TECHNICAL SESSION No. 1—STRENGTH BEHAVIOUR

"Residual Strength Determination in Direct Shear", R.M. Cullen & I.B. Donald.

"Undrained Shear Strengths in Clays", R.H.G. Parry.

"Effects of Salt Content on Thixotropic Behaviour of a Compacted Clay", Surachat Sambhandharaksa & Za-Chieh Moh.

"The Deformation and Yield of Clays in Direct Simple Shear", L.K. Walker.

"Brittle Fracture of Rock at Low Confining Pressures", E.T. Brown.

"The Prediction and Measurement of the Long-Term Strength of Rock", D.P. Singh & W.E. Bamford.

"Liquefaction of Saturated Granular Soils", M. Kurzeme.

The Collapse of Sands Upon Saturation", P.J. Moore & D.V. Millar.

GENERAL REPORTER - Mr. P.L. NEWLAND:

The authors of the paper on "Effects of Salt Content on Thixotropic Behaviour of a Compacted Clay" make use of variables which are not clearly defined but appear to be as follows:-

Salt content%	=	wt. of salt per 100 gm. of dry soil.
Dry unit wt.	=	wt. of dry soil plus salt per unit vol. of wet soil.
Water content%	=	wt. of water per 100 gm. salt plus dry soil.

The results in the authors' Fig. 1 have been replotted in Fig. D1 using more conventional definitions of dry unit weight and water content, in both of which the weight of salt is excluded. It will now be seen that the results present an entirely different picture.

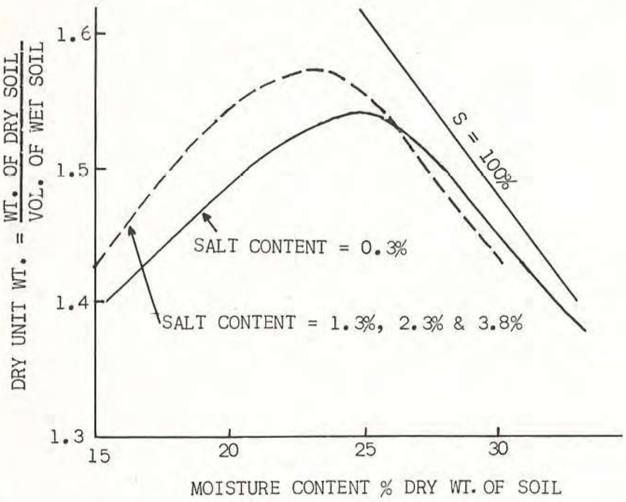


Fig. D1. - Compaction Curves for Different Salt Contents.

The quantity, salt content, is retained in Fig. D1 but a preferable quantity would be salt concentration (as used in the later section of the paper) which refers to the weight of salt per litre of pore water. Thus, in Fig. D1 a salt content of 3.8%, the salt concentration varies between about 250 gm./l. for a water content of 15% and 110 gm./l. at a water content of 35%.

Since the authors analyse their results in great detail and provide very complex explanations in terms of interparticle forces, etc., for the observed behaviour, the appropriate choice of variables is a subject for discussion.

Dr. Walker's paper evaluates the results of simple shear tests on Kaolin in terms of the theory associated with the concept of a unique state boundary surface.

The major part of the paper presents results on both normally and over-consolidated samples which seem to accord fairly well with that part of the theory concerned with the principle of a unique surface. There is, however, one major discrepancy, namely in Fig. 8 where the results of constant τ_{xy} tests are presented. A curve linking the initial points of these tests, i.e. ABFEDH, diverges considerably from the curve AX for the state boundary surface. This will be seen by reference to Fig. D2. It would be interesting to know if the duration of the consolidation increment preceding application of τ_{xy} in these tests was long enough to allow some secondary consolidation to occur, since similar behaviour has been observed by the Reporter under such circumstances.

Also shown in Fig. 3 are the results of simple shear tests carried out by Thurairajah (Ref. D2) on the same Kaolin as that used by Dr. Walker, together with the curve predicted by Roscoe and Burland (Ref. D1) on the basis of their modified Cam-clay theory. It would be interesting to hear the author's comments on this.

The final section of the paper deals with the all-important stress-strain behaviour on which the main justification for the Cam-clay theory rests. Unfortunately, this section is considerably condensed and therefore difficult to follow. This is compounded by a reference to the author's Ph.D. thesis.

Focusing attention on the normally consolidated state, the author states among other things that

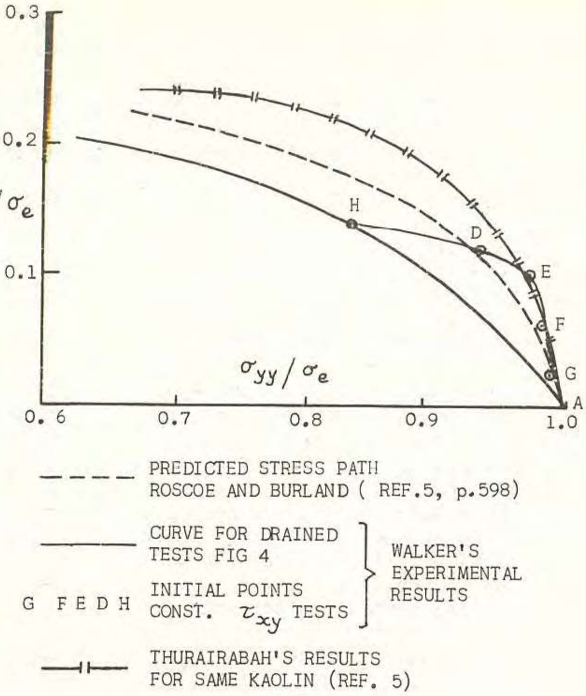


Fig. D2. - Results of Simple Shear Tests on Normally Consolidated Kaolin.

essentially the same stress angular strain curve is obtained in both drained and undrained tests. This is certainly a surprising result, particularly in view of experience with the results of triaxial tests. It would be of considerable interest if the author could present the results for the undrained tests so that they may be compared directly with those for the drained test in Fig. 10 in the paper.

The above observations, if true, certainly show up a deficiency in the Cam-clay theory. On the other hand, Roscoe and Burland (Ref. D1) demonstrate reasonable agreement between results predicted on the basis of their modified theory and the results of drained tests (Ref. D2) which are very similar to those presented by the author over the range concerned. Perhaps these anomalies may be resolved by discussion.

Dr. Kurzeme presents an interesting review of the literature on the liquefaction of saturated granular material. It will be seen from Table II that the type of loading simulated in tests on laboratory-sized samples falls in two categories; shear waves in the first and compressional waves in the second.

Most of the workers cited seemed to have concentrated on the first of these categories. Without exception, the ambient stresses in their tests were either hydrostatic (triaxial tests) or corresponded to the K_o condition (simple shear). No attempt was made to investigate the effect of higher stress ratios to simulate the existence of a building. This is rather surprising since it requires little reflection to realise that quite profound changes in behaviour might result. For example, loose samples might be expected to undergo quite large strains once the effective stress had fallen sufficiently to allow shear failure to occur. This would precede the state of general liquefaction where the effective stress had fallen to zero.

The above deficiency has probably arisen from a preoccupation with the rather dramatic phenomenon of liquefaction; from a practical point of view, however, it is the deformations associated with earthquakes that are of primary importance, as Schroeder and Schuster (author's Ref. 18) point out. These workers concerned themselves with the second category of loading, namely compressional waves. The technique used by them to simulate these waves was to vary the cell pressure sinusoidally in a triaxial apparatus with a $\frac{3}{4}$in. dia. ram. The net effect of this was to impart a varying deviator stress to the sample in the vertical direction because of the varying upthrust on the ram. Shear stresses are, of course, the only stresses which can cause changes in the effective stresses under the undrained conditions which are assumed to prevail during the transient passage of earthquake waves. If the above workers had placed more significance on this aspect, their investigation may have taken a different direction. However, their tests did cover a range of initial stress ratios and they demonstrated that large deformations occurred in certain circumstances without the attainment of general liquefaction.

It would appear that a profitable direction for future research would be to investigate the axial deformation behaviour of triaxial samples under differing stress ratios with cycling shear stresses either vertically in compression or horizontally by displacing one end of the sample relative to the other. Neither of these avenues appears to present great difficulty from the experimental point of view.

With regard to the phenomenon of collapse which forms the subject of the next paper the causes of contact instability following inundation of an unsaturated soil are twofold:-

(1) A decrease in the normal force leading to a decrease in the resistance to shear, provided there is no accompanying increase in the coefficient of friction, and

(2) a net decrease in the shear resistance.

The second case involves two possibilities, namely, a decrease in the coefficient of friction of the solid material or a destruction of the resistance to distortion provided by the lenticular film of water at particle contacts.

As far as the first of these latter possibilities is concerned, if the added moisture already coats the particles before they are brought into contact during preparation of the sample, it is difficult to imagine that the coefficient of friction of the solid material is not already modified, if at all, by the presence of the film of water. It is unlikely, therefore, that the process of inundation will produce any further change.

On the other hand, if the sample is first assembled dry and water then allowed to condense around the contact points, instability would arise only if the water penetrated the points of contact and in so doing reduced the coefficient of friction. In this case, of course, collapse would occur in the unsaturated state. If collapse did not occur, there is no reason to suppose that inundation would change the situation.

Thus meniscus effects remain as the sole source of whatever volume instability there is due to inundation.

The important question of how the samples were prepared is not clear from the authors' paper. Fig. 6 shows all the water located around the contact zone but this would depend on the method of preparation. This may not be significant if the effective stress is relatively insensitive to water content in the lenticular state, as Fig. 7 shows, although it is not clear how Eq.(6) was derived.

The equivalent effective stress calculated by the authors as being due to the lenticular water is admittedly small. However, there is no question that removal of this stress will qualitatively lead to collapse. The question is how much, since the change in effective stress can provide no indication. Some light might have been thrown on this question if tests had been carried out with different values of σ'.

The above comments have been concerned with the phenomenon of collapse under conditions of constant (zero) externally applied shear stress during the change in water content. The question of shear strength is another matter again since particles are constrained to move by an increase in the externally applied shear stress.

The method of sample preparation in the unsaturated state clearly now becomes of importance. There is also the matter of what happens to the menisci when particles move relative to each other (the work of Horne and Deere cited by the authors might well be reviewed in the light of the above statements although they do not clearly set out their method of preparation).

Can the authors explain the astonishingly large change in shear strength of the saturated sand with pre-shear void ratio (ϕ' varies from about 21° to as low as 11°)? A further question relates to the variation in e at failure (0.81 to 0.76). All the samples appear to have been prepared at an initial voids ratio greater than the critical so that one would have expected they would reach much the same strength and voids ratio at failure at a single value of σ'.

Just what the difference in strength between the "moist" and saturated samples means under these circumstances is difficult to assess. An interesting test would have been to inundate the moist sample after shear under the prevailing stresses and observe the ensuing collapse if any.

In Dr. Brown's paper on the brittle fracture of rock at low confining pressures, there appears to be an error in the last paragraph on p. 31 in the classification of rock types. The unloading curve for class 1 rocks should lie to the right of ACD and class 2 to the left.

The author's main concern is with the measurement of the post-peak stress-strain behaviour of rocks, particularly those in class 2 where failure tends to be explosive. He describes several methods by which this can be achieved with varying degrees of success.

In discussing the dense ultra-fine grained rocks in class 2(b), the author states "If violent fracture is to be prevented, large amounts of energy must be extracted from the specimen very quickly and this has not been achieved to date". A possible solution to this might be obtained by allowing the confining pressure to rise rapidly in a closed stiff system in response to lateral deformation. This could be combined with the lateral displacement monitoring described in the paper or the pressure rise itself could be used for monitoring.

By contrast with the previous paper, the paper on the long-term strength of rock is concerned with the pre-peak stress-strain properties. For one who is more familiar with the properties of soils, it is pertinent to ask, with reference to Fig. 1, whether the volumetric strain and pore pressure curves are obtained in a single test or whether they apply to tests with differing drainage conditions. If the former is the case, is there some mechanism such as partial saturation to explain this?

With regard to the prediction of the long-term strengths, the loading rate, stress-strain and log stress-log strain methods involve the same two variables, stress and strain. In principle, therefore, the methods should give the one result. The differences recorded in Table I must surely arise from the graphical procedure in making the determination. Greater precision may be claimed from a given plot but this is not necessarily synonymous with accuracy.

The volumetric strain method gives higher predicted long-term strengths than those obtained using the above methods. The authors used samples with a 3:1 length-to-diameter ratio to minimise end effects but seemingly no special measures were taken to reduce end friction. It is possible that the microfracturing which contributes to lateral deformation is particularly sensitive to end restraint. It is also interesting to note that the departure of the transverse deformation curves from linearity in Fig. 4 occurs considerably earlier than in the case of load curve. The predicted long-term strength would be about 85% on this basis which is closer to the measured long-term strength than the 88% from the stress-strain method.

References:

D1. ROSCOE, K.H. and BURLAND, J.B. - On the Generalised Stress-strain Behaviour of "Wet" Clay. Symposium on Engg. Plasticity, Cambridge University, 1968.

D2. THURAIRAJAH, A. - Some Shear Properties of Kaolin and of Sand. Ph.D. Thesis, Cambridge University, 1961.

Paper by SURACHAT SAMBHANDHARAKSA and ZA-CHIEH MOH:

The Authors in Reply:

The General Reporter has raised a basic question about the definitions of terms used in the paper. Since the soils considered in the paper consisted of three components, i.e. the soil particles, the salt, and water, any definitions adopted for defining the unit weight of the soil and the "moisture content" must be compatible and consistent with each other. There are three possible ways to define these terms as shown below

(1) Salt as part of solid:

$$\text{Dry unit weight} = \frac{\text{wt. of dry soil + wt. of salt}}{\text{total volume}}$$

$$\text{Moisture content} = \frac{\text{wt. of water}}{\text{wt. of (dry soil + salt)}}$$

(2) Salt as part of fluid:

$$\text{Dry unit weight} = \frac{\text{wt. of dry soil}}{\text{total volume of soil}}$$

$$\text{Volumetric moisture content} = \frac{\text{volume of (water + salt)}}{\text{volume of dry soil}}$$

(3) Salt not included:

$$\text{Dry unit weight} = \frac{\text{wt. of dry soil}}{\text{volume of soil}}$$

$$\text{Moisture content} = \frac{\text{wt. of water}}{\text{wt. of dry soil}}$$

The first two were the ones used by the authors in the paper. The third set is the one suggested by the General Reporter. Although the latter terms appear to be the so-called conventional definitions, they have excluded an integral component of the system, i.e. the salt. In fact, when engineers deal with marine clays, no-one excludes the salt present in the soil when computing the dry unit weight and moisture content; usually the first set of definitions is being used.

Despite the differences in opinion about the definitions to be used, the effects of salt content on the compaction characteristics of the clay studied are still the same as shown by comparing Fig. D3 with Fig. 1(a) and 1(b). There appears to be some mistake in the General Reporter's plot, perhaps because he did not have the access to the original data.

The salt content can be either expressed by percent weight of the dry soil or as concentration in the pore water. It is just a simple conversion between the two. In a number of the graphs the authors have shown both. To the practising engineers, and in particular, when one deals with compaction, it appears to be simpler if the salt content is expressed in percent by weight.

Fig. D4 presents the micrographs of three samples, all compacted at a moisture content on the dry side of the optimum but with different salt content. These pictures which show differences in the particle orientation as well as the inter-particle contacts due

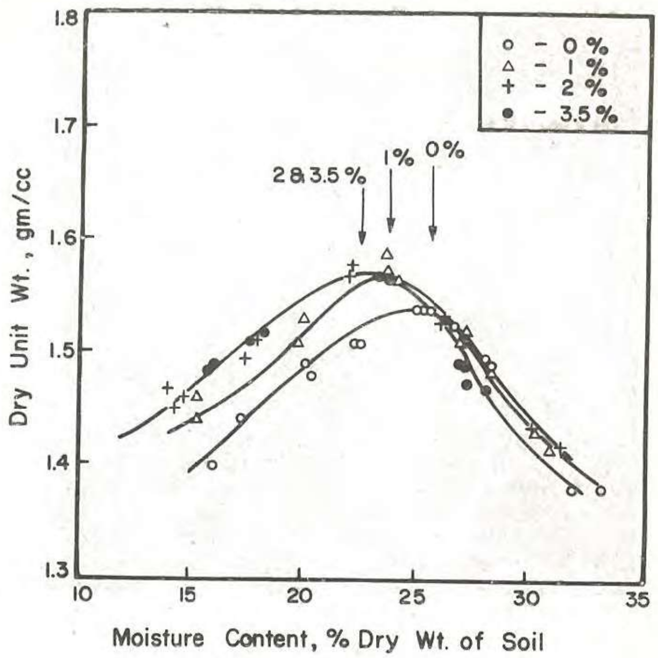
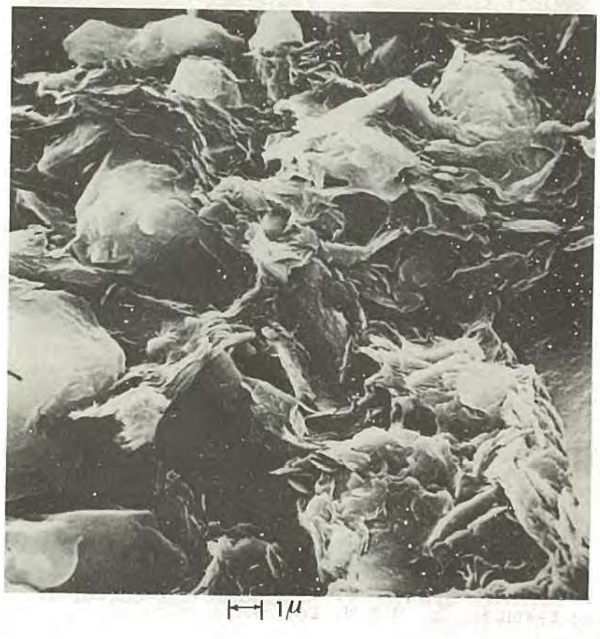


Fig. D3. - Effect of Salt Content on Compaction Characteristics of Bangkok Clay - excluding the weight of salt.



(a) Salt Content = 0.3%.

Fig. D4. - Electron-micrographs of Salt-Treated Bangkok Clay.

(b) Salt Content = 2.7%.

(c) Salt Content = 3.7%.

Fig. D4. - Electron-micrographs of Salt Treated Bangkok Clay.

to different salt content lend some support to the arguments put forward by the authors. The micrographs were taken by Mr. H.P.W. Polding of the University of Salford, Britain.

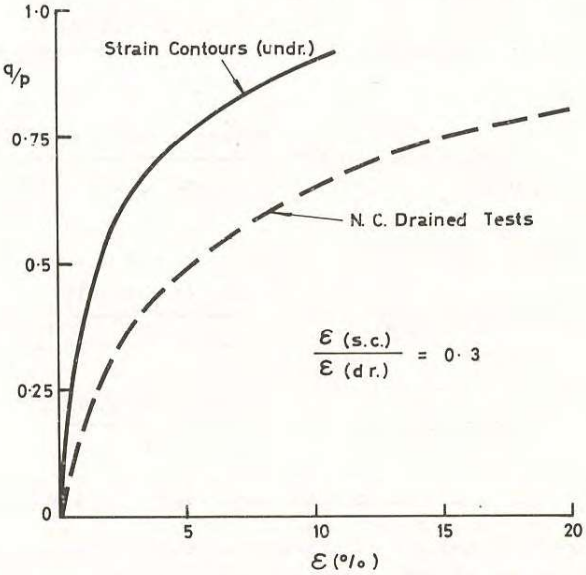
Paper by L.K. WALKER:

The Author in Reply:

The General Reporter has touched on a significant feature of the author's results, viz. the comparison between stress-strain curves for drained and undrained tests. The author has clarified this point by presenting in Fig. D5 below S.S.A. test data replotted from the paper, together with triaxial test data published by Burland (1967). The purpose of the figure is to illustrate the relative significance of the two components of shear strain discussed in the paper, these being

(a) a strain contour component which is independent of plastic volumetric strain. This can be obtained directly using drained tests on overconsolidated samples, or indirectly by using the undrained stress-strain curve.

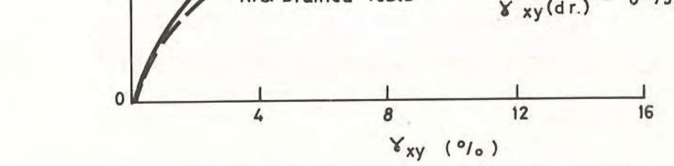
(b) a shear strain component related to plastic volumetric strain by a flow rule similar to that shown on Fig. D5.



TRIAXIAL TEST DATA (BURLAND 1967)

Triaxial Flow Rule (Burland 1965)

$$\frac{dv^p}{d\varepsilon^p} = \frac{M^2 - (q/p)^2}{2\,(q/p)}$$



S.S.A. TEST DATA (WALKER 1967)

Fig. D5. - Effective Stress Ratio - Strain Data for Kaolin.

The data plotted on Fig. D5 show a comparison between the strain contour component, and the total stress-strain curve obtained in drained shear tests on normally-consolidated samples. For the S.S.A. data, the strain contours have been taken from drained shear tests on overconsolidated samples and represent about 75% of the total shear strains. This figure would be lower (approx. 60%) if the undrained stress-strain curve had been used.

For the triaxial test data, the strain contour component has been based on the undrained stress-strain curve. This component represents, on the average, 30% of the total drained shear strains.

There are two major conclusions emphasised by the data, bearing in mind that the triaxial data are considered to be subject to less scatter caused by experimental factors. It is suggested (as in the paper) that the S.S.A. data show little difference between drained and undrained stress-strain curves. The scatter in individual test results overlaps the respective mean curves. The triaxial data, on the other hand, show the usual major difference between drained and undrained curves.

The reason for the different behaviour between the two test methods can be ascribed qualitatively to the different stress paths, which induce greater plastic volumetric strains in the triaxial test than in the S.S.A. test. The mathematical support for this proposition is contained in the paper by Roscoe and Burland (1963). The test data in Fig. D5 suggest that the application of plasticity theory as developed at Cambridge is of limited value for samples undergoing plane strain, as the theoretical strains so predicted form a small part of the total drained strains.

Paper by D.P. SINGH and W.E. BAMFORD:

The Authors in Reply:

The answers to the General Reporter's questions are as follows: The volumetric strain and pore pressure curves may be obtained in a single test, using appropriate apparatus (see A.S.T.M. Special Technical Publication 479, 1970, pp. 604-612). The initial increase in pore pressure reflects the closure of cracks and pores under increasing pressure; the decreased pore volume leads to increased pore fluid pressure. After the onset of unstable fracture propagation the rapid increase in microcracking leads to an increase in pore volume, and so to a decrease in pore pressure.

The Reporter's comment regarding the differences recorded in Table I is partially covered by paragraph 3 on page 41, and it is agreed that the graphical procedure can lead to errors. In principle, the log stress-log strain method should minimise these errors, as changes in curvature of the pen records should show up as changes from one straight line to another: by extrapolating from points on each straight line section the transition point should be precisely determined, even if no record was made of this point during the process of transcribing values off the recorder chart.

No measures should be taken to minimise end friction of rock specimens other than to polish the ends of the specimens on a surface grinder. Use of soft capping material, having lower deformation modulus than the rock, has been shown to greatly reduce the apparent strength of the rock. It has also been shown recently that the failure process in a cylindrical specimen initiates at the contact with the platens , but that provided the length/diameter ratio is greater than 2, the transverse deformations measured at the centre of the specimen are "true". The departure of the transverse deformation curves from linearity, as shown in Fig. 4, should indicate the point where the volumetric strain curve departs from linearity, as in Fig. 1. That is, it coincides with the fracture initiation stress level, and the onset of stable fracture propagation. It should not, according to Bieniawski's hypothesis, also be the long-term strength. The apparent coincidence in the case of the marble specimen cited cannot be explained except by invoking natural variability, and presuming that the marble specimens tested under sustained loading were a few percent weaker than the marble specimens tested under "quick" compression.

No criterion of fracture was used to determine the sustained-loading strength: direct observation of the specimens showed that abrupt brittle fracture occurred, with the failure modes (mainly shear failure) being usually identical with those observed in the short-term compression tests. None of the long-term tests were terminated before failure occurred. One of the loading frames was used for tests which were expected to take more than a week to complete, while the other was used for sustained loadings at stress levels which it was expected would cause failure after less than one week. The longest sustained loading test completed to date was of 3 months' duration.

Paper by M. KURZEME:

Discussion by J.H.H. GALLOWAY:

A good deal of interest in Dr. Kurzeme's review of the problems of liquefaction of sands has been apparent in the oral discussion. This interest st shows a growing awareness of earthquake problems in Australia but it seemed to me that there is a tendency to let emotion cloud the issue. Like any other engineering factor, earthquakes should be assessed objectively so that neither unwarranted risks nor excessive costs arise. It is not easy to decide what risks are unwarranted or which costs are excessive but a recent paper by Hollings (Ref. D3) gives some useful guidance. Working from simple basic concepts Hollings shows that blanket building code requirements, such as those in force in New Zealand, may provide unnecessary protection, or good protection at reasonable cost, or be almost totally ineffective depending on the circumstances of the case. By applying these concepts to each case a rational choice of protective measures can be made.

Reference:

D3. HOLLINGS, J.P. - The Economics of Earthquake Engineering. Proc. N.Z. Nat. Conf. on Earthquake Engg., Wellington, May, 1971.

Discussion by C.T.J. BUBB:

The author has discussed prediction rather than prevention of liquefaction but a careful reading of the paper hints at surcharging as a possible means of prevention rather than densification which is usually suggested.

Ambrasey and Sarma (Ref. D4) have suggested increasing the total normal stress and thus - as can be seen from the author's Eq. (1) - reducing the likelihood of liquefaction or substantial loss of bearing capacity for a given increase in pore water pressure that is for a given seismic event.

They suggested that this be done by surcharging the whole site with a permanent blanket of free draining granular fill.

Atkinson (Ref. D5) has further suggested that, as the aim is merely to increase the total normal stress, the free draining requirement might not be significant.

Provided the change in level of the site is acceptable it would seem to offer a very economical means of avoiding damage from full or partial liquefaction. In addition on coastal sites where tsunamis such as recently experienced in New Britain are to be expected the consequent elevation of the site might be a further advantage.

The author refers to the Kingston, South Australia, earthquake of 1897. In view of the fact that there have been no other events detected in the last 10 years or reported for the previous 70 years the writer has suggested elsewhere (Ref. D6) that this might well be an example of sporadic as opposed to continuous seismicity as recently suggested by Richter.

Finally in view of the known very high and continuous seismicity of New Guinea has the author any programme for carrying out his suggested field experiments in liquefaction there?

References:

D4. AMBRASEYS and SARMA - Liquefaction of Soils Induced by Earthquakes. Bulletin Seismic Soc. America, Vol. 59, 1969.

D5. ATKINSON, J.H. - The Behaviour of Foundations During Earthquakes. M.Sc. Degree Report, Imperial College, University of London, 1970.

D6. BUBB, C.T.J. - On the Seismicity of Australia. Commonwealth Dept. of Works Publication, 1971.

The Author in Reply:

To Mr. J.H.H. Galloway:

The author agrees that the extent of any protective measures for reducing liquefaction damage should be assessed objectively. Two steps are involved. Firstly, whether the soils encountered are prone to liquefaction, could be investigated in-situ by the use of the standard penetration test or in the laboratory under cyclic loading conditions. Secondly, it is necessary to determine whether for the level of seismic activity found in the area in question protective measures to reduce liquefaction damage at the time of construction are an economical proposition. Two alternatives are either to ignore the possibility that damage may occur and make good the damage if it does occur, or to take out some form of insurance to cover possible damage during the lifetime of the structure.

Generally, in areas of high levels of seismic activity, the cost of actual damage and the resulting loss of service of a structure due to liquefaction during its lifetime appear to be high, if compared to the initial cost of protective measures. However, in the recognised seismic zones of Australia, the level of activity is very low by world standards. The return period of events of significant magnitude in these areas are long compared to the lifetime of engineering structures. It is therefore probably more economical to take out insurance to cover the cost of possible damage as opposed to taking engineering measures to reduce that damage.

A method such as that of Hollings, referred to by the discussor, would provide an objective assessment of the course of action to be taken under a particular set of circumstances.

To Mr. C.T.J. Bubb:

The use of a permanent surcharge blanket over liquefaction-prone soils would certainly reduce the damage due to partial or total liquefaction response resulting from a particular earthquake. The economics of surcharging as opposed to in-situ densification or alternative foundation methods, would be highly dependent on the particular circumstances encountered at a site. A decision on the economics of protective measures can be arrived at objectively as outlined earlier.

If surcharging is the most economical means of protection, and the site in question can tolerate the change in surface level involved, a free-draining granular fill is preferable. The primary purpose of a surcharge blanket is to increase the total normal stress, but with an impervious blanket, any rise in pore water pressure as a result of shaking will take a correspondingly longer period to dissipate.

With a loose, saturated granular material, any shaking will result in some rise in the pore water pressure, that in turn causes reduction in strength (see Eq. (1)). Consequently, if the surcharge blanket is impervious such a reduction in strength will persist for a longer period than if it were free-draining which, while avoiding total failure, may result in unacceptable total or differential settlements of structures.

In his verbal introduction the author referred to the Kingston, South Australia, Earthquake (Ref. D7) of 1897, to draw attention to an early description of liquefaction response on the Australian continent. The description is of interest because many of the indicators of liquefaction response were already given at a time when the term liquefaction and the mechanisms producing the phenomenon were unknown. The soils in the Kingston area appear to be prone to liquefaction, but from the discussors work the nature of seismic activity in the area seems to be sporadic. Engineering protection against liquefaction response in such an area would not be necessary.

Paper by P.J. MOORE and D.V. MILLAR:

The Authors in Reply:

Regarding the mechanics of collapse the authors are in complete agreement with the comments of the General Reporter, namely that if the added moisture already coats the particles during preparation then it is difficult to imagine that the coefficient of friction between the solid particles could be further modified by inundation. In fact this point is discussed in the paper. However the test data shows that further change did occur following soaking of initially moist samples. One possible interpretation of this is that not all contacts were wet initially.

The authors cannot agree that "meniscus effects remain as the sole source of whatever instability there is due to inundation". The General Reporter has not demonstrated this point and the discussion in the paper suggests that meniscus effects played a minor role with the samples tested.

A variety of preparation methods for the samples was tried before the method described in the paper was adopted. The moist sand was thoroughly mixed to produce a uniform distribution of the water throughout the sample before it was placed in the oedometer.

In reply to the General Reporter's question relating to Eq. (6), this was derived simply by satisfying vertical force equilibrium for all forces and pressures at the contact illustrated in Fig. 6. The derivation given by Donald (Ph.D. thesis, University of London, 1961) namely

$$\sigma' = \frac{\pi T}{2r\,(1 + \tan\,(\theta/2))} \quad \text{for open packing}$$

is in agreement with Eq. (6).

Other tests have been carried out at values of the vertical stress of 20 lb./in.^{2} and 45 lb./in.^{2} These test results showed similar types of collapse behaviour to those observed with the 10 lb./in.^{2} value of vertical stress. In all of these tests the change in equivalent effective stress due to dissipation of the capillary stress following soaking is very small in comparison with the vertical stress (less than 1%). It is difficult to see on the basis of the data presented how the General Reporter can claim that removal of the capillary stress will lead to collapse.

The authors cannot explain the large magnitude of change in shear strength shown in Fig. 9. With other tests that have been carried out the changes in strength have not been as marked but there was always a relatively lower strength with the saturated samples at the higher void ratios.

During discussion of the paper some verbal criticism was levelled at the direct shear test data, a sample of which was presented in Fig. 9. Because the lowest point plotted in this figure corresponds to an angle of shearing resistance of approx.11° it has been suggested that the friction of brass on brass was actually measured. Even if it is assumed that the two halves of the direct shear device were in contact, an evaluation of the criticism indicates that it is invalid.

A normal stress of 10 lb./in.^{2} over the area of the sand sample corresponds to a load of 55 lb. This load may be transmitted downwards through the sand or laterally to the upper half of the direct shear device. Test measurements show that the maximum load which could be transmitted to the upper half of the device is about 17 lb. which means that at least 38 lb. must be transmitted downward through the sand. If the full angle of friction for brass is assumed to be mobilised between the two halves of the device then a maximum possible shear stress of about 0.6 lb./in.^{2} over the area of the sand sample could be generated. With the normal stress of 10 lb./in.^{2} on the same sample this shear stress corresponds to a maximum of $3\frac{1}{2}^{\circ}$ (the actual value is certainly much smaller) and not 11°, which may be attributed to brass friction. This maximum frictional effect was found to be equally applicable to dry sand, moist sand and saturated sand, so the conclusions which have been drawn from the direct shear data,such as in Fig. 9, remain unchanged.

To confirm methods of predicting liquefaction response, the field experiments suggested in the paper will have to be carried out in an area of known, high and continuous seismicity and in soils prone to liquefaction, to obtain results within a reasonable period of time. New Guinea certainly has high and continuous seismicity and also areas of liquefaction-prone soils.

At this stage the problem of pursuing the suggested programme is one of cost, primarily due to difficulty of access.

Reference:

D7. GREGORY, J.W. - The Geography of Victoria. Whitcombe and Toombs, 1903, pp. 174-176.