rarely included in specifications. The writers believe the paper shows that hydration time can be an important issue that should be considered when writing specifications.

Fuleihan and Wissa criticize the specifications that were used, but apparently do not understand the owner's rationale behind having four contractors independently build the four test pads. The objective was to find the contractor who could meet the performance requirements (e.g., field hydraulic con-

[8]Assoc. Prof., Dept. of Civ. and Envir. Engrg., Univ. of Wisconsin, Madison, WI 53706.

[9]Engrg. Consultant, Round Rock, TX 78641.

[10]Pres., STE, Inc., Baton Rouge, LA 70898.

[11]Pres., Trautwein Soil Testing Equip. Co., Houston, TX 77231.

[12]Staff Engr., Hoechst-Celanese, Inc., Pampa, TX 79066.

ductivity $<10^{-7}$ cm/s and compliance with TNRCC sampling and testing criteria) at the lowest cost. Consequently, the contractors were given wide latitude regarding construction approach and methods so that they could maximize their opportunity to win the job. Some of the contractors used more expedient methods to reduce costs but ultimately had a field hydraulic conductivity that was too high. As a result they did not get the job. Surprisingly, the contractor who won the job bid at the lowest cost, and also had the lowest field hydraulic conductivity. This contractor was based near the project, and his experience with local soils undoubtedly helped him win the job. Applying a very tight method-based specification would have precluded meeting the objective of selecting a contractor who could meet the project objective and be cost competitive. All of the contractors would have constructed a similar test pad, with similar methods, having similar field hydraulic conductivity, at essentially the same cost. The writers hope this explanation provides sufficient clarification.

Fuleihan and Wissa also suggest that water contents up to 5% wet of optimum should have been considered. The writers agree that clay clods can be more readily remolded at such high water contents, but other problematic conditions can also arise at high water contents, such as inadequate shear strength, large potential for desiccation cracking, and difficult operation of construction equipment (Seed and Boulanger 1991; Leroueil et al. 1992; Daniel and Wu 1993; Stark and Poeppel 1994; Albrecht 1996). A better approach is to select a range of water contents and dry unit weights that will meet all of the project objectives. Boutwell and Hedges (1989), Daniel and Benson (1990), and Daniel and Wu (1993) describe such methods.

The writers do not agree that the scatter in optimum water content (w_{opt}) was caused by variations in hydration. Optimum water content is determined in the laboratory using a compaction test (e.g., ASTM D 698), where the hydration time is specified. Thus, the degree of hydration probably did not vary significantly in the laboratory. Also, the data shown in Fig. 1 indicate that all four pads were compacted to similar water content relative to the line of optimums. Correlation with a particular optimum is not important, because the effort applied in the field rarely replicates that used in a laboratory test, resulting in different w_{opt} in the field than in the laboratory. Moreover, the line of optimums is essentially unique for all clayey soils (Benson and Boutwell 1992). Thus, the writers find it hard to believe that the pads were not consistently compacted wet of optimum.

Fuleihan and Wissa also suggest that more hydraulic conductivity tests should have been conducted, given the variability in water content. The writers agree that more testing would have resulted in greater confidence in their conclusions, which is nearly always the case in geotechnical practice (Liao et al. 1996). However, as in most cases, budget limitations precluded more testing. The writers disagree, however, that there was large variability in the water content. While there was significant scatter in w_{opt}, the variability in compaction water content within each test pad was typical, with standard deviations ranging from 1.1 to 1.4%.

Fuleihan and Wissa go to great lengths attempting to refute the reliability of the TSB method and the hydraulic conductivities measured on block specimens. Such criticism can often be made of small databases. Even in this case, however, the ratios of the geometric mean hydraulic conductivities were as follows: $K_{ST}/K_{SDRI} = 0.35$, $K_B/K_{SDRI} = 3.2$, and $K_{TSB}/K_{SDRI} = 0.99$. Similar results are evident in the much larger database compiled by Benson et al. (1998). For example, Fig. 11 shows a comparison between hydraulic conductivities measured with TSBs or block specimens and those measured with SDRIs for 31 sites. On average, all three methods yield similar field-scale hydraulic conductivity. In addition, contrary to the discussers' inferences, the authors *did not and still do not* recommend that SDRIs be used as a quality control tool during construction.

Fuleihan and Wissa also dismiss the likelihood of environmental distress but apparently do not understand the wide range of climatic conditions that exist in a state as large as Texas. The site location experiences some of the greatest extremes in the United States. Summers are extremely hot and dry. Winters are cold (subfreezing at times) and wet. Thus environmental distress probably occurred, and to ignore it is inappropriate.

The authors agree with Fuleihan and Wissa that similar hydraulic conductivities are measured in the field (K_F) and in the laboratory on small specimens (K_L) when good construction methods are used that eliminate macroscopic defects (e.g., Benson and Boutwell 1992; Benson et al. 1994a; Trautwein and Boutwell 1994; Benson et al. 1998). In contrast, poor agreement often exists between K_F and K_L when a liner has construction defects, because the network of pores controlling flow in the field is not captured by the small specimens tested in the laboratory. This effect was clearly shown by Benson et al. (1994a) by conducting tests on specimens of various size obtained from a full-size test pad constructed as part of remediation activities at a hazardous waste site (Fig. 12). Indeed, hydraulic conductivities of the common 76-mm-diameter specimens collected, in complete compliance with specifications from this supposedly ''well-constructed'' test pad were about

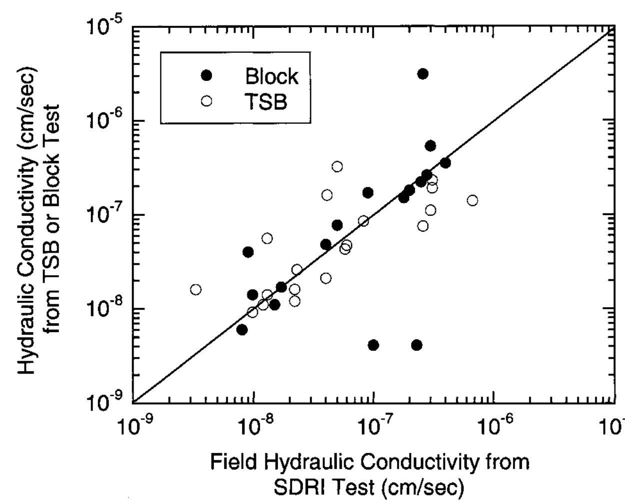

FIG. 11. Comparison of Hydraulic Conductivities Measured with TSB Tests, or on Block Specimens and SDRIs (Adapted from Benson et al. 1998)

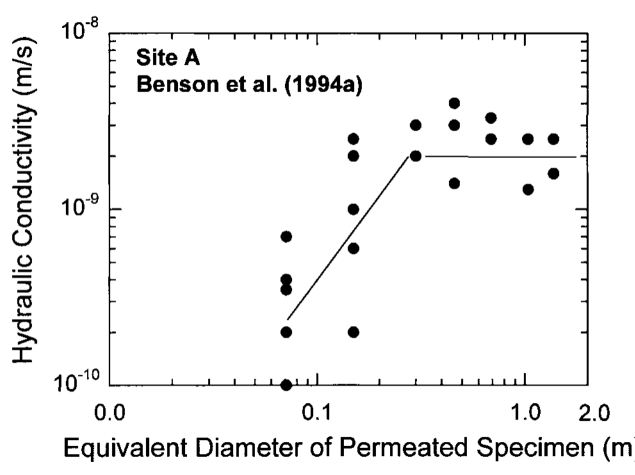

FIG. 12. Hydraulic Conductivity as Function of Specimen Size (Adapted from Benson et al. 1994a)

one order of magnitude lower than the field hydraulic conductivity, on average. Had these specimens been used for construction quality control, an unconservative ''false positive'' would have been recorded. Trautwein and Boutwell (1994) report similar findings.

The need for larger-scale tests for hydraulic conductivity assessments during specification preparation is clear in the example from Benson et al. (1994a). Since the objective of hydraulic conductivity assessment is to determine whether a liner has been constructed appropriately, the methods used to assess hydraulic conductivity must be capable of accurately testing soil prepared using acceptable *or* unacceptable methods. For example, on a project requiring concrete having a compressive strength in excess of 21,000 kPa, compression testing would never be conducted using a testing machine with a minimum reading of 21,000 kPa, because defective specimens would never be identified. A similar analogy exists in hydraulic conductivity assessment. Small laboratory test specimens will not reveal whether much higher K_F exists in the field, and thus have limited value in assessing hydraulic conductivity. This is clearly evident in the paper. For two of the pads, the small-scale laboratory tests yielded a false positive, which is unconservative. A similar conclusion can be drawn from the database compiled by Benson et al. (1998), as shown in Fig. 13.

The writers are also aware that some states require that small specimens collected from a single lift meet a maximum hydraulic conductivity requirement. However, most states that recognize such tests require that all specimens meet this requirement rather than the 95% confidence level suggested by Fuleihan and Wissa. Many states do not even recognize such test results, based on the overwhelming evidence against them in the literature. Fuleihan and Wissa also suggest that the arithmetic mean of these measurements is the appropriate average value for hydraulic conductivity. This may be true in some regulatory settings. However, the overall hydraulic conductivity of a liner is closely yet conservatively represented by the geometric mean hydraulic conductivity (Boutwell and Rauser 1990; Benson et al. 1994b). The overall hydraulic conductivity of the liner governs its performance, not the arithmetic mean of point measurements of hydraulic conductivity made within each lift. Hopefully, engineers are more concerned about the performance of the liner and the fate of the environment than the requirements in a regulation.

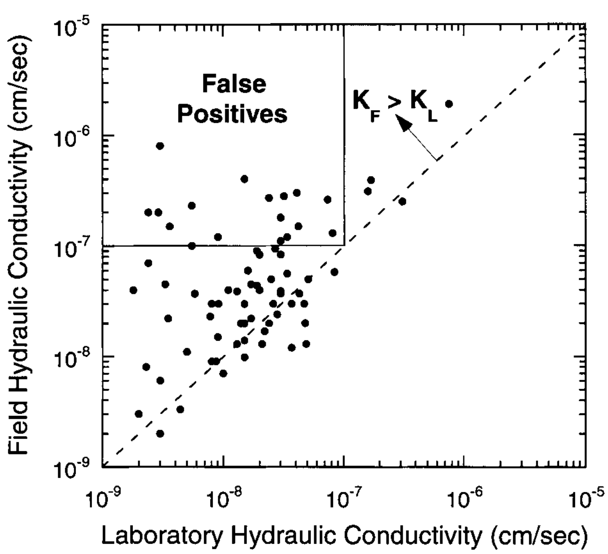

FIG. 13. Field Hydraulic Conductivity versus Hydraulic Conductivity Measured in Laboratory on Small Specimens (Adapted from Benson et al. 1998)

Finally, Fuleihan and Wissa indicate that a larger number of small-scale samples for laboratory hydraulic conductivity testing would have been collected had the test pads been constructed in Florida. This appears to be the case. However, the testing frequencies apparently used in Florida are atypical of U.S. practice. For example, Louisiana requires half the cited frequency, and as previously mentioned, many states do not recognize test results from small-scale specimens. Of the 85 test pads included in the nationwide database compiled by Benson et al. (1998), 4.4 samples were collected per test pad on average for small-scale hydraulic conducting testing in the laboratory, even though many of the test pads were sampled at a frequency greater than that required by regulations for full-scale construction. Moreover, when the large database is considered, there is no doubt that conclusions based on laboratory testing of small specimens can be unconservative (Fig. 13). Even if many more small specimens are collected and tested, the same incorrect conclusion can be drawn. If macroscopic defects exist, the hydraulic conductivity will be underestimated in each specimen because each is too small to capture the network of pores controlling flow in the field. In fact, tradition is the primary reason why small-scale tests are conducted today. If such tests were proposed today for use in quality control testing, there is little likelihood they would pass a critical review and be accepted as standard practice.

Thus, the writers stand by their conclusion that small specimens commonly collected for hydraulic conductivity testing in the laboratory have little value for hydraulic conductivity assessment of clay liners. The most likely causes for high K_F are changes in material type and deviations from construction specifications. The resources allocated to small-scale hydraulic conductivity testing would be far better spent on additional testing prior to construction to identify appropriate compaction conditions and for greater control of materials and compaction during construction.

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Assessment of Liquefaction Potential during Earthquakes by Arias Intensity[a]

Discussion by D. R. Phatak[3] and S. R. Pathak[4]

The authors' work on the Arias Intensity concept for determining liquefaction potential during earthquakes is commendable. It is gratifying to know that the model suggested by the discussers also leads to the same results about the liquefaction-prone zones, thus establishing the logical consistency of the model suggested by them. From the data of the Niigata earthquake (1964), as given by the authors, the discussers considered a few sample cases (Table 1) for determination of the sites prone to liquefaction (Fig. 8).

For this purpose, the following steps are carried out:

1. The corrected CPT resistance (q_{c1}) values converted to the seismic shear stress ratio (SSR) (Stark and Olson 1995).
2. The corrected SPT blow count values $(N_1)_{60}$ are obtained from SSR (Stark and Olson 1995).
3. From these $(N_1)_{60}$ values, the raw SPT blow count (N) values are determined (Seed et al. 1985).
4. The corrected SPT blow count ($\bar{N}$) values are calculated using overburden correction factor (C_N) from Teng's equation.
5. Liquefaction potential values are obtained using model "A" suggested by discussers from overburden stress values (Stark and Olson 1995).

It is found that the discussers' model leads to the same observations of "yes" liquefaction (for all cases considered herein) as that of the authors of the paper. Thus the discussers'

[a]December 1997, Vol. 123, No. 12, by Robert E. Kayen and James K. Mitchell (Paper 13983).

[3]Prof., Dept. of Civ. Engrg., Govt. College of Engrg., Pune, India.
[4]Prof., Dept. of Civ. Engrg., Govt. College of Engrg., Pune, India.

TABLE 1. Samples from Niigata Earthquake Data 1964

Sample case (1)	Site (2)	Depth (m) (3)	Effective vertical stress (σ_0) (KPa) (4)	q_{c1} (MPa) (5)	Seismic shear stress ratio (SSR) (6)	$(N_1)_{60}$ (7)	$\bar{N}$ (8)	Model "A" LP = $\frac{10^{1.5M}}{\sigma_0^{1.5} \cdot r^2}$ (9)	Possibility of liquefaction assessed from Fig. 8 (10)
1	Kwagishi-Cho	2.8	35.3	5.0	0.0826	7.24	12.067	0.167	Yes
2	Kwagishi-Cho	4.6	51.0	—	—	9.0	15.00	0.096	Yes
3	Kwagishi-Cho	5.2	56.9	9.3	0.156	14.48	24.13	0.0815	Yes
4	Kwagishi-Cho	8.0	81.4	6.2	0.104	9.3	15.5	0.04786	Yes
5	Kwagishi-Cho	4.8	61.8	6.8	0.115	10.17	16.95	0.072	Yes
6	Kwagishi-Cho	6.7	78.5	8.9	0.15	14.0	23.33	0.05	Yes

Note: M = 7.5, r = 71.28 km.

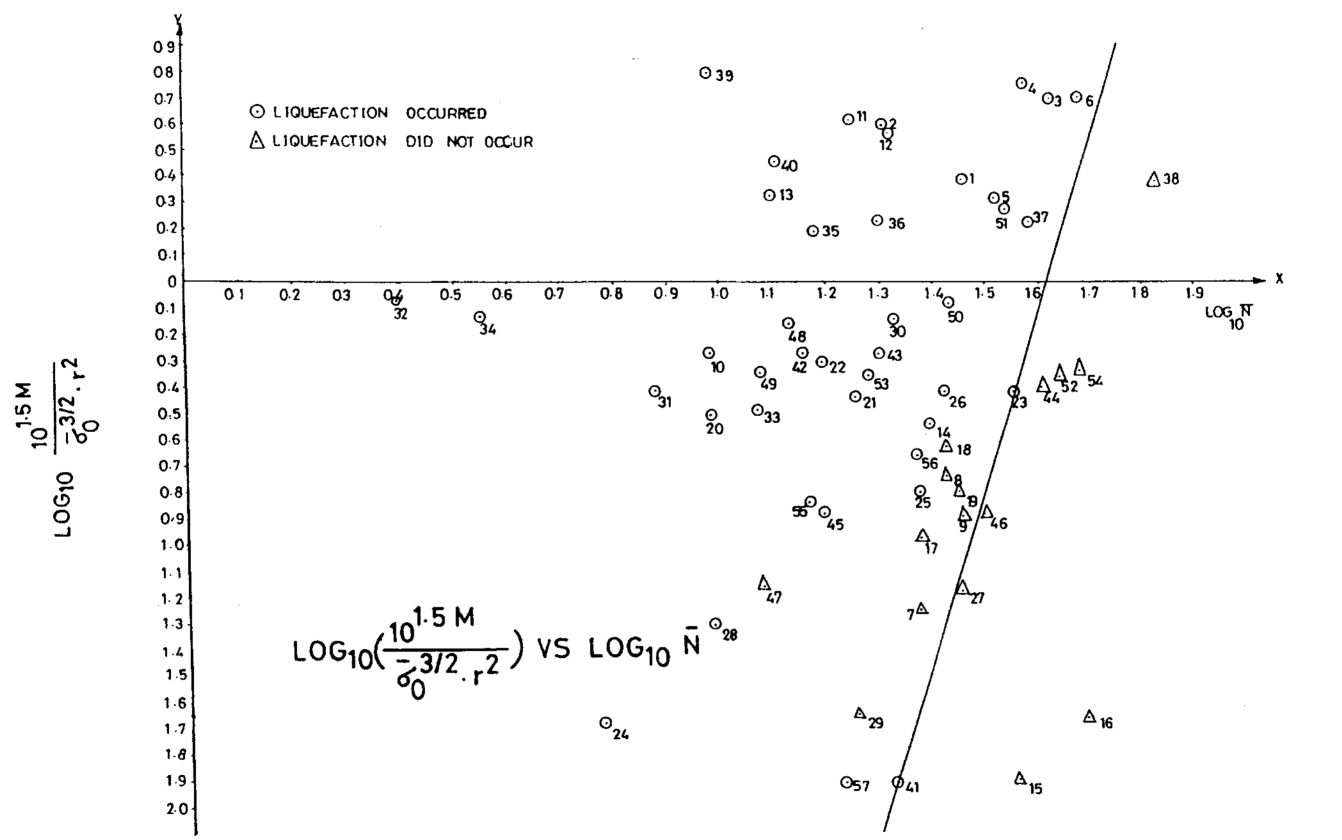

FIG. 8. Plot of Observed Data Showing Zones of Possible Liquefaction, and of No Liquefaction

model appears to be sound enough to identify the sites prone to liquefaction. Furthermore, it can be said that, without taking recourse to Arias Intensity concept, the possibility of liquefaction can also be determined by using the model suggested by the discussers.

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Discussion by M. D. Trifunac,[5] Member, ASCE

The authors propose to use the Arias Intensity for assessment of liquefaction potential of saturated cohesionless soils during earthquake shaking. Their basic assumption is that the occurrence of liquefaction can be related to pore water pressure rise, caused by seismic wave energy in the soil. This is closely related to several previous studies—He (1981), Law et al. (1990), Berrill and Davis (1985), Davis and Berrill (1982), and Trifunac (1995). This discussion comments on the authors' choice of energy-functional, boundary-separating occurrence of liquefaction from no occurrence, and on the effect of depth.

1. The Arias intensity I_h is, by definition, the normalized integral of particle acceleration of ground motion squared, $a^2(t)$, over the duration of strong shaking ($I_h \sim I_a \equiv \int_0^T a^2(t)\,dt$). It is a measure of the energy dissipated by viscous dashpots in a population of simple linear oscillators, with frequencies uniformly distributed between 0 and ∞, with unit mass, and excited by base acceleration $a(t)$. The authors acknowledge that I_h is *different from the energy of waves in the soil* but do not present *a physical justification* for their choice of I_h as a representative energy functional. The energy of wave motion in the soil is proportional to the integral of the particle velocity squared, $v^2(t)$, over the duration of strong motion ($I_v = \int_0^T v^2(t)\,dt$, Trifunac and Brady, 1975; Trifunac, 1995). The difference in using I_h or I_v becomes important when empirical scaling equations are used for various site-specification conditions, because I_h emphasizes higher frequencies while I_v is more representative of the intermediate and long periods. The difference between ''rock'' sites and ''alluvium'' sites [see (10) and (11) in the original paper], for $\log_{10} I_h$ is 0.2. This is consistent with 0.103×2 in Trifunac and Brady 1975. However, this difference for $\log_{10} I_v$ is ~ 0.7 (0.331×2, see Table 4 in Trifunac and Brady 1975). Does this mean that the authors wish to *emphasize the high frequency part* of strong motion in their empirical scaling equations? If there is some advantage in doing so it would be useful to clarify this point.
2. Computation of Arias intensity (and of the related integrals I_a and I_v) directly from recorded and processed accelerograms underestimates their true values. This is caused by band-pass filtering in data processing of digitized accelerograms (Trifunac 1971, 1972). The reduction is small for very strong ground motion, but becomes significant for intermediate, small, distant earthquakes (because of the small signal-to-noise ratio). This bias can be eliminated by extending the spectral amplitudes beyond the frequency band imposed by data processing (Trifunac 1993, 1994) and by evaluating these integrals in the frequency domain (e.g., see ''en'' model in Trifunac 1995).
3. The empirical boundary separating the liquefaction and no liquefaction (in Fig. 4) has not been defined, either by an equation or by a table. This makes it difficult to use this method and to compare it with other studies. This boundary is apparently meant to be used for $(N_1)_{60}$ greater than about 3 to 5? The threshold energy needed to trigger liquefaction depends on various site-specific variables. Is $I_h \sim 0.22$ m/s in Fig. 4, therefore, applicable just for this data set and for $(N_1)_{60} \gtrsim 5$?
4. The Arias intensity depth-of-burial reduction parameter, r_b, as used by the authors, appears to represent a weighted average of the amplitudes of one-dimensional standing waves associated with vertically incident shear waves. Since they used the Arias intensity as an ''energy based measure of earthquake shaking'' or as ''accelerogram energy,'' one can reach an erroneous conclusion that the energy of strong motion decreases with depth of burial. The flux rate of energy transmission for plane waves (across a unit area) is proportional to $\rho c v^2$, where ρ is the material density, c is the wave propagation velocity, and v the particle velocity. Assuming that there is negligible energy loss near the ground surface, it follows that the integral $\rho c \int_0^T v^2(t)\,dt$ (and also $\rho c \int_0^T a^2(t)\,dt) = \rho c I_a$) should be approximately constant for a shallow range of depths. Therefore I_a and I_h should diminish with depth proportional to $(\rho c)_{\text{surface}}/(\rho c)_{\text{depth}}$. Both ρ and c can be correlated with $(N_1)_{60}$. Consequently, even when ρ and c versus depth are not available, *site-specific depth corrections*, rather than r_b (based on the average trend for one-dimensional wave interference) should have been used in the development of the boundary curves separating the liquefaction and nonliquefaction cases (shown in Fig. 4).

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Closure by Robert E. Kayen,[6] Member, ASCE, and James K. Mitchell,[7] Honorary Member, ASCE

The writers appreciate the comments of the discussers on the assessment of liquefaction potential during earthquakes by

[5]Prof., Dept. of Civ. Engrg., Univ. of Southern California, Los Angeles, CA 90089-2531.

[6]Res. Civ. Engr., U.S. Geological Survey, 345 Middlefield Rd., Menlo Park, CA 94025. E-mail: rkayen@usgs.gov

[7]University Distinguished Prof., Virginia Tech., 109B Patton Hall, Blacksburg, VA 24061-0105. E-mail: jkm@vt.edu

Arias Intensity. It is encouraging that our findings compare favorably with both the "simplified" seismic-stress method, and with those of Phatak and Phatak for the Niigata sites, using seismic energy (Gutenberg-Richter relation) attenuated for geometric spreading.

It is the intention of the authors to develop an energy-based methodology for liquefaction assessment that accounts for (1) the local-duration of earthquake motion; (2) the frequency characteristics of motions imparting shear-stress on soil elements; and (3) local-site amplification affects. Foremost, we seek to develop a generalized energy-based procedure that gives results that compare favorably with the "simplified" seismic-shear stress approach of Seed and Idriss (1971), a well-established methodology in standard usage today. Indeed, we feel that for any energy-based methodology to be useful in standard geotechnical practice, it must be validated through comparison with the results of this verified, empirical stress-based procedure. For this reason, we have chosen to use the energy parameter *Arias Intensity,* I_h, because the acceleration time-history is directly related to the development of cyclic shear stresses within the soil column: Arias Intensity captures both the appropriate frequency content and durational aspects of the shear stress-time history. Together these quantities provide a direct measure of the input intensity at a point, and this intensity leads to shear deformation, soil structure breakdown, and liquefaction.

We agree with Trifunac that for intensities in a range *likely to trigger liquefaction* (double component $I_{hb} > 0.2$ m/s) the reduction in Arias intensity due to band-pass filter processing is negligible. We recomputed 135 uncorrected-horizontal accelerograms, spanning an intensity range of 0.1 m/s $< I_{a(\text{surface})} < 4.24$ m/s, to compare with values of corrected accelerograms. We found the reduction in Arias intensity due to processing for records capable of inducing liquefaction to be, on average, 4.9% across this range (Fig. 9). The problem of large processing reduction in I_h, due to low signal-to-noise in the accelerogram records of small or distant earthquakes, is not germane to the liquefaction problem. For liquefaction to occur, a threshold level of strong shaking is needed to trigger pore-pressure generation in even the loosest soils.

What is important here is that the liquefaction assessment methodology (based on either I_h or PGA) standardize on the use of either uncorrected or corrected accelerograms. For the purposes of standardization, we chose to use processed accelerogram records. Our dataset of 81 sites in Japan and the United States leads us to conclude that a threshold level of intensity of $I_{hb} \sim 0.22$ m/s is needed to trigger liquefaction. Certainly, additional data will help to better define the boundary at low, and high, intensities. Trifunac apparently is concerned that there are no $(N_1)_{60}$ data points < 3 defining that boundary. It should be noted that following the NCEER guidelines (Youd and Idriss 1998) for standardizing SPT data, there are no fines-content corrected SPT data points in our catalog, or that of Seed et al. (1984), that fall below 3.

We disagree with Trifunac's suggestion that depth-reduction coefficients be developed for sites using SPT data. This approach is inconsistent with standard practice for determining depth-reduction coefficients for both Arias intensity and seismic-shear stress, and cannot account for the important influence of input motion on the shape of the depth-coefficient curve. Recent studies of the variation of strong motion in the soil column by Golesorkhi (1989), Idriss (1997), Idriss and Golesorkhi (in preparation), and Kayen and Mitchell (1998) indicate that the shapes of the shear stress and Arias intensity coefficient curves are significantly controlled by the predominant period of the input-motion. Relations between predominant period and moment magnitude (Rathje et al., 1996), M_w, allow for the expression of r_b as a function of depth, z, and M_w. For the Arias intensity coefficient curve, Kayen and Mitchell (1998) proposed the following equation to describe r_b in the upper 20 m of soil (Fig. 10):

$$r_b = \exp\left(\frac{35}{M_w^2} \cdot \sin(-0.09 \cdot z)\right) \quad (1)$$

Idriss (1997) has also proposed an equation describing the shear-stress coefficient, r_d, as a function of moment magnitude

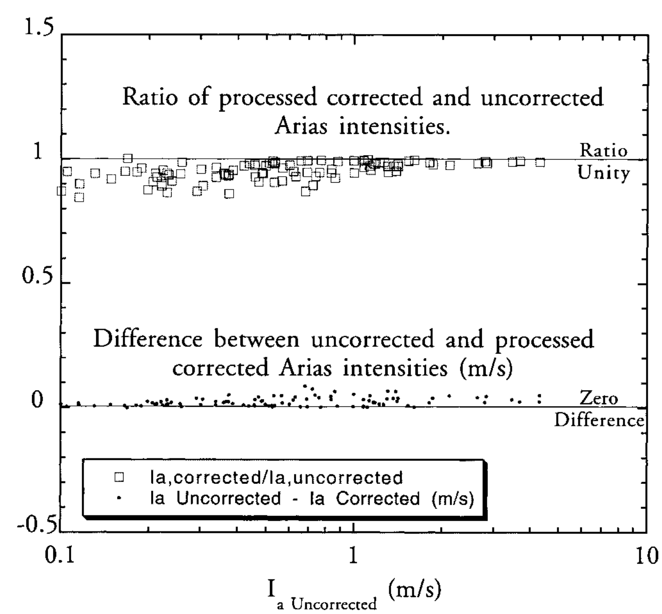

FIG. 9. Comparison of Uncorrected and Corrected Arias Intensities Expressed as Ratio (Dimensionless) and Difference (m/s), Plotted against Computed Uncorrected Arias Intensity

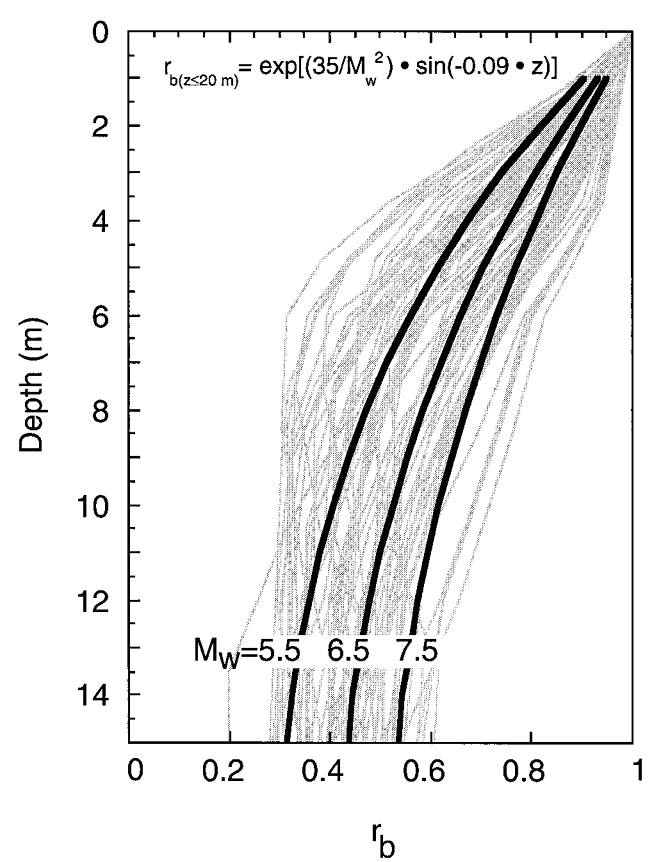

FIG. 10. Design Equation for Surface-Arias Intensity Depth-Coefficient r_b Overlaying SHAKE Results (Kayen and Mitchell 1998)

and depth. Where shear modulus data are available, and design considerations warrant it, site-specific r_b profiles can be developed using appropriate design-basis ground motions and numerical site response code. Otherwise, we recommend using (1).

Seismic energy-methodologies for liquefaction assessment based on the Gutenberg-Richter relation for energy released from an earthquake (Trifunac 1995; Davis and Berrill 1982; Berrill and Davis 1985; Law et al. 1990) are fundamentally hampered by an inability to account for the effects of local soil amplification on ground motion and the nonverifiable nature of the attenuated energy. The source-distance parameter typically used to attenuate these models, hypocentral distance, breaks down in the range of earthquake magnitudes likely to cause liquefaction ($M > 5$). An example of this is the Salinas site that liquefied during the 1906 San Francisco earthquake (site 14, Davis and Berrill 1982). This site has a computed hypocentral distance of 184 km but is actually situated only 24 km from the closest distance to the fault rupture plane. These limitations lead to *log-log* plots of an energy functional versus N_1 that have poorly segregated overlapping clouds of data points representing liquefaction and nonliquefaction sites. The use of Arias intensity negates these limitations, as the functional parameter is measured locally. A seismic-energy methodology for liquefaction assessment based on record velocity-time history (Trifunac 1995) is promising, as it (like Arias intensity) reduces the uncertainties associated with the estimation of the local severity of ground motions during earthquakes.

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SOIL-WATER CHARACTERISTIC CURVES FOR COMPACTED CLAYS[a]

Discussion by E. C. Leong[4] and H. Rahardjo[5]

The authors have presented an interesting paper describing soil-water characteristic curves for four compacted clay barrier

[a]November 1997, Vol. 123, No. 11, by James M. Tinjum, Craig H. Benson, and Lisa R. Blotz (Paper 14538).

[4]Sr. Lect., NTU-PWD Geotech. Res. Ctr., Nanyang Technol. Univ., School of Civ. and Struct. Engrg., Nanyang Ave., Singapore 639798.

[5]Dir., NTU-PWD Geotech. Res. Ctr., Nanyang Technol. Univ., School of Civ. and Struct. Engrg., Nanyang Ave., Singapore.

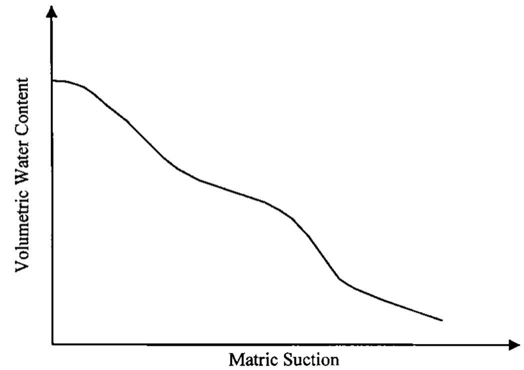

FIG. 15. Plausible Soil-Water Characteristic Curve for Soil Specimen with Bimodal Pore-Size Distribution

soils, and regression equations to estimate the parameters α and n in the van Genuchten soil-water characteristic curve equation. The authors have used Brooks-Corey and van Genuchten equations to fit the soil-water characteristic data. The discussers would like to know if they have attempted to use the Fredlund and Xing (1994) equation in their exercise. Leong and Rahardjo (1997) have shown that the popular soil-water characteristic curve equations can be derived from the following generic equation:

$$a_1\Theta^{b_1} + a_2 \exp(a_3\Theta^{b_1}) = a_4\psi^{b_2} + a_5 \exp(a_6\psi^{b_2}) + a_7 \qquad (5)$$

where a_1, a_2, a_3, a_4, a_5, a_6, a_7, b_1, and b_2 are constants; ψ = suction pressure; and Θ = normalized volumetric water content. The Brooks-Corey and van Genuchten equations can be derived from (5). These two equations are empirical in nature. Therefore, λ and Ψ_a in Brooks-Corey equation and α and n in van Genuchten equation as denoted by the authors should be treated as curve-fitting constants. In this case, could the authors clarify the use of Fig. 7 in the paper, which compares one set of fitting constants to another set.

The authors have also suggested that soil specimens compacted at water content dry of optimum contain a bimodal pore-size distribution, and that specimens compacted at water content wet of optimum contain a unimodal pore-size distribution. For a specimen with a bimodal pore-size distribution, it seems plausible that the specimen may exhibit two air-entry values, suggesting the shape of its soil-water characteristic curve to be as illustrated in Fig. 15. The discussers are interested to know if soil-water characteristic curves of the shape shown in Fig. 15 were observed by the authors in soil specimens compacted dry of optimum.

The authors have also presented regression equations to estimate the α and n in the van Genuchten equation from plasticity index (PI), compaction water content relative to optimum water content ($w - w_{\mathrm{opt}}$), and categorical variable for compactive effort (C). Have the authors considered the use of relative compaction and ratio of compaction water content to optimum water content (w/w_{opt}) in their regression analysis? The unit of α in equation (3) should be kPa^{-1} instead of kPa. The discussers would also like to suggest that α in (3) be recast as $p_a\alpha$, where p_a is atmospheric pressure. This will make (3) dimensionless.

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