

Theme Report on Topic 6  
DYNAMICS OF SOILS AND SOIL STRUCTURES

by

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INTRODUCTION

In preparing my report on the contributions to this session of the 6th International Conference on Earthquake Engineering, I reviewed a total of fifty-eight papers, of which twenty-one were of the one page or abstract variety. It seemed to me that any evaluation of the contributions in the abstracts would be very superficial and of limited value. Therefore, I have confined my report to the thirty-seven full length papers.

It is not possible to present a detailed analysis of each paper within the imposed limitations of space and time. Therefore, I shall highlight those contributions which I think have the greatest potential for expanding our understanding of soil dynamics or for increasing our capability for performing useful dynamic response analyses. My judgement in these matters is obviously influenced by my own interests and experience in the field of soil dynamics. I expect some challenges to these judgements and I hope that such challenges will stimulate interesting and fruitful discussions.

The thirty-seven papers range over a wide variety of topics and to induce some coherence into my report and ensuing discussion, I have classified the papers into three categories and offer a critical review of each category.

DYNAMIC PROPERTIES OF SOILS

We have many methods at our disposal for the dynamic response analysis of soil deposits and soil structures. Unfortunately, our analytical capability far outstrips our ability to describe the stress-strain behaviour of the soil. Only for one-dimensional behaviour, as exemplified by horizontally uniform deposits on level ground shaken by vertically propagating shear waves, can we claim to have developed rational stress-strain laws. For this case, we can directly model the gross field behaviour in a cyclic simple shear or shake table test. In the case of two-dimensional and three-dimensional problems, we can only model a limited number of the stress paths that are possible in the field and, since soil is a non-linear material, we can take the remaining stress-paths into account only by making simplifying assumptions about the mechanical nature of the soil, assumptions which may or may not fit the realities of the problem under consideration.

Even our ability to model one-dimensional shear behaviour may be quite limited in certain cases. Unless we can create in a laboratory soil sample all the characteristics of the soil in the ground the dynamic response of the two will not be the same. The soil in the ground has been subjected to a complex geological history which has created a particular structure and

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stress state in the soil which is almost impossible to duplicate in the laboratory. Furthermore the condition of the soil is influenced by certain time-dependent phenomena such as cementation and consolidation which are even more difficult to duplicate in the laboratory. Therefore reconstituted laboratory samples can be expected to represent in an adequate manner only the youngest and simplest of natural deposits, deposits such as shallow deltaic and fluvial sands. Evidence of this has been presented in paper (6-26)\* by Iwasaki and Tatsuoka who compared the results of laboratory measurements of shear modulus by the resonant column method with values of the modulus obtained from shear wave velocity measurements in the field. In both of these tests the modulus may be measured in the same strain range ( $10^{-6}$  -  $10^{-4}$ ). The investigations were carried out at two sites which had been reclaimed by deposition of sand. The sands were clean and had been recently deposited and, therefore, the effects of cementation, consolidation and geological processing might be expected to be minimal. For each site the agreement between the values of the shear modulus predicted by the laboratory tests agreed satisfactorily with those calculated from shear wave velocities measured in the field.

Geological field evidence of the effect of the age of a sand deposit on its dynamic response has been presented in Youd and Hoose (6-7). From an analysis of reports on earthquake damage, they concluded that shallow, saturated, Holocene, fluvial, deltaic, and aeolian deposits have the highest susceptibility to liquefaction and ground failure during earthquakes; the older Pleistocene sand deposits are generally less susceptible and pre-Pleistocene deposits are usually immune to liquefaction.

It might be thought that the difficulties associated with reproducing field conditions in reconstituted samples could be avoided by taking undisturbed samples. Many features of the soil in the ground are preserved by this method but the natural stress conditions are always altered during sampling and preparation for testing, and some sample disturbance always occurs. Furthermore, except in relatively young deposits, the stress conditions in the ground cannot usually be estimated accurately and therefore cannot be recreated in the laboratory test.

A very thorough analysis of all the difficulties associated with obtaining laboratory data that might be reasonably representative of field conditions has been presented by Seed (1976).

The best way of avoiding the limitations associated with laboratory testing is to measure the necessary soil properties in the field. Geophysical field measurements have been used for many years to measure the shear wave velocity from which the shear modulus may be computed. This field modulus is associated with strains less than  $10^{-5}$  and is usually considered to be the maximum value of the shear modulus. Earthquake motions of interest to the geotechnical engineer commonly generate strains between  $10^{-5}$  and  $10^{-3}$ . Until recently, it has been necessary to measure moduli in this range of strains by laboratory tests of various kinds; resonant column, torsional shear, simple shear, and triaxial tests. In 1974, an in-situ

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The numbers in brackets are the numerical designations for papers in the preprint volume. The first numeral refers to the theme and the second set refers to the serial number of the paper in the theme.

impulse test for the measurement of shear moduli at strains in the range associated with strong earthquakes was developed (SW-AA). Descriptions of the equipment and testing procedures have been presented by Miller et al. (1975).

The emergence of the capability to measure strain dependent moduli in the field is, in my opinion, the most important development in the field of soil dynamics since the last International Conference on Earthquake Engineering at Rome in 1973. The impact of this development will not be felt until sufficient field data has been accumulated to allow us to assess the quality of the data we currently obtain by tests on reconstituted and undisturbed samples.

A number of papers in this section of the conference deal with the in-situ measurement of the dynamic properties of soils. They contain important information and ideas, and would repay close attention by all interested in soil dynamics. I devote the greater part of my attention to these papers.

Troncoso et al. (6-28), describe a modified cross-hole impulse test for the measurement of in-situ strain dependent moduli. A cylindrical anchor is bonded by radial pressure to the sides of a vertical borehole. When the anchor is driven vertically by an impact hammer falling on a spring device, it generates vertically polarized shear waves, and the velocities of these waves are measured at the same level but at different distances in other boreholes, as the waves propagate radially from the source. Since the modulus  $G$  is related to the shear wave velocity  $V_s$  by  $G = \rho V_s^2$ , in which  $\rho$  is the density, the velocity, like the modulus, is strain dependent. Therefore, the shear wave velocity increases with distance from the source as the shear strains become smaller. The shear strain  $\gamma$  is estimated by  $\gamma = \dot{w}/V_s$  in which  $\dot{w}$  is the particle velocity. Therefore, both shear strains and shear moduli are determined by measuring particle velocities and shear velocities at receiving stations positioned at different distances in the soil mass. Data are presented on shear moduli at various strains levels measured in-situ in a compacted silty sand and in laboratory tests of the same material. The results are in close agreement. It should be noted that using compacted fill as the field material removes most of the difficulties associated with natural deposits and makes a comparison with laboratory data meaningful.

A number of important questions come to mind in attempting to evaluate this in-situ test procedure. How difficult and time-consuming is it to run a reliable test? What difficulties arise in interpreting the recorded data? Can the signal be unambiguously separated from the noise on the recordings without resorting to complicated signal processing? How reliable is the data at the low strain level when the signal to noise ratio (noise primarily due to microtremors) may be quite low? How reliable are the particle velocity measurements and how are they obtained? After the signals have been processed what techniques of data reduction are used to get the particle and wave velocities? How sensitive are the results to techniques of data reduction?

The techniques of data reduction are discussed in a paper by Werner and Van Dillen (6-24). Numerous special in-situ tests were carried out to test the repeatability of the data and the sensitivity of the soil response

measurements to the effects of various test parameters. These special tests were then analysed by the finite element method using soil properties determined in cyclic triaxial tests. The node points of the finite element grid were treated as if they were sensors in the field and the field data-processing techniques were applied to the node-point velocity histories to estimate the shear moduli and strains in the soil. The computed shear moduli were compared to the input moduli and the measure of agreement was taken to be indicative of the effectiveness of the data processing technique.

Reasonable agreement was found when the finite element results were adjusted to account for the frequency response characteristics of the sensors. It is not clear from the paper how much this adjustment depended on the judgement of the authors and how much was determined unambiguously by the mechanical properties of the system. This is a point on which further elaboration would be very welcome.

The authors describe some tests to determine the effect of prior loading and conclude that these effects were generally slight and noticeable only after the initial positive velocity pulse had passed. The negative velocity pulse is as large as the positive and differences of over 50% are noticeable due to prior loading - a not inconsequential difference. Other investigators (Finn et al. 1970, Seed et al. 1975a) have found that prior seismic loading has a profound effect on seismic response under undrained conditions. Generally, prior seismic loading which induces small strains increases the in-situ shear modulus and increases the resistance to liquefaction. The beneficial effect of prior loading by smaller summer waves is now being taken into account in the foundation design of offshore gravity structures in the North Sea to resist storm wave loadings.

An excellent insight into the problems associated with the processing of the recorded signals of waves in the ground is provided in an extremely useful and illuminating paper by Goto et al. (6-29). They describe the measurement of shear wave velocity in the ground at small strains using the standard penetration test as the source of wave energy. The wave motion is recorded at the ground surface. As the penetration of the sampler increases, the strength of the recorded signal begins to drop and at a depth of about 20 metres the signal cannot be separated from the background noise of micro-tremors and drill-rig without resorting to signal enhancement. In the case of the standard penetration test in moderately strong soils a rather simple procedure exists for minimizing the effects of random noise. In such soils, there will be a significant number of blows to drive the hammer about a metre. In this case, we can reasonably assume that the signal from each hammer blow is the same. Therefore if we digitally stack the records from each blow the effects of the random noises in each record tend to cancel out and the signal will be enhanced. Goto et al. show some very clear examples of the effects of digital stacking on clarifying the wave signal.

Troncoso et al. also used the standard penetration test to generate the wave energy for their in-situ procedure and obtained results comparable to that using the anchor source. Since they measured the wave signals in adjacent boreholes, and not at the ground surface, it would be interesting to hear what difficulties in signal processing were caused by environmental noise.

A different approach to in-situ testing is described by Esashi, Yoshida, and Nishi (6-25) who use a modified form of the Wroth self-drilling pressuremeter to generate a wide range of radial strains at a specific location in a borehole. As the length of the loading section is long compared to its diameter, the pressuremeter is considered to generate a two-dimensional strain state describable in polar coordinates. The radial strain and stress amplitudes at the borehole are controllable and create, in effect, an in-situ cyclic loading test which yields the damping ratio, dynamic Young's modulus, and dynamic lateral force coefficient as functions of strain. It would appear that in this method the problems of data reduction are greatly minimized and there is greater certainty about what is actually measured. What I cannot comment on, but would like to have discussed later, is how this method compares with that of the shear wave method with respect to the difficulties of performing the test and the level of skill and sophistication required to obtain reliable results.

Richart, Anderson, and Stokoe (6-27) present an intriguingly simple procedure for determining soil properties in the ground from laboratory test data. They suggest first determining the shear modulus - shear strain relationship in the laboratory for the range of interest. Then the in-situ shear modulus at small strains is determined by geophysical methods as  $G_{\max}$  (field). If the difference  $G_{\max}$  (field) -  $G_{\max}$  (laboratory) =  $A_r$ , then the shear modulus at any strain level in the field is given by

$$G(\text{field}) = G(\text{laboratory}) + A_r$$

Now that we can measure the strain dependent moduli in the field, it would seem very useful to test whether this simple procedure for determining field moduli gives results which are accurate enough for engineering purposes. It would be interesting to hear whether, in fact, such an evaluation has been carried out already or whether there are any plans for so doing.

In addition to needing good data on stress-strain relations, our methods of dynamic analysis are also sorely in need of a fully monitored field check. In recent years, major field studies such as the Rio Blanco event (Banister et al. 1975) have been carried out with simulated earthquake motion. The University of New Mexico has played a major role in the simulation of such motions by high explosives. The paper by Higgins and Triandafilidis (2-27) sets out very clearly the criteria for adequate simulation and the techniques for the enhancement of the simulated motions.

Sharma and Wang (6-12) present a description of the kinds of tests that may be used to determine the elastic, viscoelastic and viscoplastic response of soils. The results presented are, as usual, based on one-dimensional tests, and their applicability to two and three-dimensional problems are subject to the usual simplifying assumptions.

Chae and Au (6-30) present data on the dynamic properties of expansive soils treated with lime, salt, and a lime-salt combination. They show that such treatments are very effective in increasing the rigidity and energy dissipation characteristics of the soils.

In a new approach to modelling soils Hill, Lee, and Udawadia (6-33) present a method for the non-parametric modelling of non-linear soils based on Norbert Wiener's general theory of non-linear system identification. The

method is based on determining the functional responses between input to a soil system and the output. No a priori information whatsoever is specified regarding the soil. Mathematically, the relationship between input and output is specified by a series of kernel functions.

The method for evaluating the kernels is not that proposed by Wiener but is due to Lee and Schetzen (1965) who developed a procedure using correlation techniques. The theoretical and computational basis of the method is mathematically secure. However, the usefulness and applicability of any method of system identification cannot be judged on just mathematical grounds; system validation is also required. Since no data is given a specific judgement on the method is not possible.

Some general comments can be made based on accumulated experience with non-linear distributed systems. For many practical cases, the number of parameters (sample values) needed to define the kernels is too large. Eykhoff (1974) shows that if even a very coarse representation is made of the first kernel by 10 sample values, then 55 sample values would be required to get equal resolution of the second kernel and 220 sample values would be required for the third kernel. The number of sample values increases dramatically with kernel order. Consequently parametric models to which physical insight can make a contribution are generally preferred to the non-parametric model suggested by the authors.

One technical objection may be made to the authors' model. Soil in the non-linear range of behaviour is a hysteretic material. Such materials have an infinite memory. The authors' model has a finite memory and from the purely theoretical point of view would not seem appropriate for soil. I do not know how important this limitation may be practically.

Gurpinar (1976) has applied system identification techniques to earthquake data measured in the San Francisco area. He compared input motions to soil layers at bedrock level with output motions measured in the basements of buildings and determined various frequency response functions. The utility of these functions is somewhat in question as the coherence functions between input and output fluctuate rather widely. Part of this fluctuation is undoubtedly due to imprecise definition of the system in the field and all the inputs and outputs of that system. This is likely to be a difficult problem with distributed parameter systems like soil foundations and may preclude the successful use of non-parametric methods of system identification in field problems in earthquake engineering.

#### LIQUEFACTION

I have already referred to Youd's paper which concludes that the liquefaction potential of sand deposits appears to decrease with increasing geological age. This conclusion receives further support from the data presented by Kuribayashi, Iwasaki, and Tatsuoka (7-6). After analysing the incidence of liquefaction during forty-four (44) earthquakes in Japan since 1872, they found that liquefied sites were limited to alluvial deposits and reclaimed land along seas or lakes.

There have been many attempts to characterize simply and empirically those sites which might liquefy during an earthquake using liquefaction data from previous earthquakes (Ohsaki, 1966; Koizumi, 1966; Kishida, 1966;

Seed and Peacock, 1971; Castro, 1975; Christian et al., 1975). The condition of the site is often described by standard penetration test data or by relative density measurements, and the intensity of the earthquake motion by the maximum acceleration or the average shear stress which is derived from the maximum acceleration (Seed and Idriss, 1971). There is considerable uncertainty about the magnitude of the acceleration that may be expected at a given site during an earthquake of given magnitude, and other less variable indices of earthquake intensity at a site have been sought. Kuribayashi et al. index the liquefaction potential of a site in terms of the characteristics of the earthquake rather than of the site. They present data linking the magnitudes (M) of earthquakes and distances (R) from the epicentres to the most distant liquefied sites. They obtained a lower-bound line to this data in the form

$$\log_{10} R = 0.77 M - 3.6$$

in which R is in kilometers and M is greater than 6.0. They suggest that liquefaction may occur in alluvial or reclaimed deposits only inside an area with radius R for a given magnitude. It is interesting to note that for M = 6, 7, and 8, the values of R are 10, 60, and 360 kilometers, respectively.

Yegian and Whitman (7-5) place the problem of liquefaction in the context of overall seismic risk analysis and attempt to assess the probability of foundation failure due to liquefaction. In their risk analysis, they also characterize the intensity of the earthquake by parameters related to the earthquake rather than the maximum acceleration of the site. They choose the magnitude M also but, unlike Kuribayashi, select the hypocentral distance rather than the epicentral distance. The hypocentral distance appears to be more appropriate at closer distances, especially for deep earthquakes. For shallow earthquakes and at greater distances, the two measures of distance are quite similar.

The authors claim that the method "avoids the unnecessary use of an acceleration attenuation law which introduces great uncertainties". Instead, they use a parameter  $S_c$  which is proportional to the maximum shear stress. This parameter has a structure that appears very similar to an attenuation law. It would seem that any parameter proportional to average shear stress, which, in turn, is proportional to average acceleration, must somehow or other be dependent on the attenuation law for acceleration. There is not sufficient detail in the paper to allow me to clarify this point and a further amplification by the authors would be welcome.

A relationship is given between  $S_c$  and the corrected standard penetration test value  $N'$  which separates sites which are likely to liquefy from those which are not. What is unique about this criterion for assessing liquefaction potential is that quantitative measures of uncertainty are associated with the criterion itself and with the parameters which define it.

Another method of probabilistic analysis is presented by Chou and Oguntala (6-26) which attempts to determine the conditional probability of liquefaction occurring during an earthquake. Rather simple probability functions are presented for the various significant variables affecting

liquefaction. Only a bare outline of the structure of the method is given; there are no examples of its application to sites. It is not possible to express an opinion on its effectiveness in predicting liquefaction potential.

Work still continues on the fundamentals of liquefaction under laboratory conditions. Gupta and Prakash (6-4) found that sand with a relative density of 65% did not liquefy in shake table tests. This agrees with recent results by Seed (1976), who found that the resistance to liquefaction increased very steeply around a relative density of about 70%.

Two new tests for measuring liquefaction are described; Wolfe, Annaki and Lee (6-01) describe a cubic cyclic stress test based on the apparatus developed by Ko and Scott (1967). In all but one of its possible loading modes, the test gave results which were somewhat similar to triaxial test data; in one mode the results were quite different.

Comparisons of cyclic test data from various types of tests have usually been made without taking into account the different magnitudes of compliance in the various test systems. Compliance has the same effect as partial drainage and always leads to increased resistance to liquefaction. A recent analytical and experimental study of the effect of compliance by Martin, Finn, and Seed (1976) has shown that, with common sample dimensions, errors of the order of 50% can be caused by compliance in determining the cyclic stress levels causing liquefaction in a given number of cycles in triaxial tests. The magnitude of the error depends on the type of equipment used, the grain size of the particles, and the size of the samples.

In an effort to measure liquefaction resistance at as close to zero compliance as possible, Finn and Vaid (6-02) developed a form of constant volume simple shear test. In this test, a vertical stress is applied to either a saturated or dry sand sample. The loading head is locked and the sample cycled under drained conditions. The progressive reduction of vertical pressure on the loading head during cycling is equivalent to the increase in pore-water pressure in the corresponding undrained cyclic loading test. The compliance in the system is very low and due only to the recovery of deformation from the loading system as the pressure comes off the sand. This compliance is approximately 1/20 of the compliance measured in the system during the usual undrained cyclic loading tests. In addition to low compliance, the new test is free from the difficult and time-consuming features of undrained cyclic tests and gives results with extraordinarily high reproduceability.

Sherif, Ishibashi, and Tsuchiya (6-05) present very useful data on the effect of values of the pore-pressure coefficient  $B$  on soil liquefaction potential which allows one to estimate the increase in liquefaction resistance due to partial saturation in the field.

There has been continuing discussion over the years among soil engineers about our practice of representing earthquake motions by an equivalent number of uniform cycles. There seems to be agreement that, for practical purposes, this approximation is useful for conditions of level ground. We have also been using the simplification for slopes. The biased character

of the stress distribution in slopes makes it more likely that the order of application and the direction of the actual loading pulses may be important and that, for some problems, the use of equivalent uniform cycles may not be adequate. Ishihara and Takatsu (6-Q3) present data which shows that, under conditions of pre-existing static shear stresses as in a slope, the direction of loading by significant pulses in a stress-history may be important.

There are two interesting complementary contributions to the theme of liquefaction. The first by Bhandari (6-11) discusses the improvement by densification of the resistance of the ground under oil tanks to liquefaction. This procedure raises the interesting question - is the resistance of a column of soil to liquefaction increased by densification if the column is surrounded by looser soil which will liquefy. The field evidence from the performance of oil tank foundations at Hachinoe during the Toka-Chioki earthquake of 1968 clearly indicates that, despite being surrounded with loose sand, adequately densified foundations of oil tanks will not liquefy.

Nandakumaran, Bhargava, and Mukerjee (6-10) present a method for analysing this problem. The method, as they state, is very conservative and predicts increases in pore-water pressures in the densified soil due to the liquefied soil surrounding it, which are much higher than those measured in shake table tests, although the computed values show the same general trend as the measured values.

The papers on liquefaction give the impression that there have been no radical developments in the field of liquefaction studies since the Rome conference. This is true, in the main. Certainly, there do not seem to be any seminal ideas which might lead to significant, new developments. However, some very important developments of an evolutionary nature have occurred; a clearer picture of the fundamentals of the liquefaction process has emerged, (Martin, Finn and Seed, 1975; Bazant and Krizek, 1976). The effects of multi-directional shaking have been quantified (Seed et al., 1975) and various approaches to dynamic effective stress analysis of saturated deposits have been developed (Finn et al., 1976a,b,c; Seed et al., 1976; Streeter et al., 1974). It is rather ironic that concurrently with our greatly expanded capacity for rational analysis, serious doubts should arise about the relevance of our experimental laboratory data to field problems because of the difficulty of obtaining representative samples. I am looking to the in-situ methods presented at this conference to help to resolve this dilemma for us.

#### DYNAMIC RESPONSE OF SOIL-STRUCTURES

Five papers deal with various aspects of the seismic response of earth dams. Takahashi et al. (6-15) present an interesting report on the dynamic behaviour of some high rockfill dams recently constructed in Japan on which earthquake observations and field vibration tests have been carried out. The maximum crest acceleration recorded was about  $0.1g$  and maximum strains were in the range  $10^{-4}$  to  $10^{-3}$ . At these strain levels, the equivalent viscous damping factor in the first mode was in the range of 5-6 per cent which is consistent with laboratory test data, and the response of the dam was linear elastic.

The first mode of vibration was predominant in the response of each dam to earthquake motions and its shape was strongly influenced by the shape of the valley in which the dam was situated. The first mode of the Tataragi dam which is situated in a U-shaped valley is a straight line from foundation to crest, but in the other dams which were in V-shaped valleys the shape of the first mode was strongly curved in the upper levels of the dam. This indicated a relatively greater participation of this section of the dam in the vibration response than would be expected from the usual two-dimensional plane-strain analyses. Moss and Carr (6-21) have examined the effect of valley shape on the dynamic response of the valley itself.

At the low levels of excitation to which the dams were subjected, dynamic amplification ratios of up to 10 were measured between accelerations at the base and the crest. Watanabe (6-13), on the basis of tests on dam models on shaking tables and accelerograms recorded on 14 earth fill dams, estimates that when the base acceleration of the dam is about 0.2g the amplification factor at the crest is between 1.6 and 2.0; the lower figure applying to rock fill dams. The differences between the amplification factors of Watanabe and Takahashi are reconcilable; the greatly increased accelerations considered by Watanabe result in strong non-linear behaviour with greatly increased damping.

Okamoto et al. (6-14) present experimental and finite element studies of large sized two-dimensional models of rockfill dams to determine the stability of the slopes against permanent displacements and slumping during earthquakes. They found that the accelerations at which slope displacements occurred when water was impounded were about 60% of those when the reservoir was empty. Berms were found beneficial in preventing and limiting slope displacements; higher accelerations were necessary to generate permanent displacements in slopes having berms.

Arya et al. (6-16) report on a study of the potential slope deformation in two probable sections of a 99m high rockfill dam; one with a central core, the other with an inclined core. The deformations of the dam have been worked out by computing its acceleration response by shear beam analysis and then examining the deformation potential of the slopes using the yield acceleration concept (Seed and Goodman, 1964). Experimental data from model tests generally confirmed the analytical conclusions that overall deformations would be less than 1% of the height of the dam. It is interesting to note that a pseudo-static analysis of the top portion of the dam with seismic coefficients corresponding to the computed response at that elevation would have indicated an unsafe design. Yen and Wang (6-02) also use the concept of yield acceleration to study seismically induced shallow slope failures on hillsides.

Bhargava et al. (6-17) present an interesting analysis of slope stability problems at the Beas Dam at Pong, India. A number of plastic shear zones in clay shale bands were noted in the foundation rocks at the Beas site. A two-dimensional stability analysis of the dam indicated the need for a huge amount of material for toe stabilization. Model tests were carried out to study possible mechanisms of sliding. Three-dimensional stability analyses of probable failure wedges were then carried out and a satisfactory factor of safety greater than 1.0 was found. Examples of this

kind are necessary reminders that our mathematical models must be in some reasonable accord with reality.

Savinov and Natarius of VNIIG, Leningrad (6-34), in one of the more novel and interesting papers in this section, discuss operational means of increasing the seismic stability of earth dams. Two of the major effects of an earthquake on a dam are to alter greatly the hydrodynamic pressures of the reservoir on the dam and to decrease the stability of the upstream slope by inertia forces acting upstream. Two ingenious solutions are proposed for these problems. To the first approximation, the hydrodynamic water pressures depend on the modulus of elasticity of the water. Therefore, if a thick boundary layer near the dam face is aerated to reduce the effective modulus the seismic water pressures can be reduced to a level at which they can be ignored. Tests were carried out in connection with the huge Nurek hydropower system that confirmed the feasibility of the method. It was estimated that the use of an aeration system triggered automatically by the earthquake would reduce the inertial forces by a factor of two in stability calculations.

The second method involves increasing the stability of a potential failure wedge of the upstream slope by inducing higher effective stresses by pumping from inclined filters in the slope. The pumping would be automatically initiated by the earthquake motions. This technique was tested by a detailed evaluation for the Rogun hydropower project. Use of this technique in the Rogun development would result in huge savings in materials because of the steeper slopes allowable.

It would be very interesting to hear the views of those with major responsibilities for dam design on these particular measures for improving or ensuring seismic stability. It would also be of interest to hear how the reliability of the electrical and mechanical systems would be guaranteed.

The remaining papers deal with more general matters of dynamic response. Ayala and Aranda (6-20) examine the effect of different boundary conditions on soil amplification studies. In particular, they examine the effects of assuming the bedrock to be rigid and the excitation to be due to shear waves only. Their results show that surface motions are not greatly affected by the boundary conditions in the one-dimensional case but that motions near the rigid base may be considerably in error. For two-dimensional problems, they show that the motions at all locations are radically altered by assuming rigid boundaries. It would seem that the effect of the boundary conditions on the motions in the region of interest in two-dimensional problems would depend on the location of the boundaries and that the boundary effects might be rendered inconsequential if the boundaries were located far enough away. It would be of interest to hear to what extent the factor of minimum distance to the boundary was considered by the authors.

Boncheva (6-32) considers the very interesting problem of the amplification of surface waves. She shows the amplification spectra to depend on layer thickness and develops a criterion for determining when the layer may be treated as a half-space and when it will act like a waveguide.

Singh and Donovan (6-15) consider the seismic response of frozen-thawed soil systems and supply some useful information on dynamic properties of frozen soils.

Jakovlev (6-36) examines the problem of the stability of retaining walls under seismic conditions and concludes that a more accurate assessment of stability and stress state may be obtained by using the safe stress theory of Sokolovsky and Golushkevich.

Franciosi (6-22) proposed a discrete particle model for soils consisting of spheres connected by elastic-plastic braces and Migliore (6-10) used the model to evaluate the seismic stability of an embankment. The model is intriguing and seems to have some possibilities for future development.

Two papers present solutions to the vibration of a saturated layer assuming elastic behaviour and coupling between the fluid and the soil skeleton. This problem has been treated previously by Ghaboussi and Wilson (1972). Taga and Toqashi (6-23) present a frequency response analysis and Eskin and Eisler (6-35) presents a finite difference algorithm. These elastic methods deal with transient pore-water pressures and neglect the residual pore-water pressures that develop with non-elastic deformations of the soil skeleton. When the residual pore-water pressures exceed 30% of the effective over-burden pressure serious errors occur in estimating the dynamic response if the effect of residual pore-water pressure on the moduli is neglected (Finn et al., 1976a).

The papers in this section, for the most part, focus on certain practical problems of dynamic response and employ techniques of analysis that are long established and already in the main stream of professional practice. There are no papers that illustrate the advances in seismic response analyses of soils and soil structures that have occurred since the Rome conference some of which were referred to above. Another useful method of dynamic response has been described by Idriss et al. (1974).

#### CONCLUSION

I wish to commend the authors for the time and effort they have expended in preparing the many useful and interesting contributions to the field of soil dynamics. Reviewing the papers for this session has been a pleasure for me and I express my appreciation to the Organizing Committee for the privilege of acting as General Reporter.

Many interesting projects are referred to in the one page abstracts which I had to exclude from my report. I urge you to search these abstracts for material of interest to you. As the author of one of these abstracts, I would be happy to send copies of the original report on which the abstract is based to anyone who is interested and I am sure that other authors of abstracts would also be happy to oblige in the same way.

In conclusion, I would like to re-affirm my judgement that the most significant new development for soil dynamics is in-situ testing to determine strain dependent moduli and damping. This development has been carried out so far by only a few people. I hope we will take advantage of their presence at this conference to find out all we can about these new techniques.

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## DISCUSSIONS ON "THEME REPORT"

V. Khosla (U.S.A.)

The writer has two general comments to offer,

1. Too many theoretical formulations and experimental techniques are presently being advocated by various researchers to find the shear modulus of soil. Variation in their results because of the varied techniques is not of the order of 10, 20 or 30 per cent but some times 100, 200 or 500 per cent. My concern is, are we really benefiting the people involved with the design of structures under earthquake loading conditions or we are simply creating confusion by loading the practicing engineers with a pile of curves and formula from where he is expected to choose some curve to suit his needs.

2. To predict the dynamic response of materials under earthquake loading conditions results of either simple shear test conditions or dynamically loading in the vertical direction have been used. In my opinion both techniques differ considerably from the actual loading conditions. If we study the well known expressions for the compressional waves and shear waves, we will find that importance of one wave relative to the other will depend upon the Poisson's ratio  $\nu$  of the material. For softs, particularly sands where  $\nu$  may vary in the neighbourhood of 0.45, better understanding of the actual soil behavior may be achieved by investigating the material response by pulsating the chamber pressure in a triaxial apparatus, while keeping the static shear stresses constant.

In my paper "Mechanical Characterization of Elastic Plastic work Hardening Soil", though I have not presented the results for this series, formulation utilizing the concept of theory of plasticity have been suggested by which we may be able to determine the material response under all loading conditions from limited number of triaxial tests for axial compressional conditions.

R.V. Whitman (U.S.A.)

This replies to a question directed by Dr. Finn to the authors of Paper 7-25 (see p. 7 of theme report). The parameter  $S_c$  is indeed derived from attenuation equations for peak acceleration and the authors agree that peak acceleration is an important parameter in determining whether or not

liquefaction occurs. The probability analysis might be carried out by the equation:

$$P[LIQ] = \sum_{M,R} P[LIQ|A,D]P[A,D|M,R]P[M,R]$$

where A denotes peak acceleration, D duration, 'M' magnitude and 'R' hypocentral distance.  $P_{A,D/M,R}$  reflects uncertainty in attenuation, while  $P_{LIQ/A,D}$  reflects uncertainty about liquefaction for a given peak acceleration and duration. An analysis of this type appears in Ref. 3.

Empirical plots based directly upon the shear stresses at a site (e.g. Ref. 1) include many sites where the peak ground acceleration has been estimated from some attenuation equation, but the associated uncertainty is not indicated in the plots. The advantage of the proposed empirical plot is that only quantities (M and R) with low uncertainty are used. Uncertainty in attenuation is one of several reasons why there is not a sharp boundary between the solid and open circles.

#### S. Prakash (India)

For in-situ determination of dynamic properties Indian Standards Institution has written a Code IS 5249-1969. This code envisages essentially a block test on 1.5m x 0.75m x 0.7m high block.

In the revised version, of IS 5249-1976, which is under print at the moment, the effect of (i) strain and (ii) confining pressure has also been incorporated.

Also a few other methods e.g. the (i) wave propagation test (ii) cyclic plate load test have also been included.

We view the use of these codal provisions as routine use of Plate Load Test "in this country for determination of allowable soil pressure".

The discussor feels happy to see the gradual acceptance of this code and its use in India through out. And we are collecting very useful systematic data.

#### S.K. Saxena (U.S.A.)

In the nature exist cemented soils, and this cementation is eliminated in sampling and consolidation.

Due to natural cementation, their behaviour is erratic.

Can we discuss any rational methods to evaluate their properties.

Author's Closure

Not received.