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Session 7: Discussion and Replies

Multiple Authors

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Discusion by Albert T.F. Chen,
Research Civil Engineer,
U.S. Geological Survey,
Menlo Park, CA, on "Earthdams and Stability of Slopes Under Earthquakes".

Four papers will be discussed here. In the order of their appearance in the proceedings of this conference, these papers are:

1). Analysis of Dynamically Coupled Percolation and Deformation Problems of Saturated Sands, by Shen;
2). Effective Stress Analysis of Seismically Induced Stability Problems, by Kavanzanjian and Chameau;
3). Permanent Deformation of Earth Dams under Earthquakes, by Shieh and Huang; and
4). Pore Pressure Analysis for an Earth Dam during Earthquake, by Sonpal and Davie.

All of these papers deal with various analytical techniques that are used for studying earthdams and stability of slopes during earthquakes, and in one way or another, each of the papers consider effects of increase in pore pressure during earthquake loading.

In a very brief presentation, Sonpal and Davie show that the pore pressure distribution within an embankment can be established by means of a pseudostatic analysis with assigned seismic coefficients. The pore pressure contours so obtained led the authors to draw certain conclusions about deformation patterns of earthdams during earthquakes; those conclusions are consistent with observations. Because of the briefness of the authors' presentation, it is not possible to comment on the relevance of this approach. However, it should be emphasized that pseudostatic approaches generally do not take into account the effect of strength reduction caused by pore-pressure increase and are not intended for making quantitative estimates of permanent deformation.

Kavanzanjian and Chameau stress the importance of effective stress analysis. For one- and two-dimensional analysis of stability problems during earthquakes, they propose a stochastic formulation of the Finn, Lee, and Martin nonlinear pore pressure generation model. According to the authors, the advantages of the stochastic approach are (1) that it allows us to consider the effect of the order of arrival of the shear stress cycles in a frequency domain analysis, and (2) that it enables the engineer to incorporate geotechnical, seismological, and analytical uncertainties into the stability assessment. In addition, the stochastic approach also facilitates probabilistic evaluation of the damage potential associated with pore pressure build-up for use in seismic hazard analysis.

Unfortunately, the work described by the authors is not completely developed. The approach they propose appears to be rational and potentially useful; the value of the method will become clearer after the the work is completed.

As a modification of Makdisi and Seed's simplified procedure for estimating earthquake-induced deformation in dams and embankments, Shieh and Huang propose the use of a permanent displacement spectrum. Treating the sliding portion of the dam as a single-degree-of-freedom system, the authors define the permanent displacement spectrum as the difference between the displacement responses derived from a nonlinear and a linear system. The nonlinear system assumes that the sliding mass develops a perfect plastic behavior above a certain yield acceleration. Either the maximum spectral value or the spectral value corresponding to the average maximum acceleration of the sliding mass can be used as the design value for permanent deformation. This method appears effective for evaluating deformation of earthdams during a given earthquake, and it is capable of simulating gradual decrease in the shear strength of soils under dynamic loading caused by pore pressure build-up. The authors have not shown adequately that linear and nonlinear deformations can be separated in the frequency domain. However, the authors have shown that for three different earthdams, the results based on the permanent displacement spectrum method are comparable with results from other methods.

Shen presents an elegant two-dimensional dynamic analysis of an earthdam with full consideration given to the generation and dissipation of pore pressure. The dynamic response analysis is similar to the equivalent-linear analysis used in the UC Berkeley computer program QUAD-4; the effective stress model appears to be a two-dimensional version of the model by Finn, Lee, and Martin; and the consideration of pore-pressure dissipation was based on Biot's theory of consolidation. By manipulating the drainage conditions, the author was succeeded in explaining the mechanics of the delayed failure during the 1975 Haicheng earthquake of the upstream slope of Shimenling earthdam in Liaoning Province. That upstream slope failed 80 minutes after the passage of the main shock.

Those who work with two- or three-dimensional consolidation problems have long recognized that under quasi-static loadings, the maximum excess pore pressure need not occur at the onset of the loading. It appears that the same phenomenon can also take place in a dynamic environment. Shen's paper points out that it is important to look into the dissipation of pore pressure and the effect of drainage conditions both during an earthquake and after. All of Shen's previous papers were published in Chinese and are not readily available outside of China; this paper provides a good indicator of the type and the level of research being conducted in China.

Because post-earthquake failures have been observed in China, Japan, and the United States, it is difficult to accept the statement made by Sonpal and Davie that "The critical time will be immediately at the time of earthquake, the dam will be safe in the subsequent condition." This statement should only be interpreted as a conclusion derived from the use of that particular type of analysis. Another striking contrast between the paper by Shen and the one by Sonpal and Davie is in the complexity of the input required for the type of analysis. No fewer than 17 material constants are required for the dynamically coupled analysis proposed by Shen, whereas probably no more than four material constants are needed for the pseudo-static analysis described by Sonpal and Davie.

The number of material constants required for an analysis directly affects the amount of effort (and therefore the resources) required for that analysis. The economic factor is rarely mentioned in the four papers discussed herein, but this factor must be borne in mind if methods described in these papers are to be put to use.

Discussion by E.G. Prater, on "Dynamically Coupled Percolation and Deformation Analysis of Earthdams" by Z.J. Shen

The author expresses the opinion, probably with justification, that if severe failure of the slope of an earth dam occurs it will not be due to inertia forces as such, but rather to the build-up of pore pressures. In an attempt to analyse the problem he has developed an effective stress model requiring 11 material constants, 5 of which describe the increments of volumetric and deviatoric residual strain. He also defines two new measures, coaction and asymmetry, related to the crest and trough values of the principle stresses, which likewise enter into the constitutive relations. Their use is not easy to follow. The modulus and damping values are based on the well-known equations of Hardin and Drnevich. The general approach shows similarities with that of Finn et al (1977).

The procedure for analysis, as far as the dis-discusser understands it,is as follows. Subsequent to static (preearthquake) analysis the authors program EFESD is used in conjunction with the widely used program QUAD4. The latter is a dynamic analysis FE program based on linear elasticity and viscous damping, requiring fairly small time steps for accuracy. Normally it is used in the "equivalent linear" manner, which means adjusting the soil properties after every run in the time domain and iterating in this sense. When coupled to a consolidation program, however, continuous adjustment of soil properties should be carried out. Here, however, for the sake of cost saving and due to the nature of the formulated material law the shear modulus and damping are updated at larger intervals of time (a multiple of Δt - in the numerical example an interval of 2 sec or 80 Δt). Thus increments of residual strain are calculated, introduced as initial strains into a Biot-type consolidation program to calculate pore pressures, which are then used to calculate the new effective stresses.

One wonders why the dynamic consolidation equations of Biot were not used in the first place. Also, one could question the correctness of the author's view that it would be more costly and troublesome to develop a general elasto-plastic model incorporating an autogeneous strain calculation (Zienkiewicz et al, 1980). However, the approach is more satisfactory than the conventional Seed-Lee-Idriss method, from which it is a logical development.

The author presents interesting results for an analysis based on a dam which failed in the 1975 Haicheng earthquake. Unfortunately, the true seismic input is lacking for the damsite, as well as the soil properties. The results illustrate the importance of post-earthquake dissipation and stability analysis.

Reference:

Zienkiewicz O.C., K.H. Leung, E. Hinton and C.T. Chang (1980)
Earth Dam Analysis for Earthquakes, Conf. I.C.E. London, Design of Dams to Resist Earthquakes.

Discussion by E.G. Prater, on "Design Measures to Improve Performance of Fill Dams Under Earthquake Loading" by K.L. Logani.

This paper describes practical means of providing seismic resistance for embankment dams. Perhaps the introductory remarks of the author need to be qualified somewhat. It is probably exaggerated to call the pseudo-static method of analysis "a very dangerous engineering practice". This would be true of only a certain class of dams, namely those of hydraulic fill construction or generally dams containing uncompacted zones which could liquefy during shaking. Some of the Japanese dams listed in Table I were conservatively designed using high seismic coefficients (usually up to 0.2). For properly engineered dams, as these large dams are, with adequate attention being given to compaction and zoning, this is arguably a possible alternative. The importance of analysis (including FE) is also depreciated. Little cognizance is taken of the strides made in the last 10 years, including some valuable back-analyses of vibration test data (Abdel-Ghaffar and Scott, 1979).

The author has collected together some data of large dams built in regions of medium to high seismicity (Table I), from which separate graphs of freeboard, width of core at foundation level and crest length in function of dam height are plotted. This data is complemented by a table (II) showing the actual design changes made to accomodate seismic loading in several dam projects of the authors company. The protective measures incorporated in the particular case of an Argentinian dam with a dispersive clay core are delineated.

Reference:

Abdel-Ghaffar A.M. & R.F. Scott (1979) Analysis of Earth Dam Response to Earthquakes, ASCE, Jnl. Geot. Eng. Div., V. 105, No. GT12, pp. 1379-1404.

Discussion by E.G. Prater,
on "Seismic Analysis of
Spinney Mountain Dam" by
J.V. Williamson and M.E. Shaffer.

The project described by the authors concerns a 30 m high zoned earth dam in a region of low seismicity (i.e. the Denver Formation Sediments, Colorado). The thorough geological investigation revealed the presence of a fault about 1 1/2 miles from the dam site (detected by scarps shown up on aerial photographs) and which showed signs of activity within the last 35 millenia. The fault offset was about 2 1/2 m. Thus the dam design had to take account of the eventualities of tectonic movement at the site itself and of damaging ground shaking. Therefore, the somewhat unexpected result of the seismological investigation was a design earthquake M = 6.2, a_{max} = 0.6 g, and duration of strong shaking of 15 s and this in what is regarded as an aseismic region! The high peak acceleration results from the small epicentral distance.

For the conventional pseudostatic analysis a seismic coefficient of 0.1 (g) - estimated for potentially active faults 30 miles away - had been used. For the reassessed seismological situation a dynamic analysis was now considered necessary. Since cohesionless materials were not present in the embankment or its foundation the Newmark approach was selected, in preference to the Seed-Lee-Idriss method. One of the results of research in recent years is that if the embankment materials do not weaken under cyclic loading the inertia forces due to the earthquake are unlikely to induce significant deformations unless the horizontal yield acceleration is very low, which should not be the case with properly engineered slopes. For the three selected earthquake records and a yield acceleration of 0.16 g the maximum crest displacement was calculated to be less than 5 cm. The value was a fraction of this if the modifications to Newmark's method introduced by Makdisi and Seed were considered.

Concerning this kind of analysis it should be borne in mind that the actual deformation calculated is very sensitive to changes in yield acceleration and thus also to soil properties. Furthermore, the failure mechanism is not as simple as assumed and the development of a distinct slip surface may not happen. Nevertheless, the result is a good indicator of adequate earthquake resistance for this dam.

The rest of the paper describes practical design aspects, including the contingency plan of providing a zone of "crack-stopper" material if a capable fault were discovered during the excavation works.

All in all, this was a commendably thorough investigation for what is a relatively small dam.

Discussion by E.G. Prater,
on "Evaluation of the Seismic Stability of Foundation Soils of Revelstoke Earthfill Dam",
by K.S. Khilhani and P.M. Byrne.

The authors describe the investigation of the seismic resistance of the foundation materials contained in a deep (up to 50 m) sand channel crossing the dam axis near its right abutment. Standard penetration values N_1 and relative densities of samples from the core trench were obtained. The correlation between the two was poor, especially for the downstream side. In addition 30 cyclic triaxial tests were conducted on 86 mm dia. samples. The results (Fig. 4) are given in terms of liquefaction (presumably 100 % pore pressure ratio) or 5 % double amplitude cyclic axial strain. Unfortunately the data points are not distinguished in this respect. The Shelby tube samples and the upstream block samples exhibit the lowest resistance (i.e. 10 cycles of $\sigma_d \approx 0.37\ \sigma'_{3c}$ produce liquefaction). The higher cyclic strength of downstream block samples is attributed to less disturbance and a higher silt content (no grain size distribution curves are supplied). However, no static strength data is presented. Such data would have been helpful, not only to assess the extent of sample disturbance. The standard penetration resistances show quite the opposite trend, surprisingly, to the triaxial data for the block samples. It is interesting to note that the 5 reconstituted samples, whose average dry unit weight was about 10 % higher than that of the in-situ value (γ_d = 15.4 KN/m^3), were of the order of 70 % stronger cp. liquefaction resistance of Shelby tube samples. Here, the authors do not discuss the question of ageing (or sustained consolidation pressure), an apparently important but still poorly explained factor, (Seed, 1979).

The evaluation method followed is conventional, except that 3 one-dimensional soil columns are used for the dynamic analysis part. Considering that the final evaluation of liquefaction resistance is based simply on an empirical correlation obtained for level ground conditions greater refinement would hardly be justified.

The question tentatively raised concerning the influence of effective confining pressure and liquefaction has been discussed elsewhere both for static (Casagrande, 1976) and cyclic loading loading (Studer et al, 1980).

Further, the belief that liquefaction resistance is higher for sloping ground conditions is a fallacy. The amount of residual pore pressure induced is less due to anisotropic stress conditions (Studer et al, 1980). This does not mean, however, that the factor of safety is necessarily more favourable.

For the discusser the most disturbing facet of the investigation is that the trouble was taken to perform a large number of cyclic triaxial tests but then only SPT results were used. The latter are regarded by many engineers as unreliable. The authors, however, overcome this problem by taking conservative values from a statistical plot of the data, (Fig. 7).

A final observation is that if the triaxial test results had been used and the evaluation carried out along the lines of the Seed-Leed-Idriss method (considering horizontal planes to obtain the in-situ stress ratio) the values in Fig. 4 would have to be corrected by a factor c_r = 0.6 (Seed, 1979). For a maximum dynamic stress ratio in the fine sand of 0.16 the F.S. drops below unity on this basis. That the methods give such differing results is highly unsatisfactory from a practical viewpoint. What engineering decisions would have been made in the absence of SPT results?

References:

Casagrande, A. (1976) Liquefaction and Cyclic Deformation of Sands: A critical Review. Harvard Soil Mechs. Series No. 88.

Seed, H.B. (1979) Soil Liquefaction and Cyclic Mobility for Level Ground Conditions during Earthquakes, ASCE, Jnl. Geot. Eng. Div., V. 105, GT2, pp. 201-255.

Studer, J., Zingg, N. and Prater, E.G. (1980) Investigation of Cyclic Stress-Strain characteristics of Gravel Material, 7th World Conf. Earthquake Eng., Instanbul, V. 3, pp. 355-361.

Discussion by William Y.J. Shieh, Harza Engineering Company, Chicago, IL on "Seismic Analys Analysis as a Tool in the Design of Two Earth Dams", by A. Yziguel et al.

Two statics, dynamic and permanent deformation analysis studies for two embankment dams, one resting on alluvium and the other on a rock foundation are clearly described in the paper. These studies use the latest analytical methods such as a finite element non-linear incremental analysis for the static analysis, the equivalent linear method for the dynamic analysis and the Newmark method for the permanent deformation analysis. All analyses are very well done in obtaining the stresses and dynamic responses of embankment dams. Authors of this paper have demonstrated that these analytical methods are well within the capabilities of engineers. They are relatively economical and simple to use and interpret.

The authors have spent considerable efforts in the analyses of dam and in evaluating of the results. The results were evaluated for the risk by taking ratio of the induced shear stress to the shear strength. The reciprocal of the risk will be the cyclic shear strength stress ratio which can be considered as a dynamic safety factor.

The permanent deformations of the fill were properly evaluated from the dynamic response by the Newmark method. The other form of evaluating the potential deformation produced by the earthquake is to study the strain potential in an embankment. The strain potential can be obtained by comparing the cyclic shear stresses required to cause selected levels of strain in laboratory test specimens with the equivalent uniform cyclic shear stresses induced by the earthquakes. The distribution of the potential strain within the embankment can be used to estimate the maximum potential settlement and lateral movement of the embankment during the earthquakes.

The phenomena of resonance as evidenced by overall high amplification for both dams analyzed by the authors can be critical. The non-linear seismic response analysis (i.e., Shieh & Huang, 1981) should be a significant aid, for proper design of the embankment in resisting potentially large deformation due to this resonance.

Discussion by William Y.J. Shieh, Harza Engineering Company, Chicago, IL on "A Simplified Procedure for Evaluating Maximum Response of Soil Layer During an Earthquake", by Fu Sheng Cong and J. Jingbei.

A simplified procedure for evaluating maximum response of soil layer during an earthquake by probabilistic method is presented in this paper. The soil layers is regard as a shear beam system of multi-masses. Using Kanai's and Tajinu's spectral density of ground motion, authors have derived the equations to obtained the acceleration response spectrum. The acceleration spectrum for Tangshan Earthquake (1976) obtained from the probalistic method is similar to those obtained from the mode superposition method and the visco-elastoplastic wave propagation method. This agreement of the analytical results shows that the proposed method is just as good as the other methods.

The clearness of the paper can be improved if some terms used in the equations are defined as it appears first time in the text.

Discussion by William Y.J. Shieh,
Harza Engineering Company,
Chicago, IL, on "Earthquake Resistance of a Rockfill Dam", by C. Bossoney, E.G. Prater, M. Balissat, J. Studer, and N. Zingg.

The investigation of the seismic safety of a 131 m high rockfill dam across the Rio Chixoy in Guatemala was presented in this paper. The paper did not clearly state what type of the static analysis had been made for the dam. Generally, the static analysis is made with the use of a finite element non-linear incremental analysis to simulate the layer and stage construction. The dynamic analysis was performed using the equivalent linear procedures outlined in the Seed-Lee-Idriss method. Discussion of strain dependent soil properties is lacking in the paper.

The paper presented a good state-of-the-art discussion on the pore pressure development under monotomic and cyclic loadings. Under the cyclic loading, the structure of the cohesionless soil tends to become more compact resulting in the pore pressure built-up and a reduction in the intersoil grain stress. The pore pressure built-up will cause reduction of the soil strength and may result in the permanent deformation during an earthquake and the slope stability failure right after the earthquake for the loose sand. This is known as liquefaction. For the dense sand, the subsequent cyclic loading or monotonic loading will tend to cause the soil to dilate resulting in the pore pressure drop. This pore pressure drop will cause strength increase and will restrain further deformation. This phenomenon of the limited deformation is known as "Cyclic Mobility."

Under the monotomic loadings, the volume of a compacted cohesionless soil tends to decrease first and then increase as the soil continues to deform as shown in Figure 1. The volume increase of the soil will generate pore pressure suction which will cause the strength increase and will restrain further deformation of the soil. This phenomenon of limited deformations under the cyclic loading due to pore pressure built-up and subsequent restrain of deformation due to pore pressure drop caused by the soil dilatation is known as "Cyclic Mobility." By using Figure 1, one can estimate the deformation of the embankment.

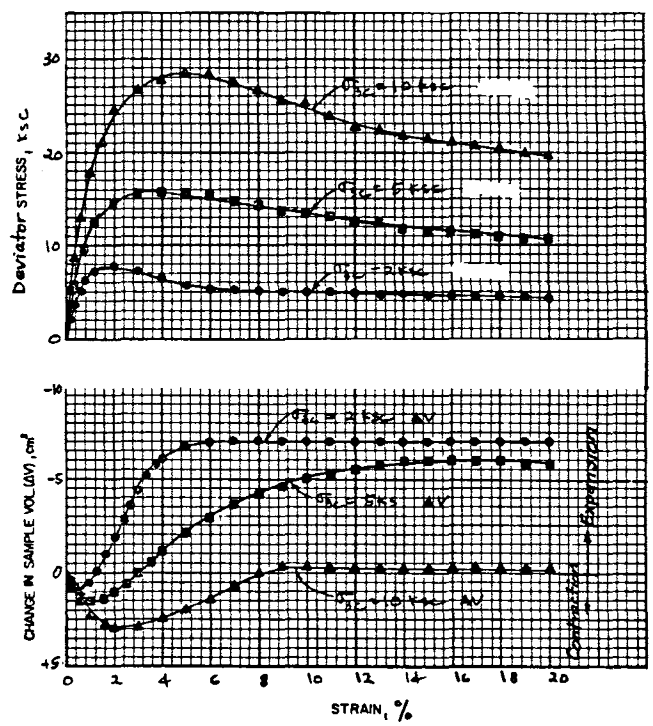

Figure 1. Deviator Stresses and Volume Changes of Compacted Silty Sand

Discusison by William Y.J. Shieh,
Harza Engineering Company,
Chicago, IL, on "Seismic Deformation of Dams by Correlation Method", by Y.K. Lin, K.V. Rodda, C.W. Perry and D.K. Gill.

Analyses of anticipated earthquake performance of three similar earth dams in northern California are presented. One dam was analyzed with use of full scale finite element analyzes, the Makdisi-Seed simplified method and the Sarma method. Other two dams were analyzed with use of the Makdisi-Seed method and the Sarma method. The results show that there is a good agreement between computed displacements using finite element method the Makdisi-Seed method. However, permanent displacements obtained from the Sarma method are 65% to 160% greater than the displacement obtained from the finite element method and the Makdisi-Seed method.

The displacement obtained from the finite element program DEFORM based on the strain potential is the maximum potential displacements which can be expected during the earthquation motions. The strain potential is the partial reversible strain in laboratory test specimens for the induced cyclic shear stresses. Therefore, the displacement obtained from the Program DEFORM are partially reversible and can not be used to compare with the irreversible permanent deformation obtained from the Makdisi-Seed method, the Sarma method or the Newmark method. Any agreement between the displacements obtained from the finite element method and the Makdisi-Seed method is coincidental.

The permanent deformations obtained by double integration of the accelerations exceeding the yield acceleration are the approximate permanent displacements. The dynamic response history is always affected by the preceding events in the history. Any adjustment of the peak acceleration at the beginning will affect the later peak accelerations. Therefore, the true permanent displacements can only be obtained by the non-linear dynamic analysis such as the Newmark method.

Discussion by Spencer N. Chen,
Senior Geotechnical Engineer,
Tippetts-Abbett-McCarthy-Stratton
Engineers and Architects, NY, on
"Newmark's Sliding Block Approach".

Newmark's sliding block approach for computing the downslope permanent displacement of a sliding mass has been mentioned or discussed by many authors at this conference. The writer agrees with the statement made by Seed during Session 7 that Newmark's concept is still state-of-the-art for computing permanent displacement of a sliding mass along a known failure surface. The resulting permanent displacement using this concept is dependent upon the method used in the computation of the yield acceleration and the relative velocity. Following are some comments concerning the Newmark approach.

(1) Yield Acceleration. The Newmark concept for calculating displacement during dynamic loading, discussed by Seed, is illustrated in Figures 1 and 2 which are similar to Figures 22 and 23, respectively, presented by Prakash in Session No. 3. Figure 2 shows that the yield accelerations Ky_1, Ky_2, Ky_3, are different for different peak accelerations. Neither Seed, nor Prakash presented a clear explanation of the variation of yield acceleration with time for a given sliding block subjected to earthquake loading. Although Newmark's approach was mentioned in many papers only a few authors presented actual application of variable yield acceleration (1, 2, 3). Further improvement of the State-of-the-Art requires additional research to determine the effects that dynamic pore pressures, changes in shear strength and other related factors may have on yield acceleration as a function of time.

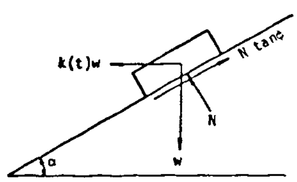

Figure 1. Forces on Sliding Block (after Newmark, 1965)

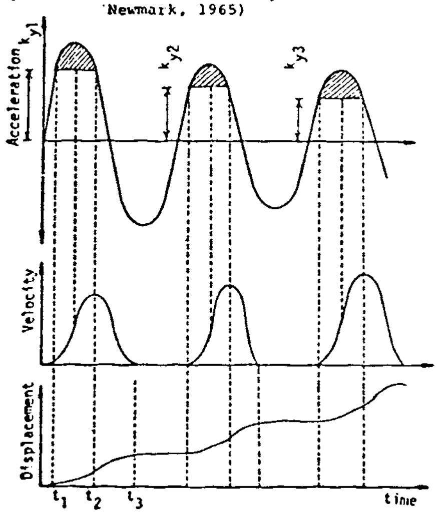

Figure 2. Integration of Effective Acceleration Time History to Determine Velocities and Displacements (after Newmark, 1965)

(2) Relative Velocity. Figure 2 shows that for the first peak acceleration the relative velocity starts from time t_1, reaches a maximum at time t_2, when the base acceleration intersects with yield acceleration, and then reduces to zero at time t_3. Many authors mentioned using double integration to obtain the relative displacement for each peak that exceeds the yield acceleration; it is unclear how the relative velocity varies with time. For a constant yield acceleration, n_g, the writer prefers the use of the method shown in Fig 3 [8] which is essentially similar to the method used by U.S. Army Engineer Waterways Experiment Station (Fig. 4). That is, depending on the shape of the base accelerogram, the maximum relative velocity may not coincide with the time at which the base acceleration intersects the yield acceleration. The time t_3, representing zero relative velocity can be determined easily by a computer program (4) if the velocities of the base and sliding block are computed separately and compared at each time increment of the digitized accelerogram. The relative displacement during each period can be computed by intergrating the shaded area as shown in Fig. 3.

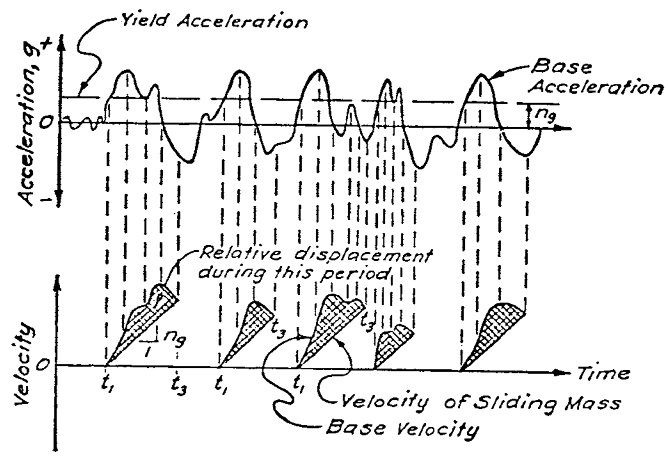

Figure 3 Computation of Permanent Displacement Unsymmetrical Resistance

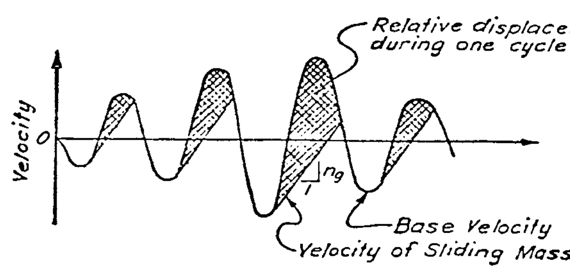

Figure 4 Computation of Permonent Displacement Unsymmetrical Resistance (After U.S. Army Engineer W.E.S. paper S-71-17, Nov. 1977).

(3) Direction of Earthquake. Vertical accelerogram and the N-S or E-W component of horizontal accelerogram are usually used to represent the design earthquake. The direction of the earthquake motion given in the design earthquake may not coincide with the orientation of the

slope being analyzed and the direction of the minimum dynamic resistance of the sliding mass. (The minimum dynamic resistance of the sliding mass is defined as the minimum inertia force that is required to produce a factor of safety of unity and thus cause the sliding mass to experience permanent displacement.) The problem is much more complicated if the average values of the time history of accelerations along the failure plane, obtained from the dynamic response analysis, are used to represent the base acceleration in Newmark's approach.

It is normal practice, however, to assume that the yield acceleration or minimum dynamic resistance acts horizontally and that the direction of the design earthquake is in the most critical direction. Also, the positive peak acceleration of the horizontal accelerogram is normally assumed to have the same orientation as the yield acceleration. Since the design earthquake is usually estimated from earthquake record (s) obtained elsewhere and the direction of the earthquake motion is difficult to predict, it is recommended that, in order to be conservative, positive and negative peak accelerations of the horizontal accelerogram be assumed to act in downslope direction and that the total permanent displacement for each be computed separately by Newmark's method. The higher computed value can then be selected as the permanent displacement for the given yield acceleration. The consideration of both positive and negative peak acceleration toward downslope is especially needed if the given design earthquake is not well balanced. In the computer program (4) developed for Newmark's approach to the computation of permanent displacement, the negative peak acceleration in the downslope direction can be achieved by multiplying the entire accelerogram by minus one (-1). This writer's experience shows that depending on the characteristics of the accelerogram and the level of constant yield accleration, the difference between the total permanent displacement computed separately for the positive and the negative acceleration in the downslope direction can be as high as 10%.

In summary, Newmark's concept of assuming the whole sliding mass as a rigid body may not truly represent the actual behavior of the slope under dynamic loading. However, by using yield acceleration as a function of time, and by improving the formulation of relative velocity, Newmark's approach could be improved to better represent actual behavior.

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2. Wang Siging and Zhan Juming (1981), "On the Dynamic Stability of Block Sliding on Rock Slopes", Volume I Session 7, pp. 431-434.

3. William Y.J. Shieh and Rodney J. Hung (1981), "Permanent Deformation of Earth Dam Under Earthquakes", Volume I Session 7, pp. 453-458.

4. "Computer Program YDBH9" developed by TAMS Geotechnical Department for Newmark's Sliding Block Analysis.

Discussion by J.N. Srivastava, on "Rockfill Dam Safety Problem in a Seismic Zone" by A. Moroianu and V. Perlea.

The paper gives the details of design studies carried out for two rockfill dams in Romania and the methodology adopted is suggested for use in analysis of other rockfill dams. The main feature is consideration of the earthquake intensity at two levels - a mean one for which the dam should conserve its integrity and a higher level when the dam could suffer unimportant damage. The mean and high level earthquake characteristics - magnitudes, return periods, maximum accelerations and predominant periods - have been determined on the consideration of the distance of the site from known earthquake sources. Four known earthquakes (such as El Centro) are then modified to meet the earthquake characteristics requirements for both mean and higher level as well as stochastic accelerograms are considered. In my opinion this two level intensity should not be necessary. During an earthquake, one is prepared for an acceptable damage. This acceptable damage has to be defined with respect to the risks which one could take for any site and the high level intensity earthquake should not cause damage greater than acceptable. Could it not be possible to define under the present state of knowledge, an earthquake applicable for the dam site, which could be used in the analysis of the dam?

The authors propose finite element analysis in the elastic range to determine the response of the structure giving the accelerations in the height of dam and a circular arc stability analysis to find the factor of safety. No mention has been made for the method for assessing the development of pressures in the rockfill. Two sets of values are taken for the elastic properties of the core and the shells, the elastic modulus being in the ratio of 1:10. No justification is given for the values taken. In another paper presented by Professor Priscu and others in the technical bulletin of the Institute of Construction, Bucharest Annual XXI, No. 1-2, 1978, different properties of materials are taken for this dam. This needs some explanation. A general question could, however, be asked as to why the analysis of such dams still continues to be based on the psuedo static stability of a slipping mass. The codes of Japan, India and other countries still provide for such methods. The answer would probably lie in the uncertainties still involved in the determination of dynamic properties of materials and behavior during earthquakes. Maybe, relatively better performance of these dams also contributes to sticking to the simpler method.

The authors have made two statements in the paper, "comparative computations showed that the influence of the water in the reservoir does not have an important influence on the acceleration distribution", and "the angle of internal friction sensibly decreases with normal stress increasing , that is with the depth, for non-cohesive soil". These statements are too sweeping and not always correct.

Another important point made in the paper, which is important from the practical considerations, is the use of relatively weaker material. This material has been placed just downstream of the core, which is the location of maximum vertical stresses during static conditions, especially if the core transfers its load to the shells. However, the possibility of developement of pore pressures during seismic conditions are less, and the choice of location is suitable.

Discussion by J.N. Srivastava, on "Dynamic Properties of Embankment Dam", by M. Oner and M. Erdik.

There is a minor omission in the paper. It is not specified that the figures showing the agreement between the computed and observed displacements refer to which dam.

I could not have access to the 1980 paper of the first author, which has the derivation of the semi-empirical formulae for the fundamental period. However, I find that the formula is not dimensionally correct and the authors could clarify the same. I am also unable to appreciate why another dimensionless factor AF is introduced. This factor is about 22 times the well known k_{2max} factor, which could have been used. The paper is a useful one as the values for AF for different types of dams could be used in the preliminary designs.

Discussion by J.N. Srivastava, on "Estimate of Displacements of Rockfill Dams Due to Seismic Shaking" by J.L. VonThun and C.W. Harris.

The paper suggest that the value of effective friction angle should be reduced by 10 degrees after the first motion. This is questionable as in angular rockfill there may be only slight reduction in friction angle due to movement. There could be crushing at contact zones but the reduction would not be of that order. The paper does not state if the values should be reduced in every motion or kept constant after the first motion. The paper also does not consider the cohesion part in the core and the thinking seems to be that the cohesion is lost in the first movement.

Discussion by Z.J. Shen ,
Nanjing Hydraulic Research
Institute, China, on
"Seismic Deformation of Dams by
Correlative Methods", by Y.K. Lin
K.V. Rodda, C.W. Perry, and
D.K. Gill.

In recent years extensive attention has been paid to the subject of deformation of soil medium under transient loading. In this Conference there are several papers concerning with this topic too.

In following we will give some comments on the methods commonly used in the United States as described by the authors. One method to obtain deformation of embankments is the integration of so-called strain potential deduced from dynamic analysis. To our knowledge, the strain potential is associated with elastic behaviour only, and logically speaking, the deformation thus obtained disappears once the seismic shaking ceases. Hence, it seems that this method is not concerned with permanent deformation of soils. The method proposed by Makdisi and Seed as well as that proposed by Sarma are both based on the Newmark's concept of slide of rigid block, when the induced accerelation exceeds some limit value. These approaches are more or less rational, but the rigid body movement assumption seems far deviated from the actual behaviour of soils. In addition, both methods can not account for the stress redistribution in soil in the process of its deformation.

It is worthy of pointing out that the deformation itself is not so harmful to earth dams, provided that it is not accompanied by some types of slope failure. One can suppose that the extraordinary settlement of dam crest caused by seismic shaking may result in overflow of reservoir water. But such an accident is unlikely probable. The only exception reported is Hebgen Dam, where the dam crest was overtopped sereval times by earthquake-induced waves in reservoir (Seed 1978). But even there the settlement is not the main cause of overtopping. On the other hand, it is well known that cracking in dam body is the type of damage commonly observed after earthquake. The water flow through cracks may cause the erosion of dam materials and bring it to complete failure. This type of failure always takes place at several hours or even several days after earth quake. For example, seven small dams failed in this way after Nigata earthquake in 1964 (Takase 1966). But until now, little attention has been paid to the problem of dynamic cracking of earth materials.

The authors call the first method 'rigorous' and the last two 'simplified', and use all three methods to evaluate the deformation of Guadalupe dam induced by a credible earthquake. They find out that maximum horizontal displacements computed by these three methods are deviated not far from each other and here upon draw a conclusion that the simplified methods can be used for practical purpose. It is our opinion that none of the existing methods for seismic deformation analysis at present may be considered as a 'rigorous' one. The use of the simplified methods of course is allowable if there are experimental evidences to support it. But using aforementioned methods one can obtain horizontal displacement only, and it is unable to conclude with confidence about dam safety from this displacement only. It must be emphasized again that it is the deformation-connected cracking but not the deformation itself may damage the dams.

The cracks of earth dams developed during earthquake may be caused by dynamic stress, when the latter exceeds the dynamic tensile strength of soil. But they may also be formed as a result of redistribution of static stresses accompanied by the accumulation of permanent deformation. To attack this problem, it is recommended first to develop a laboratory method of tension test for cohesive soils under repeated loading. As to the assesment of stress redistribution, we have developed such a method as a part of our general procedure for dynamically coupled percolation and deformation analysis of earth structures (Paper No. 74 in this conferece). Recently, this procedure has been used to check the performance of Dou-He earth dam near Tangshan during an earthquake on July 28, 1976. The dam material is sandy clay, while its foundation consists of stratus of sandy clay of low plasticity, clayey sand and sand. Figure 1 shows the configuration of this dam before and after earthquake and also the location of main cracks. Some of the cracks were filled with fine sand, squeezed out of foundation during liquefaction. Results of analysis for maximum dam section are illustrated in fig.2. The input bedrock motion is one of major aftershocks, recorded in Qian'an station and scaled to a maximum acceleration of 400 gal. The location of bedrock surface is assumed at 30 m below the dam base while its actual depth is believed much greater than this value. Hence, the computed results only represent qualitatively the actual performance. The calculated

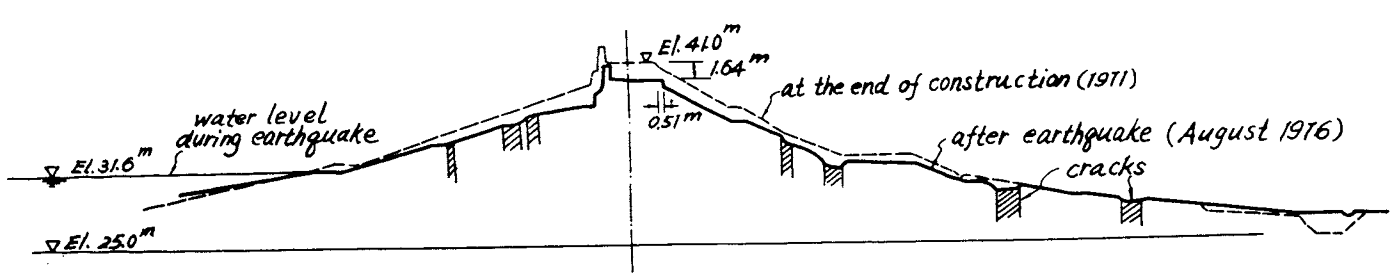

Fig. 1. Configuration of Dou-He Dam before and after Earthquake

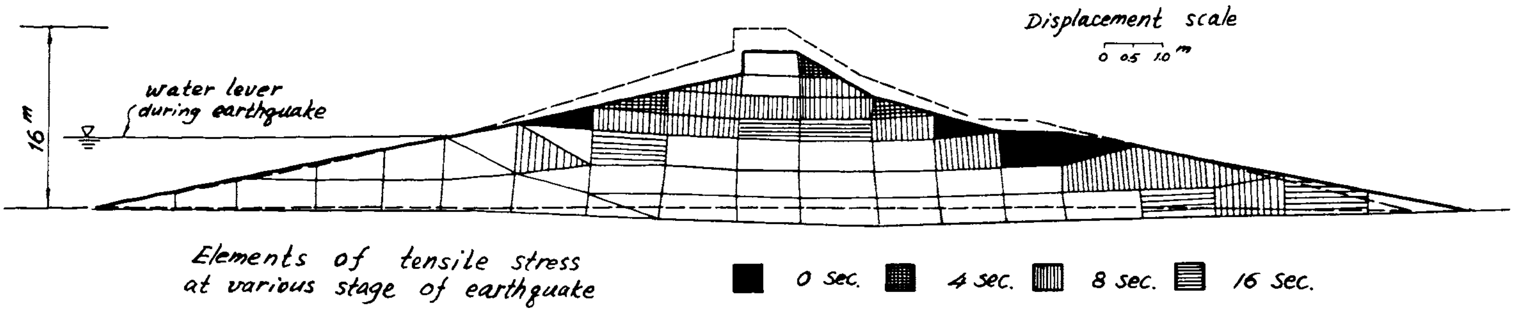

Fig. 2. Computed Deformation and Zone of Tensile Stress

crest settlement is 0.45 m while the actual values recorded on left and right of the studied section is 0.79 m and 1.22 m respectively. However, the location of tension zone calculated by this method agrees reasonably with that of the actual cracks. Naturally, the method of two-dimensional analysis can be used to predict longitudinal cracks only, and this type of cracks may not be so vital to earth dams. It is expected that in the near future the method of three-dimensional analysis will be developed to meet the requiment for predicting the dynamic cracking of earth dams in all direction.

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AUTHOR'S REPLY

Closure by Z.J. Shen.

In response to the discussions of A.T.F.Chen and E.G. Prater the author will concentrate on the two following points.

First, I generally agree with Mr. Chen's statement that there are too many material constants used in our dynamically coupled analysis. This may limit its widespreading use in practice. Further work is necessary to simplify the procedure both in formulation of the model itself and in the laboratory method for determinating the material constants. But it is also unreasonable to use an oversimplified method, disregarding many important features of soil behavior against loading. On the other hand, the well known fact is that it is unable to draw reliable material constants from the disturbed samples tested in the laboratory. Therefore, we appreciate the efforts made by American and Japenese research workers to get undisturbed samples of sands using freezing techniques. But, we doubt whether this rather troublesome testing procedure can be used as a routine one. As we see, a better way to get reliable soil constants is to use the in-situ cyclic loading testing technique. Such a method, using vibroflotation equipment as a loading system, is being developed in the Nanjing Hydraulic Research Institute.

The second topic I wish to deal with is the use of the elasto-plastic model for the seismic analysis of soil structures. The elasto-plastic approach is no doubt more rational than the equivalent viscoelastic one, especially in the case of sands. The procedure of statically coupled deformation and consolidation analysis using both a nonlinear elastic model and an elasto-plastic model has been developed in China for several years (Shen et al,1979,Zhang, 1981). The reasons why we did not start our work on soil dynamics by means of elasto-plastic models have already been given in our original paper. The first reason is that none of the existing elasto-plastic models can satisfactorily describe all the main features of soil behavior under cyclic loading. Some of them may be considered as better ones, such as models of Dafalias (Dafalias et al, 1980) and Sato (Sato et al, 1980). However, while they succeed in explaining many aspects of soil behavior, they may fail in some other cases. For example, Dafalias's model predicts limited growth of pore pressure under undrained conditions, thus being unable to explain the complete liquefaction of loose sand, while Sato's model predicts the same amounts of volumetric strain in the first and subsequent loading cycles under drained conditions. So far as the model of autogeneous strain is concerned, it seems to us that theoretically this model is not so rigorous as aforemetioned ones. According to this model the autogeneous volumetric strain is always positive or contractive, while real soils may give contractice strain in one moment and dilatant strain in another. Only in a complete loading cycle the net volumetric strain is always non-negative. Another reason concerns computing time. When an elasto-plastic model is used, it is necessary to repeat formulation and decomposation of the global stiffness or coefficient matrix for each small time step,while in the case of a visco-elastic model this should be done only every 50 or 100 steps. Still another trouble which we are confronted with in the development of a more rigorous theory is the pulsative movement of the pore fluid relative to the soil skeleton, which was once dealed with by Ghaboussi and Wilson (1973), who regarded soil as an elastic porous medium. Anyhow, we hope that this type of elasto-plastic theory of soil dynamics will be introduced in the near furture.

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AUTHOR'S REPLY

Closure by E.G. Prater.

In reply to Mr. Shieh the standard analysis procedures were not presented in greater detail due to the limited scope of the paper; see, however, the reference Bossoney and Dungar (1980). The strain-dependent soil properties for the dynamic analysis were also omitted for the same reason. However, an extensive laboratory investigation using the cyclic triaxial apparatus had been undertaken to furnish these properties for the core and shell materials. For the clay the maximum shear modulus was estimated from a formula given by Hardin and Drnevich. The resulting normalized curve G (γ) lay somewhat above the standard curve of Seed & Idriss built into the program QUAD4. For the shell material the fraction <25 mm was used (sample dia. = 150 mm). Laboratory resonant column tests seem to underestimate the value of G_{max} for gravels and thus the value of K_2 given was based on the observed performance of the Oroville dam. From the laboratory test results the secant modulus E at an axial strain of $2 \cdot 10^{-3}$ % and a confining pressure of 800 kN/m^2 was $2.5 \cdot 10^6$ kN/m^2, (i.e. about 80 % of the max. value based on $K_2 = 200$). The shape of the curve was more or less the same as for published data on sands, while the damping values were within the range of scatter for sands.

The discussor reiterates the phenomenological behaviour of cohesionless soils using the popular terms liquefaction and cyclic mobility. To the author it is not apparent how the enormously complicated problem of estimating the deformations in an earth dam can be estimated simply using a figure such as he shows. However, I do not want to pursue this topic, but rather to take up again the question of the significance of porewater pressure build-up. So often one hears the statement that "the pore pressure built-up will cause reduction of the soil strength". I had hoped that in the paper the point had been made clear that pore pressure build-up is a necessary but not a sufficient condition to induce failure. The fact that the stress path point lies on the critical state live or even outside it (but within the Hvorslev surface) does not even mean failure is incipient, e.g. compare states D and F in Fig. 4 of the paper.

In the oral reply to the question of the moderator (W.F. Marcuson III) on the effect of hydraulic gradients the author suggested that the failure of the Lower San Fernando Dam was not due to liquefaction in the classical sense of the term.

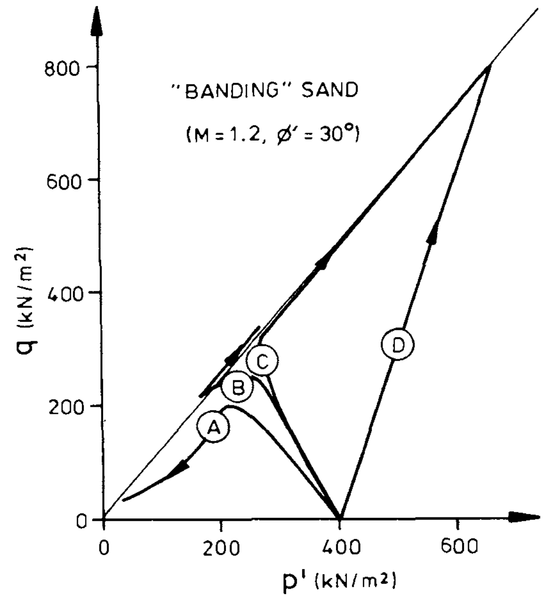

Fig. 1 Monotonic triaxial tests on saturated sand (after Casagrande, 1976)

In Fig. 1 above liquefaction is illustrated from some results (plotted here in the p'-q diagram) reported by Casagrande (1976). Samples A and D tested under undrained and drained conditions respectively have initially a relative density $D_r = 30$ %. Sample A exhibits classical liquefaction response under monotonic loading. Sample B ($D_r = 44$ %) also appears to be developing a flow structure but then recovers and tends to dilate. Sample C ($D_r = 47$ %) shows the typical tendency to contract and then dilate developing at failure pore suctions under undrained conditions. If we now compare a typical result (both drained and undrained) for the material from the critical zone in the lower San Fernando Dam (Fig. 2) with the results here presented in a p' - q diagram we note that the material is typical of dilating (dry of critical state) soil with no strong suggestion of the development of a flow structure. This is to be expected with a D_r value of 55 %.

Figs. 1 and 2 show the behaviour of an element of soil. Under cyclic stresses the hydraulic fill material although denser than critical may build-up porewater pressures but if the element remains undrained the strength is still given by point a (Fig. 2). A conventional pseudo-static analysis would indicate an adequate factor of safety. If the material could dilate during or subsequent to the earthquake the strength reduces (cf. drained strength: peak at point b and ultimate at point c). Thus the importance of pore pressure redistribution and volume changes in potential shear

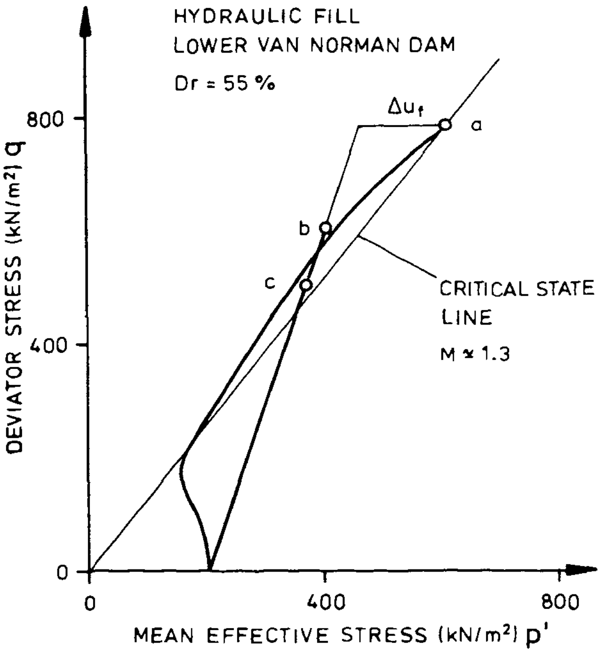

Fig. 2 Typical triaxial test results for Lower San Fernando Dam Material (after Lee et al, 1975)

zones is evident. It is known that the Lower San Fernando Dam failure was delayed about 1 minute (Seed, 1979) and Shen (1981) reports an upstream slope failure induced 80 minutes after the passage of the earthquake. The factors contributing to the mechanism of failure were discussed briefly in the paper. These will now be further discussed with reference to Fig. 2 above. In the region of the potential slip zone the material is consolidated under anisotropic stresses and the initial stress point would lie typically a little below point c. The factor of safety F, depending upon the choice of strength parameters, was about 1.3 for a seismic coefficient of 0.15 (Seed, 1979). A seismic coefficient of about 0.22 to 0.34 would be needed to reduce F to unity (Seed et al, 1975). In fact, the peak accelerations were about 0.5 to 0.6 g. Thus the stress path during the earthquake may have risen to the level of point b (Fig. 2) or even higher, so that pore suctions would have been induced transiently during the earthquake. If these were limited to the potential slip zone locally high hydraulic gradients would be set up helping to enhance dilatancy (increase in void ratio) with a loosening of the structure. In the case of post earthquake failure it would require a further increase in pore pressure in the potential slip zone (caused by transient flow and pressure redistribution) reducing the effective stress in the already loosened material before the static (or post earthquake) level of deviator stress could cause a Coulomb slip surface to develop. It may thus be explained that the undrained strength characteristics of a soil element are not the decisive factor, but consideration must also be given to the relief of pore suctions and further reduction of effective stress. The phenomenon is complicated, but the approach followed in the paper, i.e. of directly applying the effective stress principle with the cyclically induced pore pressures and neglecting the effects of dilatancy, - though conservative - provides an amenable method of analysis.

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AUTHOR'S REPLIES

Closure by K. Khilnani and Peter M. Byrne.

The moderator, Mr. William Marcuson, commented that our Factor of Safety, F=1.4 against liquefaction is overly conservative for an earth dam. We are pleased to hear this. For Nuclear Power plants, F=1.5 against liquefaction is required. Professor Seed in his graduate lectures at Berkeley which I was priveleged to attend in 1977 suggested F=1.5 is appropriate for critical structures and F=1.2 is appropriate for ordinary structures. However, there are other factors such as the induced strain levels that should be considered.

If liquefaction would result in unlimited strains, then failure would result and F=1.5 would seem appropriate. If liquefaction is accompanied by limited strains then a lower factor of safety can be accepted. The cyclic triaxial tests on block samples gave very large strains on liquefaction and hence a factor of safety significantly lower than 1.5 would not seem advisable.

The acceptable F value should reflect how well the design earthquake and the dynamic resistance of the soil is known. It should also reflect the consequences of failure. In the case of Revelstoke dam, the town of Revelstoke, population 6000, is located 3 miles downstream and a failure of the dam would be catastrophic.

The acceptable F should also reflect the cost. If it would not cost very much to increase F, then perhaps we should go for the higher F and reduce the risk. Since the factor of safety involves a number of judgemental factors perhaps a statistical approach rather than a deterministic approach would be more appropriate.

The moderator also suggested that our evaluation of liquefaction potential involved 2 separate approaches; an analytical approach and an empirical approach. This is not so, only one approach was considered as follows: The dynamic stresses caused by the earthquake were computed from an equivalent elastic analysis and the dynamic resistance was evaluated from the normalized penetration value, N_1. The dynamic resistance from triaxial tests was also examined but was considered low because of disturbance and was not used. The factor of safety was computed as the ratio of the dynamic resistance to the dynamic stress.

The dynamic resistance was obtained from N_1 values and Seed's liquefaction chart (Figure 6). The lines on his chart represent a lower bound and hence could be considered to already have a factor of safety built into them in the same way the Terzaghi-Peck settlement charts have. Hence our actual factor of safety will be higher than we record.

Dr. Prater is basically concerned about our evaluation of liquefaction resistance of the natural sand. Liquefaction resistance values were obtained from both laboratory cyclic loading tests on "undisturbed" samples, and from standard penetration values. The laboratory tests gave high values of resistance for samples taken from beneath the upstream slope. The difference in resistance obtained was considered to be mainly one of disturbance. The downstream sands are more silty and consequently less sensitive to sample disturbance.

It was considered therefore, that the laboratory tests particularly on samples from the upstream side gave unrealistically low values of resistance. In this regard, Dr. Peck (1979) has speculated that:

(1) Unless the cyclic loading tests used to evaluate liquefaction potential can be performed on absolutely undisturbed samples, which is manifestly impossible, the results will probably indicate too great a likelihood of liquefaction; and (2) in many instances the resistance to liquefaction in the field may be appreciably, even spectacularly, greater than that determined on the basis of conventional cyclic laboratory tests on reconstituted or even "undisturbed" samples if no allowances are made for the various possible beneficial effects such as time, repeated small shearing forces, and stress history.

It was therefore decided to base the liquefaction resistance of the in-situ sand from normalized standard penetration resistance values, N_1, and field experience as represented by Seed's chart (Figure 6 of our paper). The weighted average N_1 values used are based on 306 tests upstream and 268 tests downstream. It is considered that resistance values obtained in this way will be more reliable than results of laboratory tests which must be corrected for disturbance, aging, etc.

Dr. Prater comments that "Further, the belief that liquefaction resistance is higher for sloping ground conditions is a fallacy." Dr. Prater presents no positive evidence for his comment. On the other hand the simple shear test results of Vaid and Finn (1979) clearly show that for sands of high relative density, a very significant increase in the dynamic resistance occurs in the presence of a static shear stress or static bias. The increased resistance can be expressed in terms of a static ratio factor, R_{st} and is shown in Figure 1. It indicates that if the static stress ratio τ_{st}/σ'_o=0.1 rather than zero as it would be for level ground, the dynamic resistance will be increased by about 1.4 above its value or level ground. Figure 1 is appropriate for sands of 70 percent relative density.

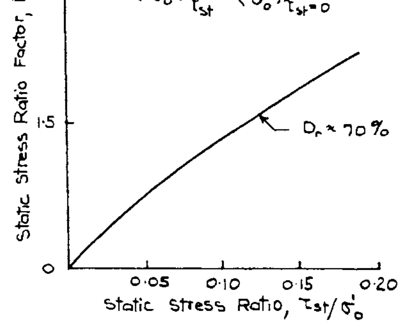

Figure 1 Effect of Static Stress on Dynamic Resistance.

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Closure by Fu Shengcong, and Jiang Jingbei.

The authors appreciate the comments made by Mr. William Y.J. Shieh. The following corrections should be made to the original paper: The right of the formula (1) should be $2\xi_j w_j \dot{x}_g + w_j^2 x_g$ instead of $2\xi_j w_j \dot{x}_j + w_j^2 x_g$. The line 4 below the (1), $\dot{x}_j$, x_j should read $\dot{x}_g$, x_g. The damping ratio of $|H(ip)|^2$ in the formula (5) should be ξ_j **instead** of ξ . The formula (7) should be

$$P(\eta) = \frac{1}{\sqrt{2\pi}}\left[Ee^{-\eta^2/2E^2} + \eta(1-E^2)e^{-\eta^2/2}\int^{[\eta(1-E^2)/E} e^{-\ddot{x}/2}d\ddot{x}\right]$$

Closure by G. Post.

I have no comment on W.Y.J. Shieh's discussion. I agree that the other means of evaluating the potential deformation produced by an earthquake is to study the strain potential in an embankment.

AUTHOR'S REPLIES

Closure by J.L. VonThun and C.W. Harris.

The authors thank Mr. Srivastava for his discussion. The questions raised refer to the example problem at the end of the paper. The specifics of the problem were chosen to illustrate the versatility of the computer code and do not represent actual knowledge of material properties. The query concerning the amount and rate of degradation of the effective friction angle upon shear movement raises an interesting point. The authors do not at present know of any procedure to evaluate the reduction in effective friction angle, however, anticipate that the required corelation may be determined in the future.

Srivastava states that the cohesive strength of the material is not accounted for and intimates that this may be due to loss of cohesion upon movement.

In fact, the opposite is true. The cohesive strength is fully accounted for in the excess available resistance of equation two and it is assumed that displacements do not alter the cohesive strength. The frictional strength may be reduced by varying the effective friction angle.

The computer code allows the user to activate any reasonable combination of options for excess pore pressure, hydrodynamic forces, shear strength reduction and dilation. Several of the parameters required for the options are not readily available in the literature or through current laboratory procedures. Continued research to refine the input parameters is planned.

Closure by M. Oner and M. Erdik.

The figure showing the computed and observed mode shapes refer to Keban Dam, the larger of the two dams tested.

The expressions for the fundamental period, Eqs. (5) and (6) are indeed dimensionally incorrect as they appear in the text. But this is due to the square-root signs ommitted in typing. The editor has already been informed on this error, and an "Errata" should appear in Vol. 3 of the Proceedings. The reason why the authors prefer the use of AF factor is that it is dimensionless while K_2max has a complicated dimension in British units.

Closure by R.C. Sonpal.

The authors thank A.T.F. Chen for taking interest in the paper.

Briefness of presentation was directed by the Conference Organizers.

The study of the pore pressure pattern during an earthquake was oriented towards only the qualitative estimation of relative deformations at different locations within the dam body.

Closure by J.V. Williamson and M.E. Schaffer.

Author agrees with the discussion by Dr. E.G. Prater.

AUTHOR'S REPLY

Closure by Andrian Moroinau and V. Perlea.

Authors Replies : 1. Theoretically, the determining of the probable damages corresponding to the high level earthquake, could be sufficient, but there are many reasons to consider the mean level too. A few of them are presented in the following.

There are many uncertainities both in determining the characteristics of the ground motion during a strong earthquake, and in determining the damages caused by such an event. Designers are not acquainted yet with an approach like this, especially because nor always experiment can be done ; even if someone succeeds in doing it, there are many difficulties in modelling the behaviour of the structure up to the failure due to earthquake loading. The experience gained during strong earthquakes is also reduced.

Therefore, current norms, as Mr. Srivastava pointed out, consider only a mean level for which the structure has to mantain his integrity. Current practice, as it resulted from the Proceedings of the last International Congress on Large Dams, is to consider the two levels, the mean one for which no damage is allowed, and a stronger one for which limited damages are allowed. The considering of a mean level of the seismic action is suitable because the behavior determined by calculus, corresponding to this level, can be compared with the actual behavior of the structure, such earthquakes occuring many times during the life of the structure. One can find presented in the literature too the actual behavior of dams during weak or mean earthquakes.

2. The influence of the pore pressure build-up was not taken into account, because the parameters defining the behavior of the rockfill to cyclic load could not be determined experimentally. In the case of rockfill dams it is usually considered that the pore pressure increase during an earthquake is not significant.

Owing also to the lack of experimental data, different values of the deformability characteristics were considered, in order to cover the range of the values presented in the literature. A more detailed calculus than usually was done, because Siriu dam is the first large dam situated in a seismic area which was realized in Romania ; that is why many studies on it were performed, such as that accomplished by Prof. Priscu and his co-workers.

It is a common practice to use the accelerations obtained by a calculus in elastic range with the stability analysis which supposes a nonlineare behavior. One can accept this approach, considering that the limit equilibrium methods by which the stability analysis is carried out, are conventional, as well as the allowable safety factors established from experience.

3. As Prof. Seed pointed out, hydrodynamic pressure of the water on the upstream face of the rockfill dams is negligibile, owing of the gentle slope. This results theoretically (Napetvaridze's, Zangar's formulas) for an undeformable dam, and resulted too from the calculus which was carried out by the authors taking into account the deformability of the dam. As a consequence, no sensible difference between the accelerations determined with or without taking into account the presence of the water in the reservoir did appear.

The dependence on confining stress of the angle of internal friction for rockfill, expresses the fact that, for this material, the envelope of failure Mohr circles is no more a straight one but a curvilinear one, like that of concrete, the slope of this envelope decreasing with increasing normal stress. Many experiments presented in the literature confirm this.

4. The location of weak material which was chosen, near the core both in the upstream and downstream shells, could be unfavorable from some reasons, like that of an increasing deformation of the core. But the authors think that in the studied case the stability considerations must prevail, the location which was chosen being the most adequate from this point of view.

5. The authors agree with Prof. Seed and express their gratitude for his valuable intervention.

6. The authors do agree with Mr. Srivastava that it is preferable that the characteristics of the material and of the seismic action should be better known, in order to reduce the scatter of the admited hypotheses, but unfortunately this is not always possible, and they had often to appreciate the safety of a structure without knowing very well these characteristics.

AUTHOR'S REPLY

Closure by Y.K. Lin, K.V. Rodda, C.W. Perry, and D.K. Gill.

The authors appreciate the valuable comments by Z.J. Shen, J.N. Srivastava and W. Shieh.

Srivastava questioned the validity of comparison between the results obtained by various methods. The authors are aware of the fundamental differences in these methods. Therefore, the correlation was not intended to be an universal one. As indicated in the paper, the correlation only applies to a group of dams which are similar in design and construction, and founded on the same geological complex having similar seismicity. The comment that the results of comparisons could be different for different dams is well recognized. Although correlation in this manner (namely, on a specific group of dams) is somewhat limited, it still can achieve significant saving in the cost of engineering evaluation.

Regarding Shen's comment that "it is the deformation-connected cracking but not the deformation itself that damages the dams", the authors do not have any disagreement with this statement. However, this does not discourage one's interest in evaluating the seismic-induced deformation of dams because it can be used to evaluate the adequacy of the remaining freeboard following the earthquake. In addition, the deformation-connected cracking depends greatly on the magnitude of seismic-induced deformation.

All three discussors commented that the displacement obtained from the finite element program DEFORM based on strain potential does not represent permanent deformation. The authors agree that the above finite element method would tend to somewhat overestimate the displacement because the strain used in the strain potential computation is partially reversible. However, it should be noted that the initial static stress condition in the major portion of the dam embankment has various degrees of anisotropy. It is also known that the strains induced in the laboratory by cyclic loading on samples consolidated and confined under anisotropic stress conditions are mostly permanent strains. Therefore, the strain potentials computed for the dam embankment based on the laboratory test results are mostly irreversible. In other words, the differences between "true" permanent displacement and the displacement computed from the DEFORM finite element procedure are not as significant as what the discussors speculate. Therefore, the good agreement between the finite element method and simplified methods cannot be simplistically concluded as being coincidental.

One of the advantages of using the finite element method to evaluate embankment deformation is that it gives the entire deformation pattern in the embankment. This is another good reason to use the correlative techniques for embankment deformation evaluation. The simplified procedures only give horizontal displacement, whereas the results of finite element analysis can be used as a basis to estimate the vertical deformation at the dam crest.

Closing Remarks by A.S. Lucks, Co-chairman.

Professor Seed's state-of-the-art presentation, the moderators report, and the papers included in the proceedings of this session provide an excellent overview of the approaches that can be taken to analyze the difficult problems associated with the stability of slopes and embankment dams subjected to earthquake loadings. In this summary I would like to comment on three areas.

I found it interesting to note the number of papers that, in one manner or another, made use of the Newmark method for determining seismically induce permanent displacements. Professor Newmark first presented this method of analyses in his Rankine Lecture of 1965, but I think it is true to say that it did not receive much attention until Professor Seed and his co-workers published the results of their studies some 13 years later. We sometimes become caught up with the use of more complex and involved computer analyses at the expense of benefiting from the use of more simple, but very elegant solutions such as the one presented by Professor Newmark.

In similar vein, it is true to say that major failures generally result from a significant failure mechanism being overlooked, conditions exposed during construction being ignored, or what is thought to be an insignificant design change not being fully evaluated. In this respect dam designers are not alone. From reading recent reports of the failure of the Alexander Kneiland platform, the apparent triggering mechanism was a "minor" modification that was required to add additional equipment. I would therefore, point out that in most cases it would be wise to strive to attempt to analyze numerous possible failure mechanisms rather than becoming committed to fewer involved and complicated analyses that quickly use up all of our time and resources. By maximizing the number of different cases that can be analyzed we can possibly stay one step ahead of Murphy! It would seem that centrifuge testing may be very useful in identifying some of the failure mechanisms that we might not otherwise anticipate.

The anticipation of possible failure mechanisms is particularly difficult with respect to the analyses of dam abutments in jointed rocks masses. Last year I was lucky enough to have the opportunity to visit two major hydroelectric projects in the Peoples Republic of China. One of these projects was currently being investigated and the other was in the early stages of construction. I was immediately impressed by the scope of the geologic investigations that had been carried out. At one project over 40 test adits had been driven in the dam abutments and the geologist had practically mapped every joint. Due to the economic pressures in this country I do not think we will be able to match the level of detail achieved in their geologic investigations. In this respect I was very pleased to see engineers from the Peoples Republic of China participating in this conference. I feel that we will all benefit from the case histories that will eventually come forth from these extensively investigated projects. The results will be useful in planning the strategy for our less ambitious exploration programs.

Finally, I was pleased to see the papers by Logani and by Williamson and Shaffer that describe defensive design measures that can be used for dams in seismic areas. I think we would do well to review the incorporation of at least a few of these details in our designs no matter what our analyses tell us.