Soil Properties and their Measurement

Mesure et Propriétiés des Sols

Mechanical Properties—Propriétés Mécaniques

Chairman / Président: G. P. TSCHEBOTARIOFF, *U.S.A.*

Vice-Chairman / Vice-président: J. L. ESCARIO, *Spain*

Assistant Reporter / Assistant Rapporteur: N. JANBU, *Norway*

Oral discussion / Discussion orale:

A. W. Bishop	*U.K.*
D. J. Henkel	*U.K.*
R. Parry	*U.K.*
M. J. Hvorslev	*U.S.A.*
D. H. Trollope	*Australia*
H. U. Smoltczyk	*Germany*
P. Habib	*France*
T. J. Osterman	*Sweden*
B. Jakobson	*Sweden*
T. K. Chaplin	*U.K.*
G. G. Meyerhof	*Canada*
T-K. Tan	*China*
E. C. W. A. Geuze	*Netherlands*
J. E. Jennings	*S. Africa*
K. S. Bawa	*U.S.A.*
T. W. Lambe	*U.S.A.*
P. W. Rowe	*U.K.*
T. K. E. Kallstenius	*Sweden*
S. J. Buchanan	*U.S.A.*
E. T. Hanrahan	*Ireland*
A. Kézdi	*Hungary*
J. Kolbuszewski	*U.K.*
R. V. Whitman	*U.S.A.*
P. J. Alley	*New Zealand*

Written contributions / Contributions par écrit:

M. E. Buisson	*France*
W. M. Kirkpatrick	*U.K.*
L. Šuklje	*Yugoslavia*

N. JANBU

Assistant Reporter, Division 1b / Assistant Rapporteur, Division 1b

The Chairman

I call first on the Assistant Reporter to give us his summary.

Assistant Reporter

To avoid too much repetition of the written report, and much more to the point to make your and my headaches as short-lasting as possible, I shall make only a few remarks concerning the proposals for discussion.

In the written report for this subdivision it has been suggested to concentrate the discussion on a few, even though comprehensive, topics concerning shear strength and deformation characteristics of soils. Moreover, it is understood that these topics should now be discussed mainly from an engineering point of view, as the more scientific approach to closely related problems has already been covered by the discussion this morning.

In his introduction to the discussion of the last International Conference, A. Casagrande said that in his opinion the shear strength of clays was the most difficult chapter in soil mechanics. In a general way it is believed that this situation may still prevail.

However, valuable papers on the subject have been published since 1953, and information derived from these papers appears to have helped to clarify to some extent the range of validity of existing shear strength hypotheses for clays. If so, it marks progress of substantial practical importance. The following considerations may serve as further illustration of this point.

Only a few years ago the undrained shear strength of clays (with effective stresses unknown) appeared to be widely accepted as a fairly reliable basis for exploring numerous stability conditions in saturated clays. Today, however, it is believed that an ever-increasing number of the profession is by experience forced to believe that the validity of this procedure is strongly limited both with respect to type of soil, to stress condition, and with respect to time.

As I see it, collected evidence seems to have indicated that the application of the undrained shear strength (in the meaning mentioned) is limited to normally consolidated clays and, more important, it appears to be restricted exclusively to those stability conditions which appear immediately after load changes have taken place, that is, before any drainage has occurred.

Even though it may seem logical to correlate undrained test conditions with undrained field conditions, it may safely be considered that whenever we are using shear strength data, obtained without knowledge of pore pressure either in the tests or in the ground, the best we can hope for is very approximate answers, and, moreover, practical evidence is widely recognized as being the only basis on which we can ascertain that even rough approximations can be expected.

As a consequence the shear strength of clays determined on the basis of effective stresses appears to have found increased recognition as being a far more reliable basis for stability analysis both in a general way and for the long-term or stationary conditions in particular.

Even though the above-mentioned findings may appear to be fairly well-founded on practical evidence, any information during this discussion which may be of further help in clarifying the range of validity of existing shear strength hypotheses of clays must be greatly appreciated.

In our discussion it is anticipated that we cannot escape dealing with the everlasting question of how to adapt the laboratory test conditions to simulate given *in situ* conditions in the best possible manner. Of the very many things which could have been brought up in this connection, just one point will be mentioned here because it appears to me to have received little attention up to now: here it is.

Most of our laboratory shear strength tests are carried out as three-dimensional, and the results thus obtained are frequently used in a two-dimensional analysis. We may therefore ask ourselves what do we know today about the differences in observed shear strength of soils when tested under two-dimensional *versus* three-dimensional stress conditions, all other pertinent data being equal? And, if our knowledge about these possible differences is inadequate, is the question not of sufficient importance to justify future research?

Regarding the available interpretation procedures it is believed to be of particular importance to consider once more: what is actually shear failure in a test specimen, or, more precisely, will just one single failure criteria suffice under all circumstances, or is it more advisable to adapt the failure criterion to the purpose of the investigation, and in that case how should it be done?

With particular reference to some of the papers in this subdivision it is considered to be of common interest to discuss the effect of the intermediate principal stress on the measured angle of internal friction in sand: and, if time allows, some consideration could also be given to the deformation characteristics of sand.

At this point I would like to mention that in my written report reference is made to Kirkpatrick's very interesting tests (1b/9) as being undrained, whereas, as you know, they were drained. It is sincerely regretted that my notification, last autumn, for the purpose of having the misprint corrected was too late.

In summarizing I would like to add that I am aware of the circumstance that the questions just indicated are well known, and most of them have been thoroughly considered many times previously. Nevertheless, it is believed that no general agreement has been reached yet. As a consequence, it is hoped that a re-consideration of some of our common problems encountered in the investigation of shear strength and deformation characteristics of soils is justified, whether the consideration is based on theory, experiments or practice, as it is generally understood that progress is highly dependent on a successful combination, not to say correlation, of all sources of information.

A. W. Bishop (U.K.)

I wish to refer to a point raised by the Assistant Reporter towards the end of his introduction. Shear strength is generally measured in the laboratory in the triaxial apparatus, using a test in which the intermediate principal stress is equal to the minor principal stress. Field conditions seldom correspond to this state of stress and in fact many of our stability problems correspond more closely to plane strain. How much influence on shear strength has the different value of the intermediate principal stress operating under these conditions?

There are two ways of approaching this question. The first is by field evidence. We can carry out triaxial tests on the soil, preferably working in terms of effective stress. We can examine field cases in which the pore pressure can be measured, and if we are fortunate enough to obtain results from an actual slip we can carry out a stability analysis knowing that the factor of safety is 1·0. The strength values required by the stability analysis can be compared with the results of the laboratory tests.

A certain amount of evidence has already been collected on this basis. One of the recent cases of note has been described in *Géotechnique* by Sevaldson who in 1956 compared a very carefully analysed slip with laboratory test results.

There is, however, a second line of approach which is perhaps more difficult but removes the influence of secondary factors, and that is to compare tests carried out in the laboratory under conditions of plane strain with conventional triaxial tests. The practical difficulties of a plane strain test are rather formidable. The shear box is not suitable for this purpose because of the unknown stress distribution within it, and the conventional shear-box test, as I pointed out in a letter to *Géotechnique* in 1954, is often open to objection on the ground that the result is ambiguous. It is not clear whether the horizontal plane in the shear box is a plane of rupture or a plane of maximum shear stress. The alternative approach is to carry out a compression test on an element of soil in which restraint is applied in the direction of the intermediate principal stress to prevent lateral yield. A plane strain test can thus be carried out but the difficulty of end-effects has to be faced.

Recently, Clive Wood, who has been working with me on this problem at Imperial College, has begun to get results from a new apparatus which we have devised. In this the compression specimen has the usual ratio of height to thickness—it is 4 in. in height and 2 in. in thickness—but is very wide (16 in. is the upper limit of the apparatus) to minimize the influence of local shear forces on the surfaces where lateral yield is prevented. The preliminary results indicate that in plane strain the angle of shearing resistance is definitely higher than in the conventional triaxial test.

In the first series of tests a moraine having a clay fraction of about 3 per cent was used. This was compacted at a water content about 3 per cent above the optimum to avoid any ambiguity in the measurement of pore pressure, the tests being carried out under undrained conditions. If the results are expressed in terms of effective stress, the values of ϕ', the angle of shearing resistance, for the conventional triaxial test and for the plane strain test are 37 and 41 degrees respectively at the point where the principal stress ratio has its maximum value, and 35 and 37 degrees respectively at the maximum deviator stress. Taking into account small differences in the cohesion intercept in the various cases, this leads to a shear strength typically about 10 per cent higher in the plane strain test than in the triaxial test for a given *effective* normal stress.

This difference is not only of academic interest, it is of practical importance, too; and will mean that there is a concealed factor of safety in our standard effective stress procedure if these results are confirmed by the more extended series of tests which we hope to perform.

A second point of considerable interest is that failure in plane strain occurs at much smaller compressive strains than in the usual triaxial test. This may help to throw some light on problems occurring both in stability and in active earth pressure on retaining walls where the deformations are hard to explain in terms of the ordinary triaxial compression test.

These results, I must emphasize, are still rather preliminary. There is also some evidence, however, from tests carried out on sand by the Norwegian Geotechnical Institute, using the vacuum technique for applying lateral stress, which supports the conclusion that in plane strain a rather larger friction angle is encountered.

D. J. HENKEL (U.K.)

In considering the range of validity of shear strength parameters it is necessary to think in terms of a three-dimensional failure surface. Failure conditions in plane strain and axial symmetry are only particular cases of the general problem. In the conventional triaxial cell only the case of axial symmetry can be investigated and a large number of tests on clay (LL = 43, PL = 18) have been carried out at the Imperial College to obtain as much information as possible about shear parameters under conditions of axial symmetry.

Undrained tests in both extension and compression have been performed and confirm that a unique result in terms of effective stress is obtained in each case irrespective of the way the axial and radial total stresses change during the test. Drained compression tests were carried out with: (a) the axial stress increasing; (b) radial stress decreasing; and (c) the axial and radial stresses adjusted so that the average principal effective stress remained constant. A similar set of drained extension tests were performed with: (a) the axial stress decreasing; (b) the radial stress increasing; and (c) the average principal effective stress kept constant.

The results for the angle of shearing resistance obtained from samples normally consolidated under an all-round stress have been tabulated.

Type of test	ϕ'
Compression	
Undrained	23°
Drained-axial stress increased	22°
Drained-radial stress decreased	22°
Drained-average principal stress constant	22°
Extension	
Undrained	22½°
Drained-axial stress decreased	22½°
Drained-radial stress increased	21½°
Drained-average principal stress constant	22°

It can be seen that the differences between the results from the various types of test are small and certainly from an engineering point of view there is a unique value for the angle of shearing resistance in terms of effective stresses.

It is of interest to note that although the angles of shearing resistance in compression and extension are very similar, the ratio of the undrained shear strength to the consolidation pressure (c/p) is different in the two tests. On the average it was found that the (c/p) values in extension were 15 per cent lower than those in compression and this can be explained by the larger pressures which are set up in the extension tests.

These test results have also been examined in terms of Hvorslev's parameter (C_e/p) and $\tan \phi_e$ and the results from all the various types of test show very small variations.

R. PARRY (U.K.)

D. J. HENKEL (1a/25) has described various stress paths that may be applied to a sample in the triaxial cell. As part of the programme that Henkel is pursuing at the Imperial College, I recently carried out a programme of tests on remoulded Weald clay and London clay. It is apparent from these tests that the total volume change is composed of at least two different types of volume change. We have assumed that there is an elastic component and a non-elastic, or plastic, component.

The further assumptions have been made that the elastic properties are isotropic and that the elastic modulus increases with increasing stress and decreases with increasing voids ratio.

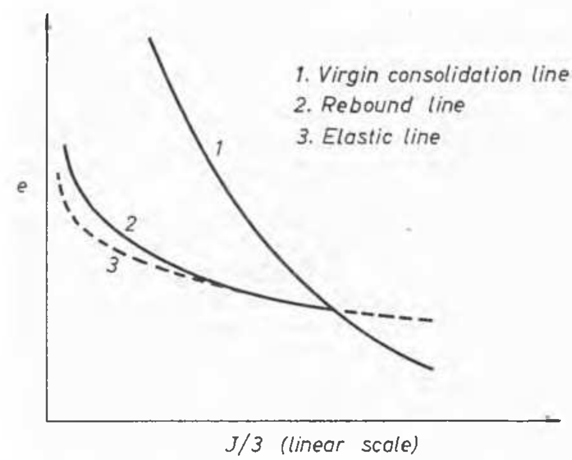


Fig. 1

The simplest equation relating these variables in this way is:

$$E = k \cdot \frac{J}{3e} \qquad \ldots\ldots (1)$$

where $J/3$ is the average principal effective stress, e is the voids ratio, and k is a constant for any clay.

Using this expression, the elastic volume change is:

$$\left(\frac{\Delta v}{v}\right)_E = \frac{1-2\mu}{k}(\Delta\sigma'_A + 2\Delta\sigma'_R) \times \frac{3e}{J} \qquad \ldots\ldots (2)$$

for a sample tested in the triaxial cell, where μ = Poisson's ratio, and $\Delta\sigma'_A$, $\Delta\sigma'_R$ = the axial and radial effective stresses.

Values of $(1-2\mu)/k$ were obtained by assuming that the early portion of the rebound curve on the consolidation plot is sensibly elastic.

The values obtained in this way were:

for Weald clay $\frac{1-2\mu}{k} = 0{\cdot}0075$

London clay $\frac{1-2\mu}{k} = 0{\cdot}0100$

The value of E obtained from these figures varies a great deal with the assumed value of μ. Unfortunately, there is little evidence at the moment to indicate the true value of μ. I feel, myself, that it may be somewhere between 0·1 and 0·2.

Taking a value of $\mu = 0{\cdot}2$ we have: $E = 80J/3e$ for Weald clay.

Now the voids ratio shows comparatively little variation compared with J, so we can assume an average $e = 0{\cdot}53$.

Then $E = 150J/3$ lb./sq. in., and the elastic shear modulus $G = [E/\{2(1+\mu)\}] = 60J/3$ lb./sq. in.

For a normally consolidated sample of Weald clay with an unconfined strength of roughly 400 lb./sq. ft., the value of E is about 750 lb./sq. in., or 50 kg/cm^2. The corresponding value of G is 300 lb./sq. in. or 20 kg/cm^2. For London clay the values are approximately half those for Weald clay. These values of G are of the same order as those obtained by Denisov and Reltov using the technique of damped torsional oscillations. I would be interested to know a little more about the properties of the clays they used, such as the PL, LL, and consistency.

When we subtract the elastic volume changes from the total volume changes for the various tests, a consistent pattern of plastic volume change emerges. A good correlation is obtained between the plastic volume change and the stress history at failure.

A useful laboratory test in a study of this nature is the J constant test in which the stress path followed is such that elastic volume change is zero. It can be seen from equation 2 that this occurs if

$$\Delta\sigma'_A = -2\Delta\sigma'_R \qquad \ldots. \ (3)$$

It was found that during unloading and reloading a standard drained test gave a ratio of elastic to plastic volume change much greater than during the first loading, while a J constant test showed only a small volume change.

M. J. Hvorslev (U.S.A.)

The paper by A. Balla (1b/2) deals with the influence of end restraint on the stress conditions in the triaxial compression test, which is a problem of great importance for further basic research on the physical properties of soils. The simpler problem of the influence of end restraint during uni-axial or unconfined compression of cylinders has been under consideration since the latter part of the nineteenth century, and Filon published his classical theoretical solution of this problem in 1902. He made the assumption that the end restraint is produced by a ring, the height of which converges to zero; that is, radial movements of points on the end surfaces are prevented only at the cylindrical surface or boundary.

In 1944 Pickett presented a solution of the actual uni-axial problem in which the end restraint is produced by friction, and he assumed that this friction is great enough to prevent radial movements of any point of the end surfaces. Pickett used the Fourier method and encountered the difficulty that the series converges slowly near the ends of the cylinder, and definite values were not obtained for stresses at the cylindrical boundary of the end surfaces. In 1951 D'Appolonia and Newmark applied the lattice analogy method to the problem and obtained numerical values of stresses for all points on the end surfaces. The distributions of axial normal stresses obtained by the three methods agree in form but differ to some extent in numerical values as shown in the table above. However, Filon's distribution of radial, tangential, and shearing stresses near the end surfaces differs materially in both numerical values and form from those obtained by Pickett and D'Appolonia–Newmark.

Ratio of computed to average axial normal stresses in restrained cylinder

Distance from axis of cylinder	$r=0$	$r=\frac{R}{2}$	$r=R$
Mid-plane			
Filon	1·134	1·062	0·894
Pickett	1·073	1·040	0·935
D'Appolonia–Newmark	1·175	1·090	0·853
End planes			
Filon	0·686	0·825	1·686
Pickett	0·887	0·877	—
D'Appolonia–Newmark	0·851	0·825	1·700

The above-mentioned three solutions of the problem consider only axial compression of a cylinder with a length–diameter ratio of $\pi/3$ or 1·0, whereas Balla presents solutions for a cylinder with any length–diameter ratio and subjected to both axial and radial external pressures. He also considers various degrees of end restraint by introduction of a roughness factor, defined as the ratio of total shearing forces to total axial forces on a sector of the end surfaces. There are apparent inconsistencies in some of the final stress formulae and in the numerical values of stresses shown in the table, but the paper is so condensed that it is difficult to determine the cause of these inconsistencies by casual reading. They may possibly be the result of changes in nomenclature, unspecified assumptions, or the use of a roughness factor which is incompatible with the loading and stress conditions. The actual ratio of shearing forces to axial forces on the end surfaces depends not only on the restraining friction but also on the ratio of axial to radial external pressures, and it must be zero when the external loading does not produce radial deformations of the cylinder. It is hoped that this interesting and potentially very valuable paper will be made available in a more complete form.

Fig. 2 Waterways Experiment Station large triaxial apparatus
Grand appareil pour mesures triaxiales (Waterways Experiment Station)

Soils are not ideal elastic materials, and theoretical determinations of stresses, strains and volume changes in various parts of a triaxial test specimen should be supplemented by experimental investigations. In 1953 the Waterways Experiment Station of the Corps of Engineers, U.S. Army, built a large vacuum-type triaxial apparatus for test specimens with a diameter of 35·7 in. and a height of 70 in. The apparatus is shown in Fig. 2 and primarily is intended for investigation of the action of WES soil pressure cells, as described in a paper by AHLVIN (1956). These cells have a diameter of 6 in. and a thickness of 1 in. The axial load is transmitted through rigid end plates and the stress distribution in the test specimen is not uniform, but it was hoped that both the stress distribution and the registration ratios of the pressure cells could be determined by conducting tests with different principal stress ratios and with pressure cells of 50 lb./sq. in. and 100 lb./sq. in. capacities and corresponding differences in moduli of deformation. However, it was found that both the stress distribution and the pressure cell registration ratios are influenced by so many factors that additional tests may have to be performed before the test results so far obtained can be fully evaluated.

Experimentally determined distributions of axial stresses at the mid-height and ends of a test specimen of medium dense sand are shown in Figs. 3 and 4 and there compared with the theoretical stress distribution obtained by D'Appolonia and Newmark. The experimental and theoretical stress distributions are alike in general form, but the experimental data have not been corrected for over- or under-registration of the pressure cells, and there is also considerable scattering in the results of individual tests. The registration ratios of the pressure cells depend upon the deformation of the cells and the corresponding axial strains in the sand, and they vary with the stress conditions. The registration of cells close to the cylindrical surface is also influenced by the nearness of this surface and by a stress gradient over the face of the cells. Extrapolation of the experimental stress distribution curves to the cylindrical surface is not reliable, and the overall registration ratio of the cells cannot be determined with sufficient accuracy by summation of the indicated stresses over the entire cross-sectional area.

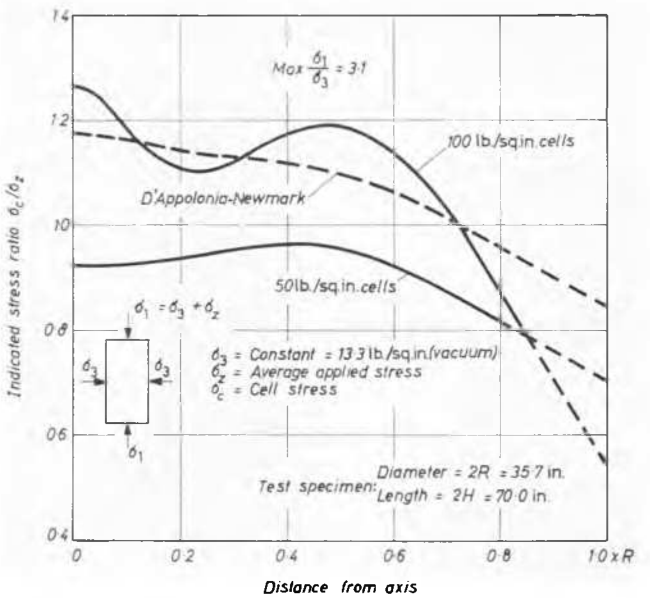


Fig. 3 Stress distribution at mid-height in a triaxial test specimen of medium dense sand

Distribution des contraintes à mi-hauteur d'un échantillon de sable (densité moyenne) pour essais triaxiaux

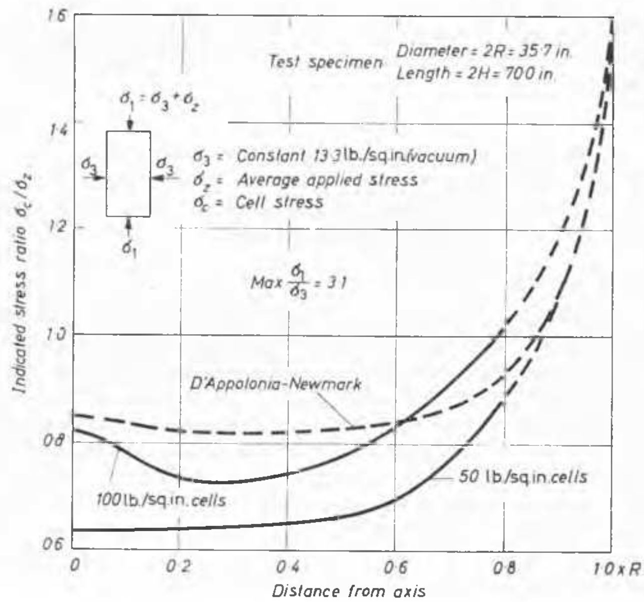


Fig. 4

The influence of end restraint on variations in volume changes over the length of triaxial test specimens of sand have also been investigated by the Waterways Experiment Station. A short description of these tests was given by SHOCKLEY in a discussion prepared for the 1953 Conference in Switzerland. It was found that the volume of the central part of the test specimen increases, whereas that of the end sections decreases towards the end of the test. This pattern of volume changes prevails for both medium dense and loose sands. It should be noted that the volume changes were determined for rather large strains of 7·5 and 10·0 per cent and that the volume of the entire test specimen had been increased at these strains.

Supplementing the above-mentioned tests, TAYLOR (1951) investigated the migration of pore water and corresponding local volume changes during undrained triaxial tests on clay. He found that water migrated from the central part to the end sections of a test specimen, or that a volume decrease occurs in the central part and a volume increase near the ends, which is the opposite of that observed for sand. Taylor suggested that this difference in behaviour of sand and clay may be due to dilation of sand and consolidation of clay at high shearing strains and failure.

It is possible that the non-uniform distribution of stresses and strains caused by the end restraint may not seriously affect the reliability of strength determinations by means of triaxial test specimens with a length-diameter ratio of at least 2·0. However, the simultaneous occurrence of a volume increase in one part and volume decreases in other parts of a triaxial test specimen greatly decreases the value and obscures the significance of measured volume changes of the entire test specimen. The pattern of volume changes in both axial and radial directions for triaxial test specimens subject to end restraint must be investigated in greater detail before reliable data can be obtained on the critical void ratio of sands and on the consolidation characteristics of soils subjected to arbitrary triaxial stress changes and failure.

References

AHLVIN, R. G. (1956). Waterways experiment station large triaxial apparatus. *Highway Research Board, Washington, D.C., Bulletin 141*, p. 19

D'APPOLONIA, E. and NEWMARK, N. M. (1951). A method for salution of the restrained cylinder under axial compression. *Proceedings First U.S. National Conference of Applied Mechanics*, pp. 217/226, *Amer. Soc. Mech. Eng.*

FILON, L. N. G. (1902). The elastic equilibrium of circular cylinders under certain practical systems of load. *Phil. Trans., A*, **198**, 147

PICKETT, G. (1944). Application of the Fourier method to the solution of certain boundary problems in the theory of elasticity. *J. Appl. Mech., Amer. Soc. Mech. Eng.*, **11**, A-176

SHOCKLEY, W. G. (1953). Discussion of paper by A. W. Bishop and G. Eldin. *Proc. 3rd International Conference on Soil Mechanics and Foundation Engineering*, Vol. 3, p. 122

TAYLOR, D. W. (1951). *Research on Shearing Resistance of Clay. Sections on water migration studies*, pp. 9, 12, 36/42. Report to the U.S. Army Engineer Waterways Experiment Station, CE, Vicksburg, Miss.

Corps of Engineers, U.S. Army Engineer Waterways Experiment Station (1950). Minutes of conference on soil aspects of potamology program. Contains data on distribution of volume changes in triaxial test specimens of sand. *Potamology Investigations, Report No. 11/7*, Corps of Engineers, Vicksburg, Miss.

D. H. TROLLOPE (Australia)

I should like to comment on some of the implications of the interesting hypothesis presented by P. W. ROWE (1b/12), as I feel much is to be gained from examining equilibrium conditions in contrast to the more usual concentration on conditions at failure. We have observed, in undrained controlled rate of stress tests, the behaviour which the author predicts. If we imagine a typical soil to consist of coarse grains distributed throughout a colloidal matrix, their resistance to deformation through friction between the larger gravitational grains can only be developed after a state of plastic yield has developed in the matrix. This is in line, I believe, with the views expressed by T-K. Tan and E. C. W. A. Geuze.

With a condition of matrix yield, it can be visualized that only enough gravitational grains are brought into contact to provide the resistance necessary to ensure equilibrium. When the maximum possible number of grains have moved into contact in the failure zone, then shear failure will develop on the application of further stress.

The concept I wish to suggest is that the mechanism of shear failure involves the build-up of what is popularly known as a granular structure in the zone of failure. Thus it appears that, provided a condition of matrix yield is developed creep will occur under drained conditions when $\phi_{aq} < \phi_e$.

It also follows that, particularly for very sensitive soils, the colloidal structure may carry the imposed stresses right up to failure and here the nature of the pore water as an electrolyte as well as the activity of the clay will influence the behaviour.

Finally, I would like to draw attention to the fact that the statements $C_e = 0$ and $C' = 0$ are not synonymous. It can readily be shown that ϕ_d—the angle of shearing resistance measured in a drained test—includes a factor which can be related to C_e in the Hvorslev strength equation. It is perhaps well to bear this in mind in view of the suggestions for discussion later in the conference concerning the $C' = 0$ hypothesis applied to long-term stability problems.

H. U. SMOLTCZYK (Germany)

Concerning the question of the factors which influence the deformation properties of cohesionless soil, I read with great interest the investigations of Paper 1b/8. However, I am wondering about the possibility of determining a modulus of elasticity and Poisson's ratio of a sand. Are we really allowed to take both terms from the mechanics of rigid bodies to soil mechanics, in spite of the fact that they have been defined by experiments which cannot be made with cohesionless soil?

On the other side we can measure two elastic qualities the shear modulus and the bulk modulus—due to their mechanical definition—by separating the plastic and the elastic behaviour from each other. This can be done by taking a shear box or a compression box with a sand specimen and putting a slowly alternating stress on it. As a function of the number of load cycles, the increase of the plastic component of deformation becomes smaller and smaller; finally, one gets the pure elastic deformation left. Doing this, and plotting these elastic deformations in the usual manner as functions of the causing stress, we have the possibility of measuring the actual bulk and shear moduli of sand.

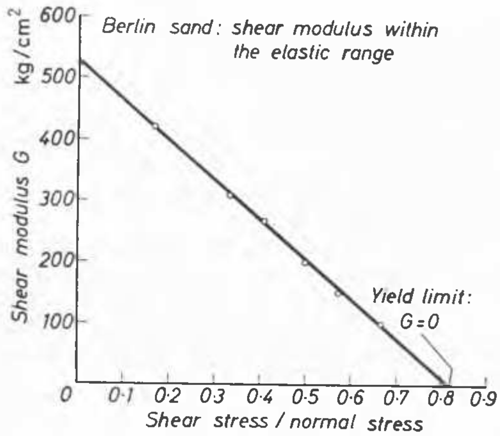


Fig. 5

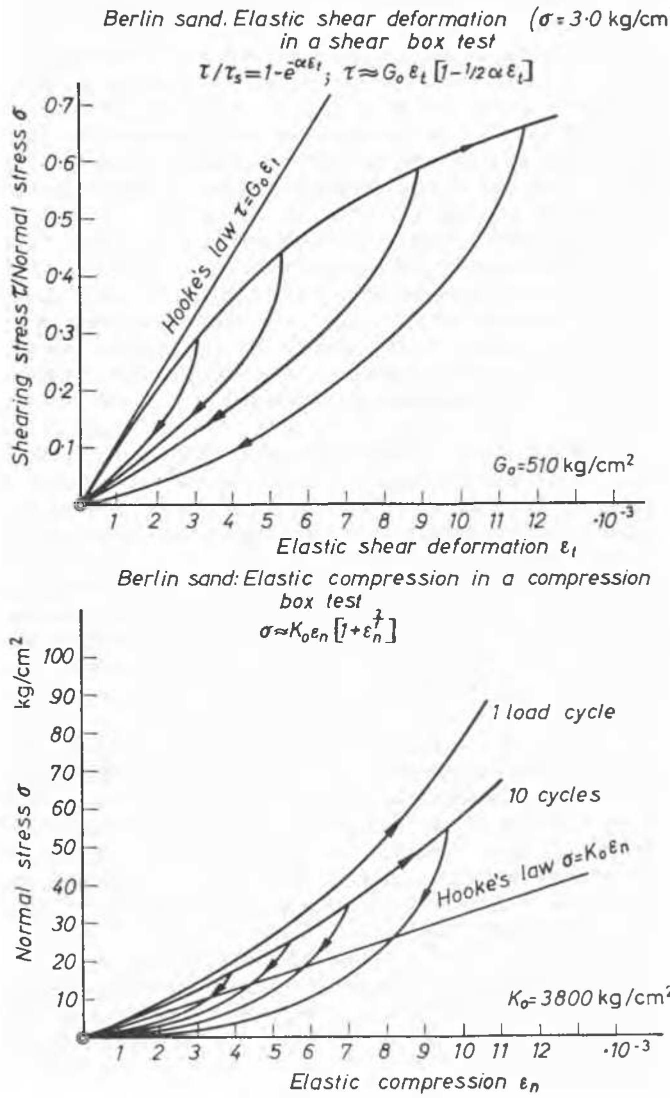


Fig. 6

Both start with a distinct initial value, the bulk modulus constantly increasing, the shear modulus decreasing to zero, Fig. 5. For instance, with Berlin sand, I measured a bulk modulus of about 4000 kg/cm^2 and an initial shear modulus of about 500 kg/cm^2. The latter was related to the full height of the specimen: in fact, it would be more reasonable to relate it to the height of the shear zone, the shear modulus becoming rather smaller. Anyway, the Poisson's ratio resulting from this (Fig. 6) has a value of about 0·4 at zero and is converging to a value of 0·5, this value being the upper limit of it. It is difficult to realize a ratio of about 0·6, for instance, as can be seen in Fig. 2 of 1b/8. I would therefore like to state that the Poisson's ratio and the modulus of elasticity can only be got indirectly by testing the bulk and the shear modulus and by a pure elastic stress–strain test.

Another important factor in the shear-box experiments is the time. The decrease of the plastic component of deformation as a function of the number of slow load cycles was found only up to about half of the statical limit load. That could mean that the dynamical angle of internal friction might occasionally be only half the statical one.

P. Habib (France)

Si l'étude de l'influence de la contrainte principale intermédiaire sur la résistance au cisaillement du sable est extrêmement importante pour l'analyse des relations efforts-déformations des corps solides en général, il n'en demeure pas moins que, du point de vue de l'ingénieur, son intérêt est beaucoup plus limité. Il y a en effet de nombreuses hypothèses beaucoup plus critiquables comme par exemple l'assimilation du sol à un corps plastique, même lorsque la courbe de cisaillement présente un maximum.

L'étude de l'influence de la contrainte principale intermédiaire est délicate pour plusieurs raisons.

Elle repose en premier lieu la question du critère de rupture; celui-ci est toujours issu d'une courbe effort-déformation; pour estimer la rupture, il est nécessaire de comparer des courbes parlant des mêmes grandeurs: il est difficile par exemple de comparer des déformations de torsion et des déformations linéaires.

En second lieu, l'expérimentation est difficile. On sait que les essais triaxiaux classiques ont mis longtemps avant d'être au point et cependant, à cette conférence même, on s'interroge encore sur la répartition des contraintes dans l'éprouvette en cours d'essai.

Enfin, il y a peu de mode opératoire; pour les valeurs extrêmes de la contrainte intermédiaire: essais triaxiaux ordinaires avec raccourcissement ou allongement; pour les valeurs intermédiaires: torsions complexes, tube, cisaillement direct, toutes méthodes déjà utilisées pour des corps solides, fragiles ou métalliques, par Rös en Suisse.

La méthode de W. M. Kirkpatrick (1b/9), celle du tube, est certes très intéressante, mais s'il s'affranchit du cisaillement parasite apporté par la membrane, je crains que la répartition des contraintes pendant la rupture, sur la surface de glissement qui, ici, a pour directrice une génératrice du cylindre, soit variable, et que, par le jeu de la dilatation cubique du sable, la contrainte moyenne dans le plan de rupture, ne soit pas celle que l'on croyait y avoir mise.

J'ai été très étonné par les résultats obtenus au triaxial (avec compression et avec extension) qui sont très différents de ceux qu'on peut trouver dans la littérature pour le sable et pour des matériaux à fort frottement interne comme les mortiers.

Peut-être les résultats de Kirkpatrick sont-ils dus à la forme des grains?

Enfin, pour répondre à la proposition de notre rapporteur général, je voudrais signaler que dans les essais triaxiaux classiques, l'influence de la membrane de caoutchouc est indécelable si son épaisseur est inférieure à 1/10 mm.

T. J. Osterman (Sweden)

I would like to make some remarks in connection with the second subject that the Assistant Reporter has proposed for discussion, 'The effect of the intermediate principal stress on the measured angle of internal friction in sand'.

A. W. Bishop and H. U. Smoltczyk have just made some interesting statements on the sand deformation problem, which we have also been investigating at the Swedish Geotechnical Institute. Here I will refer only to the phenomenon in principle, not considering for instance such important factors as shape and roundness of the sand grains.

In discussing the angle of internal friction it is better to treat the process of failure in terms of the energy required rather than the critical stress.

In a granular mass the work done in deformation is rather more complicated than is the case with a solid body. If the forces are applied slowly, however, kinetic energy can be neglected and the work done will be absorbed as deformation energy (from normal and shear stresses) in the grains and in friction between the grains. Moreover, grain travel, in most cases producing dilatancy, will occur as can be observed at the boundaries of a mass element, Fig. 8.

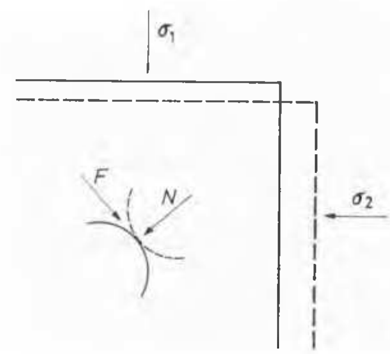


Fig. 7 Forces at the point of contact between two grains. Dotted lines indicate changes in position of boundaries

Forces agissant au point de contact entre deux grains. Les lignes hachurées représentant des changements dans la position des limites

The work equation can be written

$$W = W_g + W_f + W_d + \dots \qquad \dots . (1)$$

where W = external work (not including dilatancy); W_g = the summation of work absorbed in grain deformation; W_f = the summation of work absorbed in friction between grains; and W_d = the summation of work absorbed in dilatancy.

The work absorbed, in for instance edge rupture, is taken to be included in the above three terms.

In the case of a dense sand, grain travel will be much hindered by interlocking, and on the whole the dilatancy will be positive. When the applied stresses become high enough, sufficient energy can be mobilized to overcome the resistance due to both friction and dilatancy, as occurs at the peak point of the stress-deformation curve. The stress then falls off gradually to the ultimate value. In a loose sand no peak point exists, and here the question of dislocations is more important.

Fig. 8 shows the relevant relationships.

Consider the section of an element of a granular mass shown in Fig. 9 and suppose that the shear stress τ produced by the principal stresses σ_1 and σ_2 is large enough to cause failure on a slip surface (or more correctly, in a slip layer of thickness h) such as AB.

Let the third principal stress be σ_3 and the average angle of shear $d\gamma$. Then the unit dilatancy will change $d\delta_n$ in a direction normal to the slip surface, $d\delta_t$ parallel to the slip surface in the plane of the section and $d\delta_3$ parallel to the slip surface but normal to the plane of the section, the normal stresses being σ_n, σ_t and σ_3.

At the peak point $W = W_f + W_d$ and we have

$$\tau = \sigma_n \tan\phi + \sigma_n \cdot \frac{d\delta_n}{d\gamma} + \sigma_t \cdot \frac{d\delta_t}{d\gamma} + \sigma_3 \cdot \frac{d\delta_3}{d\gamma} \quad \ldots. \ (2)$$

where ϕ = angle of friction.

The dilatancy can be prevented from occurring in directions

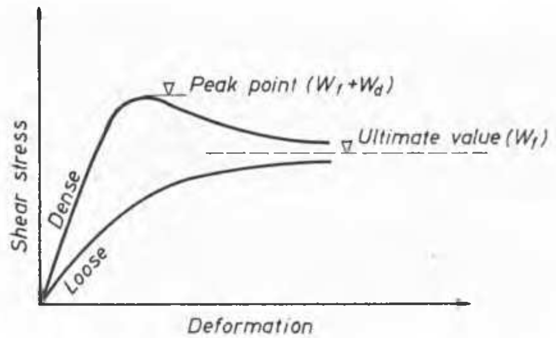


Fig. 8 Relationship between stress and deformation in dense and loose sand

Rapport entre la sollicitation et la deformation dans des sables de haute et basse densité

other than normal to the slip layer, as for instance in a direct shear machine where the change of total normal dilatancy can be measured.

At failure, the meaning of $d\gamma$ is vague and it is better to put $ds = h d\gamma$ when we can write:

$$\tau \fallingdotseq \sigma_n \tan\phi + \sigma_n \cdot h \cdot \frac{d\delta_n}{ds} \quad \ldots. \ (3)$$

which becomes

$$\tan\psi \fallingdotseq \tan\phi + h \cdot \frac{d\delta_n}{ds} \quad \ldots. \ (3a)$$

where ψ = apparent angle of friction.

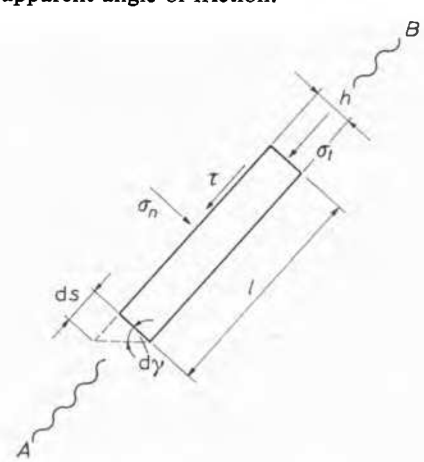


Fig. 9 An element of a slip layer in sand

Element d'une couche de glissement dans du sable

The specific case of equation 3 has also been treated by Bishop and others, and equations 3 and 3a are in principle the same as theirs.

From the rather more general equation 2 it is seen that the intermediate stress has an influence on the apparent angle of friction and that this angle must be related to the density of the sand. The orientation of grains at the boundaries can also have an effect.

B. Jakobson (Sweden)

My first impression was that H. U. Smoltczyk did not believe in my results because I have values of Poisson's ratio greater than 0·5. However, in the apparatus used by me, all stresses are perfectly known, and this is necessary in order to be able to compute the value of Poisson's ratio, so I think there will be no doubt about the results. Besides, it is quite easy to explain values of Poisson's ratio greater than 0·5, but I will not discuss the matter further at present.

I now understand that H. U. Smoltczyk means that there is no reason to use the conception of Poisson's ratio due to its great variation. Bearing in mind the still greater variations in E and G I quite agree with him that our old Hooke's law is not a good one to use with soils, but as there is not a more suitable one to replace it, we unfortunately have no choice.

P. W. Rowe is certainly correct when he supposes different coefficients of friction between the grains in my two kinds of sand, denoted A and B. As I have stated in my paper, sand A contains some polished grains but sand B does not. I think this is the greatest difference between the two kinds of sand. I have brought samples of the two sands which I will place on the Chairman's table for inspection.

T. K. Chaplin (U.K.)

The Assistant Reporter has suggested in his proposals for discussion that we should discuss the effect of the intermediate principal stress on what he calls the 'angle of internal friction of sand'. I hope he will not mind if I assume that he really means the 'angle of shearing resistance', the term often used in this country to avoid any ambiguity when describing test results which have not been corrected for the dilatancy component of the compression strength or the shear strength, as the case may be.

The influence of dilatancy can be very great in dense sands, that is, in sands at a high relative porosity, the angle of shearing resistance being then considerably higher than the true—I emphasize true—angle of internal friction. Two soils with the same true angle of internal friction, for a given relative porosity and initial effective lateral pressure σ_3', can have markedly different angles of drained shearing resistance due solely to the different amounts of dilatancy. That, in turn, depends on the shape—that is, roundness and sphericity—of the grains. A compact or dense sand or silt, such as used by Bishop and Eldin (1950), Nash (1953) and Penman (1953), needs a very large effective lateral pressure σ_3' to stop it dilating in a constant volume triaxial test. Conversely, it will dilate readily at the same porosity but at a lower value of σ_3' in a fully drained test. The deviator stress is made up of two components, one due to internal friction and the other to the product of the effective lateral pressure σ_3' and the rate of volume change with respect to lateral strain. We cannot then ignore this large cause of variation which I believe may be strongly affected by the intermediate principal stress as the combination of several different ways.

Bishop (1950) has shown that in the familiar shear box test the rate of volume change has to be multiplied by the vertical pressure, that is, by the normal pressure on the shear plane, to obtain the dilatancy correction. He has also pointed out that in the standard triaxial compression test the rate of volume change has to be multiplied by the minor principal stress, and not as one might suppose, at first sight, by the average principal stress. The intermediate principal stress is obviously equal in the triaxial compression test to the minor principal stress. A simple calculation can be made to show the contribution of changes in the intermediate principal stress under any given conditions of test on the dilatancy correction.

Of course, the intermediate principal stress must affect the development of the deviator stress ratio, that is, $(\sigma_1-\sigma_3)/\sigma_3'$.

To sum up, the influence of the intermediate principal stress can directly affect: first, the true angle of internal friction; secondly, the rate of dilation in a drained test; thirdly, the force the sample develops to prevent itself dilating at any stage in the undrained test; fourthly, the factor by which the rate of volume change must be multiplied to obtain the dilatancy correction; and finally, the rise and fall of the deviator stress ratio with increase of strain.

References

BISHOP, A. W. (1950). Discussion on shear testing. *Géotechnique, Lond.*, **2**, 113

— and ELDIN, G. (1950). *Géotechnique, Lond.*, **2** (1), 13

NASH, J. K. T. L. (1953). *3rd International Conference on Soil Mechanics and Foundation Engineering*, Vol. 1, p. 160

PENMAN, A. D. M. (1953). *Géotechnique, Lond.*, **3**, 312

G. G. MEYERHOF (Canada)

In view of the wide use of standard penetration tests for estimating the relative density of sands *in situ*, the research of H. J. GIBBS and W. G. HOLTZ (1a/9) is timely. Their results show that penetration resistance increases roughly with the square of relative density and in direct proportion to the effective overburden pressure of the sand at spoon level; moreover, the resistance is not much affected by length or weight of the rods. These experimental results can, approximately, be represented by the relationship that the standard penetration resistance (blows/ft.)

$$N = 1{\cdot}7D_r^2(p+10)$$

where D_r = relative density; and p = effective overburden pressure (lb./sq. in.).

For interpreting the relative density, values which are similar to those used by Gibbs and Holtz and are somewhat more uniform in division have been suggested (MEYERHOF, 1956), namely:

State of packing	*Very loose*	*Loose*	*Compact (or medium)*	*Dense*	*Very dense*
Relative density	0 to 0·2	0·2 to 0·4	0·4 to 0·6	0·6 to 0·8	0·8 to 1·0

The important influence of the effective overburden pressure on the penetration resistance is also shown by the present tests with saturated sands in which the resistance dropped even more than corresponding to the effective intergranular pressure. While some of this reduction, especially for the fine sand, was probably due to a quick condition near the spoon, the authors' tests support the previous conclusion by me (MEYERHOF, 1956) and further field evidence (E. SCHULTZE and H. KNAUSENBERGER, paper 2/9) that the influence of the water table is already included in the observed penetration resistance and need not therefore be allowed for separately when applying the results to foundation problems on sands.

Reference

MEYERHOF, G. G. (1956). Penetration tests and bearing capacity of cohesionless soils. *J. Soil Mech. Found. Div., Proc. Paper 866, Amer. Soc. Civ. Engrs.*, **82**, SM 1

T-K. TAN (China)

I wish first to congratulate L. ŠUKLJE on his excellent paper (1b/14) on the consolidation process in which he takes account both of the consolidation and of the well-known Buisman equation $z=h_q(\alpha_p+\alpha_s \log t)$, in which z = settlement, h = height of clay layer, q = loading intensity, α_p = coefficient of primary consolidation and α_s = coefficient of secular time effect.

The computation of the water pressures and settlement of clay layers due to consolidation and secondary time effects, which is aimed at by Šuklje in a semi-empirical way, can also be carried out on the basis of my mathematical theory. This theory, which was communicated to the last conference, starts from the assumption that clay under shear may be regarded as a Maxwell solid. In this discussion I will not deal with mathematical problems, but will show that the phenomenon of consolidation and secondary time effect may be illustrated by simple models.

Fig. 10 shows the well-known Terzaghi model and the model which I have derived for Taylor's theory—in this model the pressure is taken up by the spring and the dashpot. It can be seen directly that the ultimate settlement in this model is

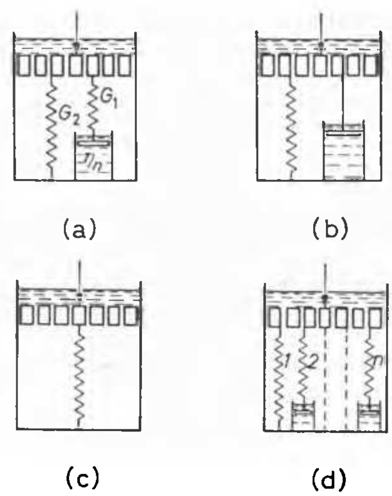


Fig. 10 Rheological models for one-dimensional theories of consolidation. (a) Model for the author's theory; (b) model for Taylor's and Goldstein's theory; (c) model for Terzaghi's and Biot's theories; (d) generalized model

Maquettes pour l'étude rhéologique des théories de consolidation unidimensionelle. (a) Maquette représentant la théorie advancée pour l'auteur; (b) Maquette représentant la théorie de Taylor et Goldstein; (c) Maquette représentant la théorie de Terzaghi et celle de Biot; (d) Maquette généralisée

determined by the spring (which is the same as in the Terzaghi theory) and that the model will not show secular effects. The illustration also shows the model underlying my theory. Directly after loading the water will be squeezed out and both springs will deform, but the piston in the dashpot moves only very slowly owing to the large viscosity (10^{13} to 10^{15} poises); so that the process after loading is mainly governed by the springs and is nearly similar to that in Terzaghi's theory. This first part is known as the primary consolidation; for larger values of the time the spring G_2 has reached its maximum shortening and the further process is mainly governed by the slow viscous movement of the dashpot, which is the secondary time effect.

I have made an extension of my model to an infinite series of parallel dashpots and springs, which is also shown.

In Fig. 11 water pressures calculated for a layer of 2 cm are shown. The difference in the isochrones calculated according to my theory and that of Terzaghi is negligible. This conclusion only follows for very thin layers of clay.

Fig 12 shows the isochrones calculated for a layer of 200 cm according to my theory and that of Terzaghi. It will be seen that for the same time factor T, the water pressure according

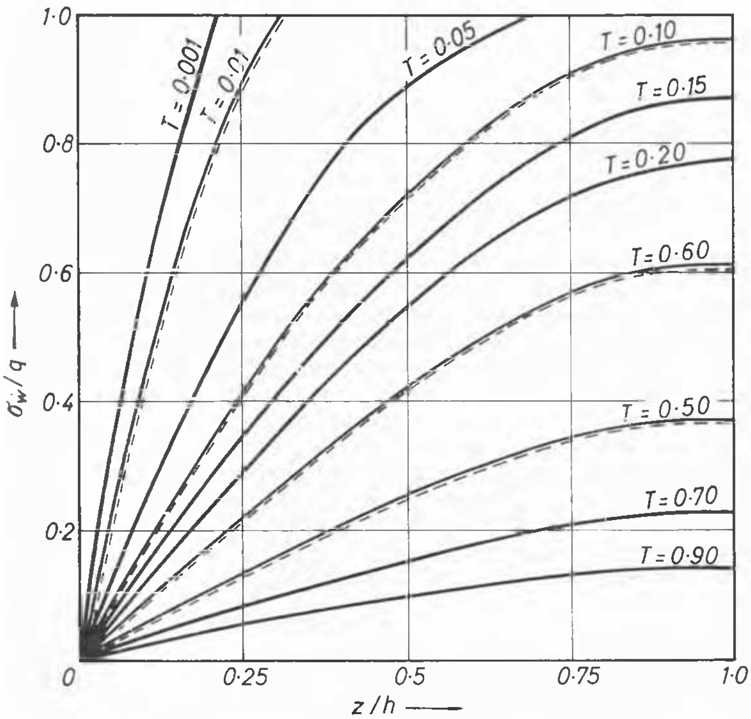


Fig. 11 Isochrones, after Terzaghi (dotted lines)
Isochrones d'après Terzaghi (lignes pointillées)

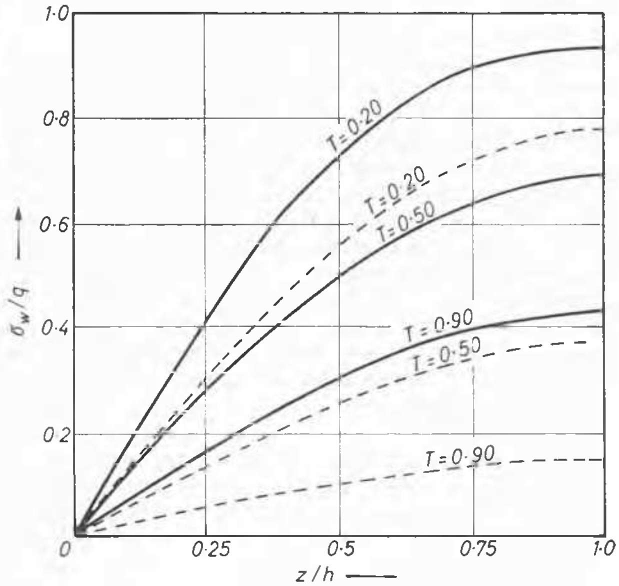


Fig. 12 Isochrones, after Terzaghi (dotted lines)
Isochrones d'après Terzaghi (lignes pointillées)

to my theory is always higher than that predicted by Terzaghi. This result is in agreement with the conclusions of Šuklje concerning the water pressure in layers of large thickness.

The settlement has been calculated for a layer of 2 cm thickness and it may be seen in Fig. 13 that the types of low secondary compression to high secondary compression may be

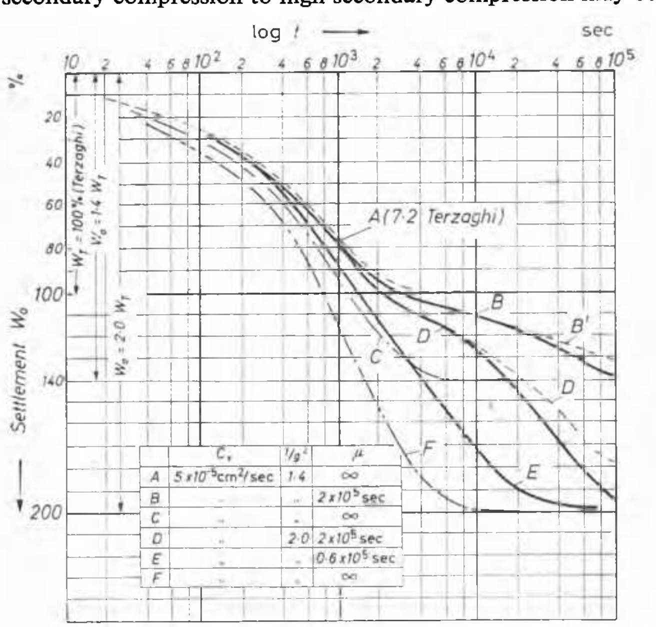


Fig. 13 Time settlement curves for $h=1$ cm, after Terzaghi
Courbes de tassement en fonction du temps pour $h=1$ cm, d'après Terzaghi

predicted from my theory depending on the coefficient of consolidation, the viscosity and the Poisson's ratio of the elastic part of the soil skeleton.

The same computation has been made for a compressible layer of 200 cm thickness as shown in Fig. 14. It may be seen that the logarithmic period may continue up to $12\frac{1}{2}$ years. It is obvious that the settlement after the hydrodynamic period should reach an ultimate value; according to my theory this may be:

$$\frac{1}{g^2} = 3\left(\frac{1-v_e}{1+v_e}\right)$$

or 1 to 3 times the settlement predicted by the Terzaghi theory.

One of the most complicated problems in applying a theory

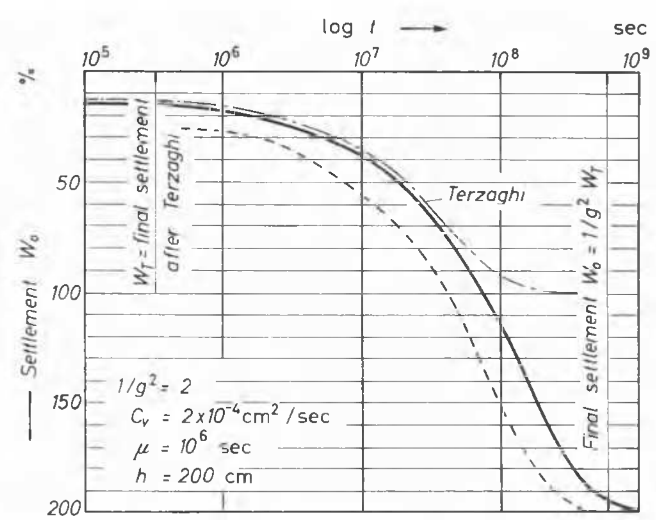


Fig. 14 Time settlement curves for $h=200$ cm, after Terzaghi
Courbes de tassement en fonction du temps pour $h=200$ cm, d'après Terzaghi

to practice is to measure the physical quantities. I will not discuss that now, but if you are interested in it I would refer you to my paper *Secondary time effects and consolidation of clays*, Academia Sinica, Institute of Civil Engineering and Architecture, Soil Mechanics Laboratory, Harbin, June 1957.

E. C. W. A. Geuze (Netherlands)

May I comment on certain points in the General Report of N. Janbu?

Since my own general report at the Zurich Conference included some suggestions on these points, I may do this very briefly.

I was at that time mostly concerned with the failure conditions, testing technique and interpretation. After the tests conducted by T-K. Tan and myself had shown that the clay material behaved perfectly as a rheological material, De Josselin de Jong and I endeavoured to study its behaviour following the more accepted techniques of triaxial testing. From these results, as presented in a paper to this conference, it follows that if either of the different techniques of triaxial or cell tests are applied, no virtual divergencies are to be found.

Once the shearing stresses are developed according to some programme of loading, the clay will behave as a rheological material, exhibiting flow characteristics which do not depend in the first place on the variation of stresses but on the magnitude of the deformation.

We are confronted with this behaviour as well in any state of stress involving shearing stresses only as in a pure state of compression in the initial stage. For we may well define the behaviour of the material on the strength of bulk stresses, but we are compelled to take the structural interparticle forces into consideration.

In limiting ourselves to the behaviour under a pure state of shearing stresses, a normally consolidated clay will show a shearing resistance, depending on the speed of deformation, so in the application of test results to practical problems we have to acknowledge a lower limit of constant stress, which may lead to an acceptable limit of speed of deformation, as I proposed in my General Report at Zurich. Therefore, any law expressing the shearing strength as a function of stress should contain this magnitude.

Secondly, I may point out that the application of the principle of pore pressure in combination with any law on the shearing strength is apt to lead to misleading results, as part of the system will take up the effective stresses depending on the nature of its resiliency.

I am afraid that we still will have to include some magnitude expressing the rigidity of this part of the system as a function of time and amount of deformation.

J. E. Jennings (Union of South Africa)

I should like to comment on Paper 1a/25 by A. W. Skempton and D. J. Henkel. Fig. 5 of this paper raises some very important principles to all who are interested in the changes in effective pressure which are brought about by changes in water tables, in particular the establishment of perched water tables from the condition where no such tables existed at the start. Looked at from the very long-time point of view, the ultimate conditions become the same as those given by A. W. Skempton, but the intermediate processes are somewhat different.

Considering first the case of the soil profile with a water table at depth D—Fig. 15a—and accepting the condition that there is no moisture deficiency due to desiccation in the soil, the negative pore pressure is given generally by

$$\bar{u} = \beta\gamma_w(D-z)$$

and is negative above the water table. β is a function related to Croney's factor α and Bishop's parameter B. In clays for practical limits of water table depth, β is probably near unity. For silts and sands β will become progressively smaller than unity as the particle sizes become larger.

If a perched water table is now established in the upper sand as shown in Fig. 15b, the effect will not be felt in the lower clay until time has elapsed. At the start an equilibrium condition may be visualized as if an impermeable membrane existed on top of the clay layer and for this condition the pore pressures in the clay will still be negative above the water table. This concept of an effective impermeable membrane is a real

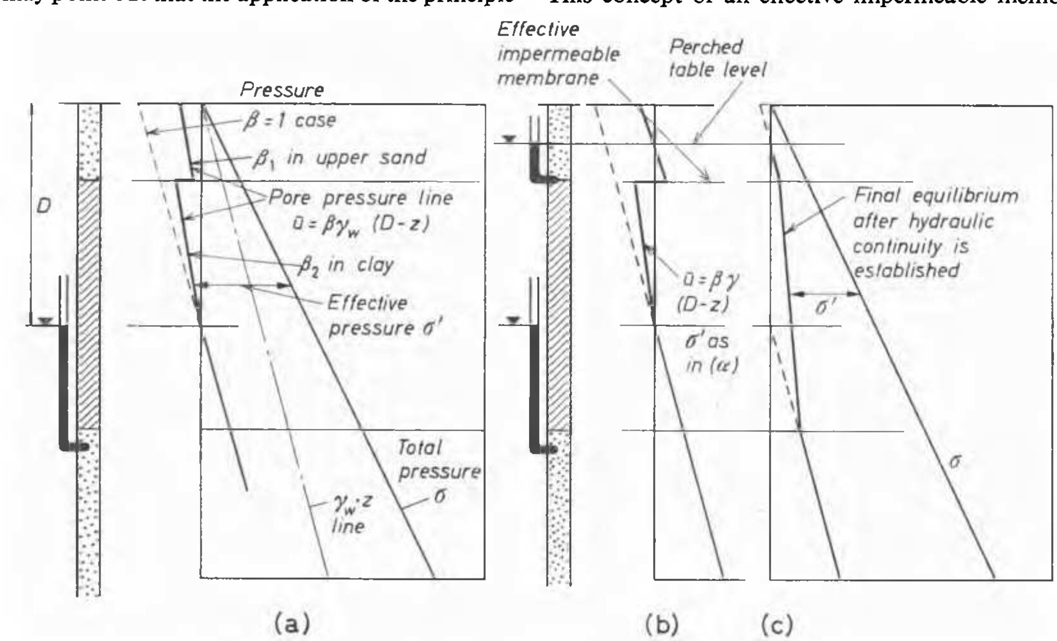


Fig. 15 Pore pressure changes during the establishment of a perched water table—conditions at final equilibrium: (a) with deep water table, (b) immediately after perched water table is established, (c) after infinite time with dynamic equilibrium conditions established between upper and lower water tables

Changements dans la pression interstitielle pendant la realisation d'une nappe d'eau suspendue—conditions d'equilibre final: (a) avec nappe d'eau profonde, (b) immediatement après la realisation d'une nappe d'eau suspendue, (c) après une periode de temps infinie avec conditions d'équilibre dynamique entre les nappes supérieures et inférieures one, since many active clays exhibit a very great decrease in permeability on wetted boundaries and the presence of very dry clay beneath a perched water table is a common observation in Africa. This is more important when the upper perched water tables are seasonal in character.

If, however, the perched water table level is maintained indefinitely and complete hydraulic continuity is established between the upper and lower water tables, flow of water takes place directly as in a simple permeability test under the gradient shown in Fig. 15c, which corresponds to Fig. 5 in Paper 1a/25. This is probably also the mechanism of the perched water table resulting from lowering of water by pumping. The important difference in the case of the wetting up condition is the existence of the negative pressure conditions in most practical cases.

K. S. Bawa (U.S.A.)

I would like to compliment the authors, J. Florentin, G. L'Heriteau and M. Farhi (1a/7) for adding to the much needed data concerning the engineering properties of laterite soils. Such data can be of considerable value in the preliminary planning and design of engineering structures in areas where similar soils are encountered.

Due to lack of sufficient information concerning the expected behaviour of laterite soils we had to undertake, some time ago, an extensive soil exploration and testing programme in connection with the design of an earth dam in Medellin, Colombia (South America). In our investigations, which we hope to report soon, we encountered soils similar to those reported by the above authors in their paper. However, in our work a differentiation is being made (Bawa, 1957) between laterites and lateritic soils which is not indicated in Paper 1a/9. This distinction is based on the magnitude of the silica-sesquioxide ratio commonly used in reference to these soils. In the absence of any other well known distinguishing property, we use this ratio to identify laterites and lateritic soils.

I would like to take the opportunity at this conference to urge engineers working in different parts of the world to report their observations concerning the physical properties and engineering behaviour of these soils, wherever found, as there is a dearth of such data. A detailed soil description and proper identification, whether the deposit is laterite or lateritic soil, will facilitate considerably the proper handling of these soils in future and would eliminate unnecessary soil investigations.

Reference

Bawa, K. S. (1957). Laterite soils and their engineering characteristics. Paper No. 1428. *Proc. ASCE–J. Soil Mech. Found. Engng.*, **83**, No. SM4 (Pt. 1) Nov.

T. W. Lambe (U.S.A.)

I should like to give a few words of explanation on my conception of the role of water in soil, particularly after the comments I have received. I wish to emphasize the fact that I consider water very important to soil behaviour, and I did not mean to give any different impression. The properties of water, particularly the dielectric constant, in developing the colloidal properties of the soil are well recognized. I also wish to emphasize the great importance of pore water tensions and our knowledge of intergranular pressures.

As I pointed out this morning, the last two or three molecular layers of water are held very strongly to the soil surface. We have made measurements by isotherm adsorption of as high as 3000 atm. required to pull this water off.

I did not question the fact that tension can be mobilized between the water and the soil. To me the next step, on assuming the fact that that soil can mobilize tension, i.e. to assume that the water is using this tension to hold particles together, is an erroneous concept. That is the impression I meant to leave this morning.

I want to emphasize again that I do not think that this potential tension holds particles together. If so, how would we explain the positive pore pressures that are measured in shear on samples having high strength?

I would also like to say a word or two about the question, how dry is a dry soil. The speaker who commented on my dry strength said that I had not dried the sample: we have made tests with samples dried all the way to 1200° C. If you make measurements of strength at all drying temperatures I think you will find that the strength increases. As best we can tell, the whole of the water has been removed from the soil at about 150° C.; at about 600° C. we have driven off all of the OH groups: this constitutes polymerization. Even if you get the moisture driven out at 600° C. you still have cohesion: if you go higher, you can get complete fusion and get a brick. Surely the gentlemen from the U.K. would not speak of water in the soil in this brick!

My concept of no water bond should not startle anybody. In all of the classical colloidal chemical work I have never seen a description of a water bond. The forces between these colloids have been explained completely without resort to a water bond. I think that this adsorbed moisture very close to the particle is extremely important in soil behaviour, particularly in time effects, in secondary compression, and in strength build-up during the secondary compression.

P. W. Rowe (U.K.)

Regarding the first proposal for discussion, I wish to emphasize the difference between shear strength at failure involving viscous flow, and mobilized shear strength when the soil is in equilibrium. If long-term failures, involving full shear strength, give satisfactory analyses taking $C'=0$ with short-term measurements of ϕ', then this may be consistent with final failure taking place in a few hours or days; but if a structure prevents motion with a complete cessation of viscous flow, then we must expect lower values of ϕ'. In this connection Paper 5/2 by E. DiBiagio and L. Bjerrum should be studied. If clay really possessed a true angle of shearing resistance in friction, such as Hvorslev's angle ϕ_e which Skempton and Bjerrum accepted as the most fundamental basis for shear strength at the last conference, then the absolute lower limit to ϕ' for calculating equilibrium pressures cannot fall much below ϕ_e. This seems to be in agreement with D. H. Trollope's studies of the clay matrix and, I believe, with E. C. W. A. Geuze's approach. In the lifetime of engineering structures the mobilized angle of shearing resistance with respect to effective stresses may be expected to lie between ϕ' from short-term tests and ϕ_e. A. W. Bishop and D. J. Henkel suggest the practical limit of $0{\cdot}8 \tan \phi'$ presumably for clays of low to medium plasticity.

Turning to the third proposal for discussion, Paper 1b/8 by B. Jakobson presents fundamental information by plotting the instantaneous 'Poisson's ratio' μ for sands against the mobilized angle of shearing resistance. This relationship can be calculated provided the true angle of friction ϕ_μ between grains of the quartz is known; however, Jakobson does not state these values. Taking for his sand A ϕ_μ 17·3° and for sand B ϕ_μ 23·7° agreement between theory and observations can be obtained. He quotes maximum angles of shearing resistance differing by 6·5°, which is of the same order as the possible differences in true intergranular friction ϕ_μ between the two sands which I have suggested.

Finally, M. ROCHA (Paper 1b/11) is incorrect in his general statement that similarity between model and prototype cannot be obtained if the soil has mass. For sheet piling, for example, we do not require his factor $1/\alpha$ to be equal to unity but to equal his $1/\lambda$.

T. K. E. KALLSTENIUS (Sweden)

I want to say a few words about the two types of normal sand demonstrated by B. Jakobson. Even large photographic enlargement does not reveal to the unskilled eye any evident differences between the grains of those two sands, but a careful petrological study reveals that the relationship between the average radius of protruding edges and average radius of the whole grains is about 10 times greater for the sand with the greatest angle of friction than for the other sand. This teaches us to take petrology into serious consideration when studying the behaviour of sands and to present that data when making reports on research concerning the qualities of sand.

S. J. BUCHANAN (U.S.A.)

In listening to the discussion regarding the shear properties of soils and their elastic behaviour I was reminded somewhat of a remark that the late T. A. Middlebrooks made at the Zurich Conference, when he voiced the thought whether soils were cognizant of our assumptions regarding homogeneity and elasticity. Within the last six months I have been very pleased to find that soils sometimes behave more elastically than we normally think.

We have been investigating the behaviour of sands and clays subjected to repetitive loading in a triaxial device, using a 4 in. diameter device, and subjecting the clays and sands to two comparable pressures. We have found that after some 20 repetitions of stress we have the specimen behaving elastically with lean clay, the standard Ottowa sand and a very angular and graded sand. For example, we find that our modulus of elasticity averages about 20,000, our Poisson's ratio ranges from 0·15 to 0·2 and our relative density ranges from 0·7 to 0·95.

These values and this behaviour cause me to ponder the use of this information for highways and for airfield pavements and subgrades, because there we do have repetitive loads. I think that possibly in the past I have been a little too prone to think of loadings of soils of the static nature, so that I think as we get into more advanced design of airfields and highways it would be well for us to take a look at the behaviour of our materials subjected to repetitive loading.

E. T. HANRAHAN (Ireland)

I would like to describe an interesting phenomenon observed in connection with the shearing behaviour of boulder clay. A large number of tests have been carried out on remoulded specimens of this soil with the fraction coarser than B.S.S. No. 7 removed, and compacted in a screw press to a condition of zero air voids. The type of test was the quick-shear triaxial test with pore pressure measurements. Fig. 16 shows the observed relationship between water content and deformation of the specimen at failure.

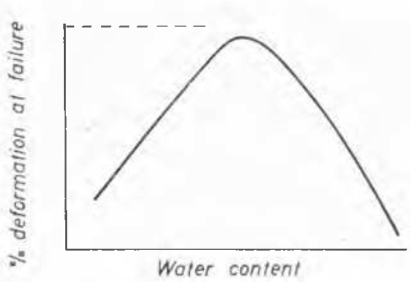


Fig. 16

Variations of the magnitude of the axial deformation of the specimen in the range of 5 to 40 per cent were recorded. The water content corresponding to the peak of the curve was within a few per cent of what might be termed the 'corrected PL' of the soil.

A. KÉZDI (Hungary)

M. J. Hvorslev referred in his remarks to Paper 1b/2 by A. BALLA. As he is working at the Technical University of Budapest I should like, in his absence, to make a few remarks on his work and to present some test results which he has achieved since writing the paper.

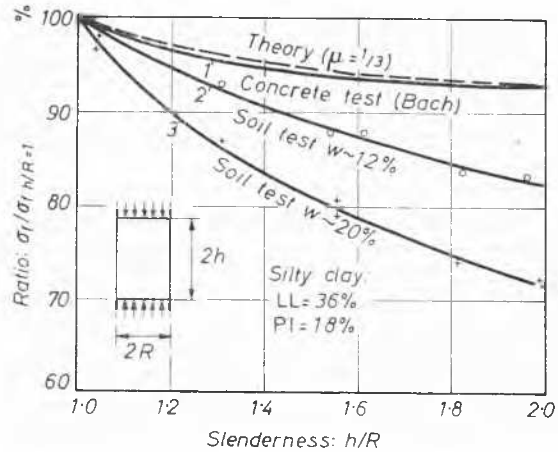


Fig. 17

His theory offers the possibility of investigating the stressed state of the unconfined compression test ($p_2=0$). In this respect it is interesting to examine the effect of the slenderness of the specimen and of the roughness of the loading plate on this state of stress. Balla considers, as a practical approximation, as compression strength the vertical p_1 stress which acts when the plastic domains developing from the boundary and from the centre meet. The extension of the plastic domain could be determined by using the three-dimensional condition of plasticity. The so defined compression strength has been plotted *versus* slenderness in Fig. 17 (compression strength in the case of $h/R=1$ is chosen 100 per cent). The same diagram gives test results for a clay (Balla's tests) and for concrete (Bach's tests). In the case of concrete, the theoretical curve

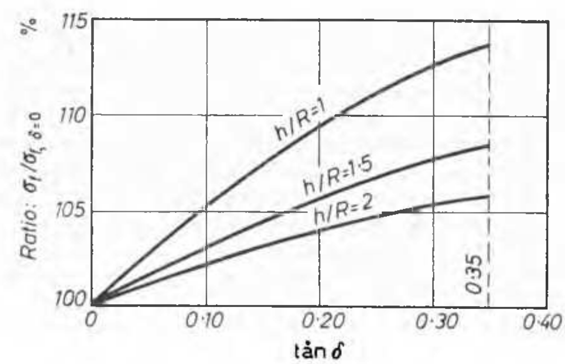


Fig. 18

fits the experimental data very well; in the case of clay, only the character of the curves is the same.

Fig. 18 gives the variation of the compression strength *versus* frictional coefficient on the loaded surface of the specimen (at $f = \tan \delta = 0$, $\sigma = 100$ per cent), with different values of h/R. The strength decreases with increasing slenderness, but increases with increasing roughness of the loaded plate. If the loaded plate is completely smooth, the compression strength does not depend on slenderness.

J. Kolbuszewski (U.K.)

I should like to refer to sands which create difficulties from time to time, not only in the Swedish laboratories but also in many other laboratories. We often have two samples of sand which, when judged by the eye, look exactly the same, and in grading analysis give two curves coinciding on the same graph, but when subjected to similar testing conditions give results very often quite different.

At our laboratories we have spent some time in looking for the reason, and one of the reasons is very simple. When we study the grain of sand we have to be able to measure and describe that grain in some way, which requires the assessment of two values, the roundness and the sphericity. These two values describe how far the grain is away from a real sphere and the surface.

We find, conducting a very simple experiment—for example, pouring the sand into a container—we get the value that is generally believed to be the maximum porosity or the minimum density for a given material, which differs according not only to the rate of pouring but also according to the values of roundness and sphericity. Unfortunately, when one reads the literature one finds that many people use for the description of their sands a method which, if applied in the case of clays, would correspond to testing by pushing in an umbrella to get the PI. Nobody would like us to do that any longer, because we have standard tests for LL and PL. However, all the methods which are at the moment applied to sands are very much of the umbrella character, because they do not take into account the influence of these two fundamental values that govern, in addition to the grading and the specific gravity, the behaviour of each sand. I would suggest that the methods described by Waddell or Rittenhouse should be adopted in soil mechanics for the complete description of our sand materials.

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

My comments are prompted by the appearance in the Proceedings of numerous papers regarding the strength of clays under long-duration loads, as determined by creep tests.

I would like to present a hypothesis for the failure of a clay sample after some duration of load application. When a deviator stress is applied to an undrained triaxial specimen pore pressure gradients are set up within the sample and a migration of the pore fluid results. Even after the pore pressure gradients have decreased to very small and perhaps unmeasurable values movement of the pore fluid continues. I believe that in many soils there is a tendency for water to migrate into the zone of largest shearing strains because of an increase in the adsorptive capacity of the clay minerals in this zone as the result of the shear process. Thus, the shear strength decreases with time, and ultimately the sample may fail under the fixed deviator stress.

Let us now pass to the interpretation of creep tests. If the long-term strength decrease is the result of water migration the time factor observed in a creep test will depend not only upon the soil properties (i.e. the desire of the clay particles to adsorb additional water and the amount of water to be adsorbed for a given strength decrease) but also upon the geometry of the sample and the whole pattern of the strains within the sample. Thus a very careful interpretation of creep test results will be needed before it will be possible to apply in a useful manner the time factor values obtained from these tests.

P. J. Alley (New Zealand)

With so many academic people at this conference it would be useful to exchange in some manner our teaching methods and our laboratory procedure for the preparation of undergraduates for their examinations, and also for their knowledge in after life. Naturally there should not be any set system of gaining these requirements, and academic freedom and choice of teaching methods at all times should be observed. A teacher tends to become stereotyped in his methods, and an exchange of information would be of value.

At Canterbury University College the aim in the laboratory is that the student shall perform as much individual work as possible. This can be accomplished in the simple tests, such as LL, PL, shrinkage limit, linear shrinkage, specific gravity, moisture content *versus* the number of blows, particle size determination with hydrometers and pipettes, mechanical analysis, etc. To demonstrate techniques it is advisable to perform the test first, and not to leave this to printed instructions. Other tests such as permeability tests, compaction, road tests, triaxial compression, unconfined compression and direct shear tests are better run in groups. Settlement tests can be well performed by allowing an interval of 5 to 10 minutes between each increment of loading. To complete the course, field trips are made, and augering, sampling and undisturbed sampling are done, and field density by all the known methods performed. Also vane and penetration equipment is demonstrated. The course for the laboratory work is 36 hours, lectures 26 hours and problems 26 hours. The present area of the laboratory is 1000 sq. ft., but the new engineering school will have 2000 sq. ft. The present space permits groups of 12 students to work at one time. Soil mechanics is a separate subject in the university syllabus.

The Chairman

I now ask the Assistant Reporter to summarize the discussion.

Assistant Reporter

It is quite impossible in the short time that is available to cover the content of all the contributions that have been made and, moreover, to do so in such a way that the comments would be worth listening to.

I will therefore simply say that probably the most important impression I have from this discussion is that the profession is certainly becoming more and more aware of the circumstance that the shear strength characteristics and the deformation characteristics of soils are by no means material constants. On the contrary they may depend on a large number of factors which I believe we have no possibility of agreeing on now—that gives us just one more reason for looking forward to the next conference!

M. E. Buisson (France)

Il arrive souvent que des sols sablo-limoneux atteignent des résistances relativement élevées à la compression, lorsqu'ils sont secs. D'une façon classique, on se méfie de ces sols, et on leur attribue, en général, une force portante qui ne dépend pas de la cohésion et est, par conséquent, basée uniquement sur le frottement. Dans certains cas, néanmoins, on est amené à envisager des fondations sur de tels sols pour rechercher le maximum d'économie. Dans ces conditions, il est nécessaire de mesurer la résistance notamment à la compression de ces sols, lorsqu'ils sont naturellement saturés ou bien de n'attribuer aux sols en question que la résistance qui est trouvée soit aux essais triaxiaux, soit éventuellement aux essais de cisaillement après saturation naturelle, par simple mise en présence de l'eau.

Au laboratoire du Bureau Veritas, nous procédons simplement sur des échantillons prélevés en vue d'essais de compression sans contrainte latérale. Laissant ces échantillons dans le tube carottier, on les empêche de gonfler et on les met en présence d'eau jusqu'à absorption d'eau maximum. A ce moment, on décarotte les échantillons et on les essaie à la compression. Ils contiennent en général encore un peu d'air, mais il semble que ce procédé soit suffisant pour obtenir l'abaissement maximum de la valeur de la résistance à la compression, au moment où la saturation est pratiquement atteinte.

Nous avons ainsi trouvé des résistances pouvant varier de plusieurs kilogrammes par cm^2 à quelques centaines de grammes seulement, avec des gammes intermédiaires correspondant à une teneur en argile plus importante.

Ce procédé très simple permet de prévoir l'influence de l'intrusion éventuelle de l'eau sur la stabilité des fondations du fait d'une élévation éventuelle du plan d'eau dû à des fuites de canalisations.

Bien entendu, les précautions en résultant doivent être complétées si l'absorption d'eau ou l'évaporation entraînent des changements de volume dans le sol. Mais il arrive souvent, en France, que ces changements de volume deviennent très faibles dans un grand nombre de cas, ce qui justifie ce procédé.

W. M. Kirkpatrick (U.K.)

The following is in reply to questions raised by P. Habib in the oral discussion in reference to my Paper 1b/9.

Habib's first question refers to the results of the triaxial compression and extension tests on a sand described in my paper. These results showed that there was no appreciable difference in the angle of internal friction measured in the two types of test. This finding is in agreement with results published by Bishop and Eldin (*Proc. 3rd Conf.*, Vol. I, p. 100) but differs from results reported by Peltier (including results of P. Habib) (*Proc. 4th Conf.*, Vol. I, p. 179).

The differences between my results in the triaxial tests and those stated in the paper by Peltier must be due to some basic difference in the conditions of test. Unless these conditions are known fully one can only speculate as to the reasons for the differences. I know what the conditions in my tests were and these were published, in as great detail as space allowed, in the Proceedings. Very little in the way of test detail was however given in Peltier's paper.

A statement made by A. Casagrande in his introduction to the discussion in Session 1 of the 3rd Conference can aptly be recalled here—'without full knowledge of every detail of a test on the strength of clays, it is virtually impossible for me to understand why I obtain results and arrive at conclusions

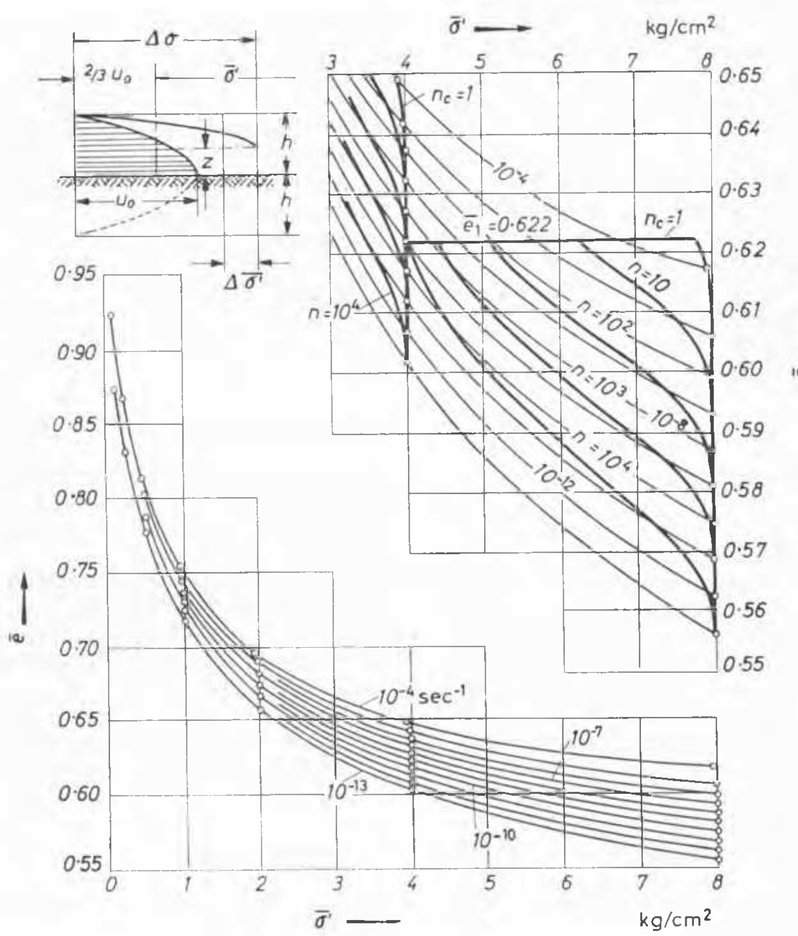
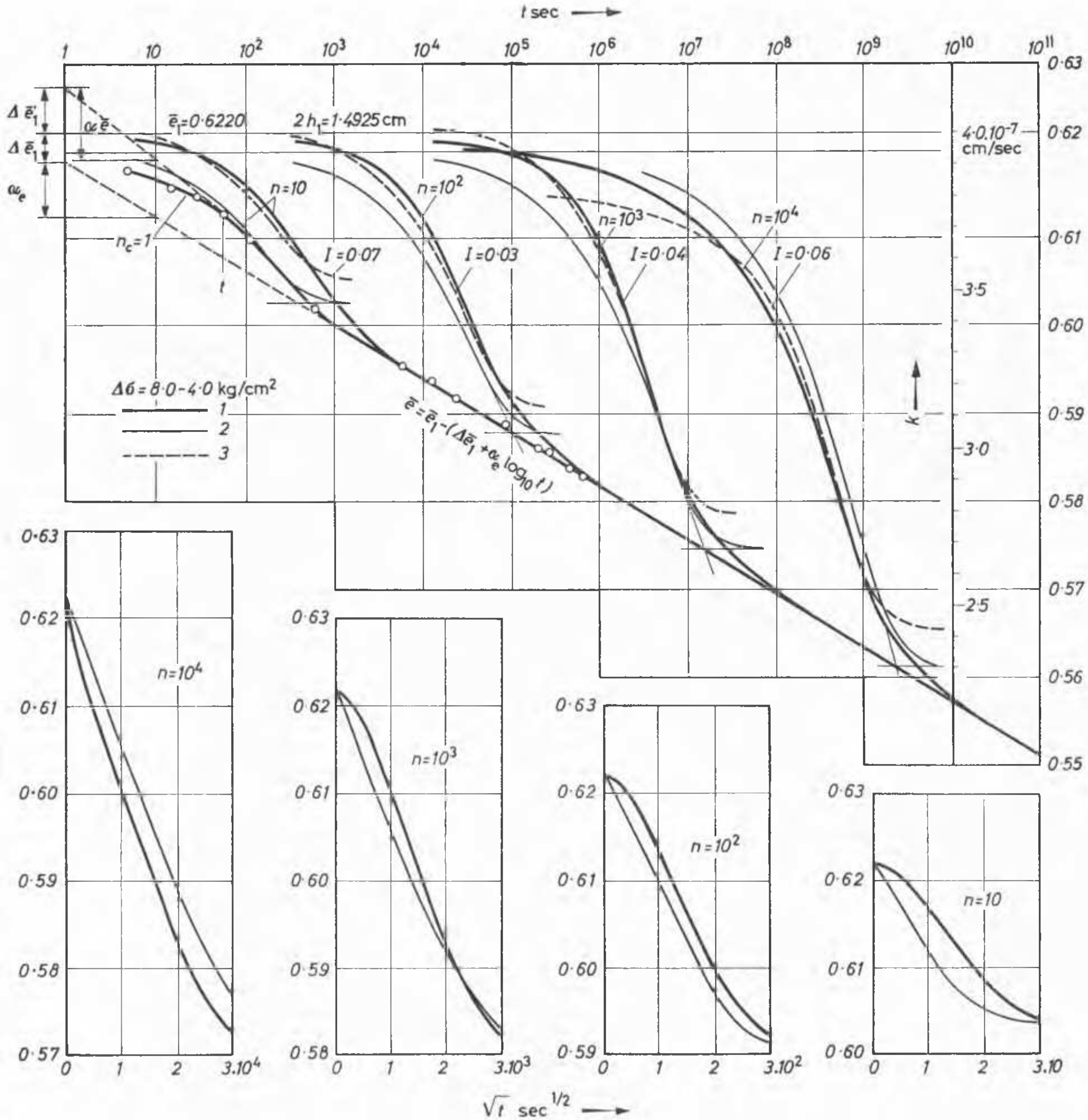
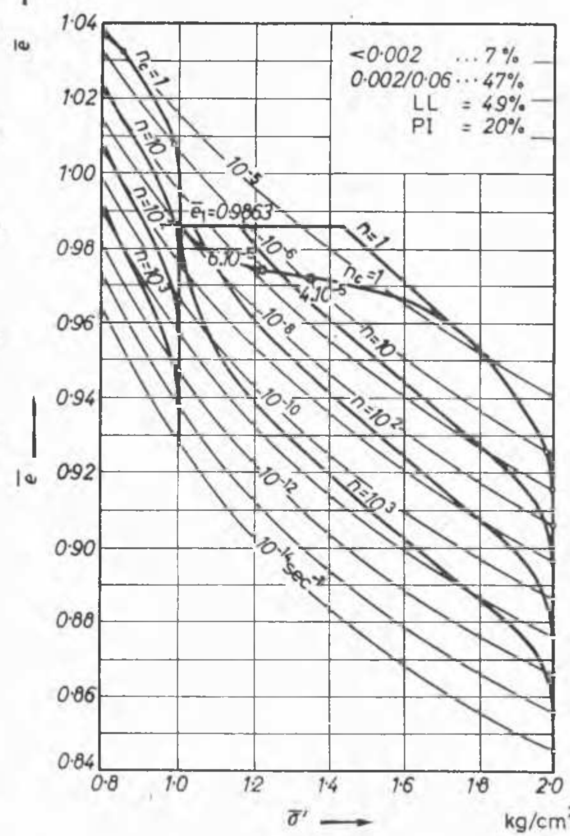


Fig. 19



t sec
$\bar{e}_1 = 0{\cdot}6220$
$2h_1 = 1{\cdot}4925$ cm
$4{\cdot}0.10^{-7}$ cm/sec
$n=10$
$n_c=1$
$n=10^2$
$n=10^3$
$n=10^4$
$I=0{\cdot}07$
$I=0{\cdot}03$
$I=0{\cdot}04$
$I=0{\cdot}06$
$\Delta\sigma = 8{\cdot}0-4{\cdot}0$ kg/cm²
$\bar{e} = \bar{e}_1 - (\Delta\bar{e}_1 + \alpha_e \log_{10} t)$
$\sqrt{t}$ sec$^{1/2}$

Fig. 20



<0·002 ... 7 %
0·002/0·06 ··· 47%
LL = 49%
PI = 20%
$\bar{e}_1 = 0{\cdot}9863$
$n=1$
$n_c=1$
$n=10$
$n=10^2$
$n=10^3$
10^{-14} sec^{-1}
$\bar{\sigma}'$
kg/cm²

Fig. 21

which are different, fundamentally different at times, from those obtained in other laboratories'. This statement was made in reference to the reporting of tests on clays but it is applicable to the reporting of tests on sands, concrete, or any other material.

Habib asks if the differences are due to the grading of the sand? I should not think this to be so, but it could easily be proved by comparing the grading curves for the materials used in the tests reported by Peltier, which were not illustrated, with the curve for the material used by me given in my paper.

I think a more likely reason for the differences is a misunderstanding in the meaning of the term 'failure'. In my paper failure was defined as the peak point on the stress–strain curve. For tests on sands it is typical that the deviator stress increases from zero to a maximum and then reduces to values lower than the maximum. The maximum, or peak point, is the only definite point on the stress–strain curve. My results agree with those of Bishop and Eldin where failure is similarly defined. No definition of failure can be found in Peltier's paper but if it were not the peak point the reason for the differences between the results may not be hard to find. If failure was assumed as occurring at some arbitrarily chosen axial strain in the two tests the deviator stress would be lower (and consequently the angle of internal friction) in the case of the extension test. The reason for this is that the deviator stress–axial strain curve for the compression test is steeper and reaches its maximum, the same maximum as for the extension test at an equivalent porosity, at a lower value of axial strain than in the extension test. The fact that the curves for the two tests are not comparable is not remarkable since the axial strain is a major principal strain in the case of the compression test and a minor principal strain in the extension test.

Habib's other question was on the effect of dilation on the value of the intermediate principal stress in the thick cylinder tests.

The effect of dilation should not alter the value of the axial

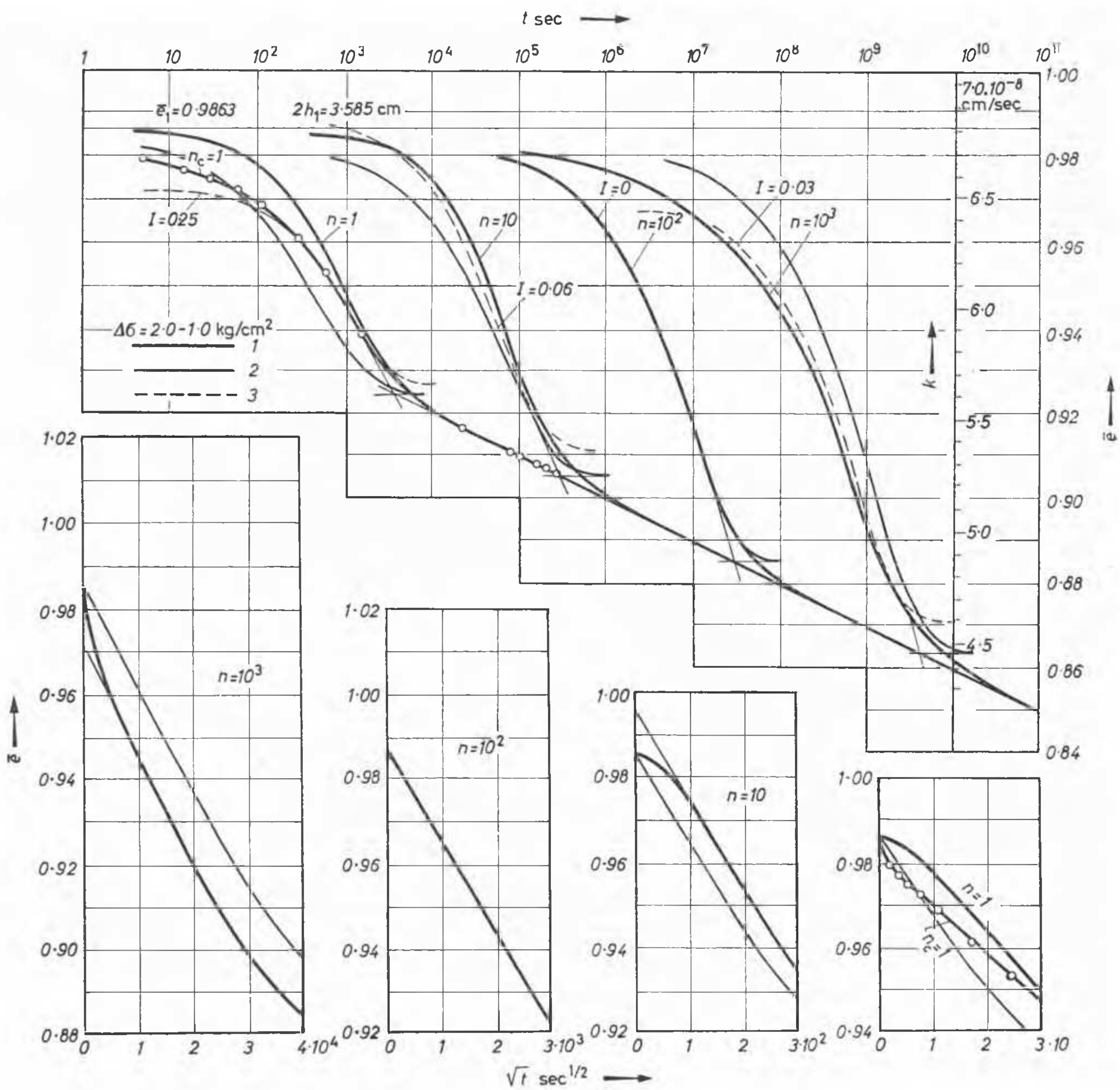


Fig. 22

stress from that calculated, neither should it alter the position of the intermediate principal stress (axial stress) in relation to the major and minor principal stresses. Even if these alterations did take place the value of the axial stress must be such as to remain intermediate between the values of the other two principal stresses, otherwise the mode of failure of the sample would be different from that observed.

The results of the thick cylinder tests act to confirm those of the triaxial tests and allow the conclusion to be drawn that the Mohr–Coulomb theory is applicable in predicting failure in sands under fully drained conditions. If, for the sake of argument, it is assumed that the axial stress in the thick cylinder tests adopted a value either closer to the major principal stress or closer to the minor principal stress than that calculated, the points on the surface of failure that this revised stress system represents would still lie close to the theoretical Mohr–Coulomb surface but they would be nearer to the directions 1 or 2 than those illustrated in Fig. 9 of my paper. The thick cylinder results would thus still provide confirmation of the applicability of the Mohr–Coulomb theory.

Correction to the Paper 1b/14

THE ANALYSIS OF THE CONSOLIDATION PROCESS BY THE ISOTACHES METHOD

by L. Šuklje

The validity of the equation 13 is limited by the conditions

$$u_0 \leqslant \Delta\sigma \qquad \text{.... (a)}$$

$$t \geqslant t_0 = \frac{h_s^2 \gamma_w \alpha_e (1+\bar{e})}{4{\cdot}6052k} \qquad \text{.... (b)}$$

When $t < t_0$, half parabolic isochrones may be supposed having maximum value $u_0 = \Delta\sigma$ at a distance z from the midplane (Fig. 19). Equating the corresponding seepage speed at the boundary surface and the consolidation speed expressed by equation 8, gives

$$h - z = -\frac{4{\cdot}6052k\Delta\sigma t}{\gamma_w \alpha_e h_s} \qquad \text{.... (c)}$$

Thus the mean additional intergranular pressure, defined by the condition

$$\Delta\bar{\sigma}'h = \tfrac{1}{3}\Delta\sigma(h-z) \qquad \text{.... (d)}$$

may be expressed

$$\frac{\Delta\bar{\alpha}'}{\Delta\sigma} = \frac{1{\cdot}53507}{h_s^2 \gamma_w} \cdot \frac{kt\Delta\sigma}{(1+\bar{e})(-\alpha_e)} \qquad \text{.... (e)}$$

Using equation e, the initial parts of some curves $(\bar{\sigma}' - e)_{n=\mathrm{const}}$ and $t - \bar{e}$, presented in Figs. 3–6 of the paper, must be corrected in the way shown in the new Figs. 19–22.

This correction has no further influence on the text of the paper and does not change the conclusions. Nevertheless it may be pointed out that the primary consolidation of thicker layers as derived by the isotaches method can be faster than the one corresponding to the Terzaghi's theory.