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particles are produced from this region. The larger particles are produced from the region near the collar of the hole. It is recognised that structural weakness planes will have a significant effect upon the degree of fragmentation. Model tests have been planned to investigate this phenomenon in detail.

Model testing is a slow process and field testing on even a moderate scale is even more costly and time-

consuming. However larger-scale tests are essential due to the lack of knowledge regarding scaling factors as applied to size distributions. A limited number of such tests have been conducted.

In conclusion it should be noted that the prediction of size distributions from blasting may also be relevant to Civil Engineering operations where a particular grade of rock fill is required.

TECHNICAL SESSION No. 6—FOUNDATIONS

"An Analysis of Pile Loading Tests in a Stiff Clay", S.B. Bromham & J.R. Styles.

"Model Tests on Piles in Clay", N.S. Mattes & H.G. Poulos.

"A High Capacity Load Test for Deep Bored Piles", J.D. Moss.

"Analysis of the Movements of Battered Piles", H.G. Poulos & M.R. Madhav.

"The Bearing Capacity of Strip Footings from the Standpoint of Plasticity Theory", E.H. Davis & J.R. Booker.

"Uplift Testing of Prototype Transmission Tower Footings", R.J. McKenzie.

"Stresses Beneath Granular Embankments", I.K. Lee & J.R. Herington.

GENERAL REPORTER - Prof. P.W. TAYLOR:

Of the seven papers presented at this session, five are concerned with piles, one with spread footings and one with embankment stresses. Your Reporter considers that this emphasis on piled foundations is desirable for, despite the fact that civil engineers have employed piled foundations for centuries, estimates of load deflection relationships and ultimate bearing capacities are still subject to a wide margin of error. For this reason, high 'factors of safety' are commonly used. More exact knowledge will undoubtedly lead to more economic design. The three papers dealing with full-scale loading tests will be considered first.

That by Bromham and Styles is of considerable interest as it attempts to compare three methods of estimating ultimate bearing capacity with results of field loading tests. Concerning field and laboratory

soil tests, it would be of interest if the authors could give the liquid limits for the soils tested and a definition of 'friction ratio' for the penetrometer. Perhaps the authors could state also whether or not 'N-values' were corrected for vertical effective stress (Ref. D1) and whether lubricated end platens were used for the triaxial tests on samples with a height/diameter ratio of unity. Referring to Fig. 5, showing time-dependent deflection under constant load, it is difficult to accept the statement that "secondary consolidation effects begin after about 30 minutes". One would expect the start of secondary consolidation to be indicated by a reverse curvature, as shown in Mattes and Poulos' paper (Fig. 2) for model piles.

In the pile loading test at 38 ft. depth, failure is clearly defined but, for that at 76ft., the load was still increasing rapidly at 0.35 in. deflection and while the elastic limit may have been passed,

true yield has not occurred, and the ultimate bearing capacity could well have been considerably greater than the maximum load applied.

For the estimation of 'skin friction' in clay, the remoulded shear strength would seem, to your Reporter, to be an obvious though perhaps conservative value to choose, rather than to apply some arbitrary value of C_u/C_u . This ratio (used by Bromham and Styles and also by Mattes and Poulos) must be strongly influenced by the sensitivity, which is not stated in either case. Bearing capacity factors are strongly dependent on the value of ϕ_u used. It seems surprising that the soil strength parameters, and hence bearing capacity factors, at 76 ft. depth (in medium sand) were taken as the same as 38 ft. depth (in very stiff clay). Analysis in terms of effective stress, using the results of drained tests, might have been more appropriate for the sand. As no strength tests are reported on samples from below 50 ft., an estimated value of ϕ' for the sand at 76 ft. depth might have provided a more realistic approach. Conclusion number 2, it is respectfully suggested, is a slight overstatement.

The paper by J.D. Moss describes a loading test in which up to 400 tons could be applied to a single pile, the reaction being obtained, not by dead weight, but from deeply anchored prestressing cables. While this is not the first time the method has been applied on the Waitemata sandstones in Auckland (Ref. D2) it is surprising that wider use is not made of the system, which appears far less cumbersome than the provision of kentledge, supporting beams, etc.

A wide variation in strength with depth (at depths only a few inches apart) is a well-known feature of the Waitemata series, clearly shown in Fig. 1, though the usual tendency for the strength to increase with depth does not appear. Your Reporter's own experience has shown that one factor strongly influencing the measured compression strength of these soft sandstones is the accuracy to which the ends of the samples are trimmed. Both Ministry of Works, and the Reporter's own tests have usually shown ϕ_u nearer 20° than the high values given.

The paper by R.J. McKenzie includes full-scale tests on piles (both parallel and belled) and on 7 ft. square spread footings at 14 ft. depth. The record of these carefully - conducted tests is a welcome addition to the literature on a topic which is otherwise sparsely documented.

The use of the Brazilian indirect tensile test is noteworthy. Almost without exception, we assume that soils fail in shear, but there are some problems (such as this) where tensile strength is of importance. In a 'deeply fissured' clay, as at the Keilor site, a wide scatter of results in both tensile and compression tests is to be expected.

The relations between theoretical and actual results are clearly set out in the conclusions to the paper. The author could, perhaps, enlarge on the statement in Section VII that piles with enlarged bases are not acceptable because of low reserve capacity. Can this not be overcome by applying an increased safety factor in design?

The paper by Mattes and Poulos is an experimental evaluation of earlier work, carried out at the

University of Sydney, in which elastic theory is applied to estimate the load settlement relationships of single piles, and of pile groups, while that by Poulos and Madhav, "Analysis of the Movements of Battered Piles" is a continuation of this theoretical work. The latter paper will be considered first.

The theory is based on the use of Mindlin's equations for displacements caused by forces acting within an elastic medium, rather than on Winkler's assumption, which had previously been used in analysis of pile groups. It is, perhaps, only to be expected that displacements of piles caused by axial or normal loads or moment are not appreciably affected by slopes of up to 30° but it is comforting to have one's intuition reinforced by theory.

The example of a single pile shown in Fig. 2 is interesting. The vertical and horizontal loads (40T. and 8T. resp.) may be combined into a single load (of 41T.) acting at 11° to the vertical. If the point of application of this load is 1.2 ft. to the right of the pile top, the moment of 50T. ft. is also applied. The two loads and moment are thus replaced by a single force. Considered in this way, the reasons for the variations in displacements and rotation with batter angle may be better appreciated.

The extension of the theory to groups of piles (some sloping) should make it a useful tool for the practising engineer. The approximation used to deal with the case where the planes of the loads and of the sloping piles are different, appears to be a reasonable one. The term "fixed-head" refers to a pile which is fixed in direction but not in position. Have the authors looked into the question of just how "massive" a pile cap must be, for the fixed-head assumption to be reasonably accurate? With regard to the example of group analysis given, the authors state: "the beneficial effects of the battered piles may clearly be seen". While this is true in the example quoted, it is not universally so. The fairly common practice of including a few sloping piles to "carry horizontal loads" when there is a much larger number of vertical piles, may have effects which are far from beneficial.

Returning to the paper of Mattes and Poulos, the elastic theories for pile deflections are here put to the test, with model piles. The authors overcome the difficulty, noted by Bromham and Styles, of determining soil moduli by using their theory (in reverse, so to speak) to find E and E' from tests on models at one length-to-diameter ratio, ($l/d = 25$) and applying these moduli to check the theory at other ratios ($l/d = 10$ and 40). The difficulty of applying elastic theory, which assumes a linear stress-strain relationship, to soils is that real soils invariably have a non-linear stress-strain relationship. At low stresses, however, the non-linearity is small enough to make the assumption of elasticity a reasonable approximation. This can be seen in Fig. 4, showing the static test results to have been made at about one-quarter of the failure load, that is, within the 'linear' range. The opinions of the authors regarding the estimation of moduli for soils in the field, would be of value.

For the static load tests on single piles, deformations were very small. Your Reporter wonders how (without some imagination) the detail of the

graphs shown in Fig. 2 was obtained, when the settlement between 0.1 and 10 minutes amounted, in each case, to about one scale division on the dial gauge.

The pile spacing in the group tests ($= 2d$) is rather less than might be considered desirable in practice. The 3×3 group of $\frac{1}{4}$ in. diameter piles might be expected to act as a single $1\frac{1}{4}$ in. square pile, at this spacing.

The conclusion that elastic theory "used with discretion. . ." provides a wise comment, which could well refer to many of the theories we apply, sometimes unthinkingly.

The paper by Lee and Herington dealing with stresses beneath granular embankments also compares theory with experiment by the use of a model. While the principles employed in the deformable base are clearly described, it is hoped that the authors may provide photographs or diagrams, during presentation, so that the physical arrangement might be better appreciated. To the practising engineer, the knowledge that the vertical stress beneath an embankment is within about $+5\%$ and -15% of that usually assumed (that is, the weight of the material directly above) will be comforting. Three construction sequences are described but two only appear to be reported in the results section. Values for stage 3 have been omitted from Table II. Values of base shear in Tables I and II appear to differ from those in Figs. 3 and 6.

The conclusions that elastic theory is reasonably applicable when deformations are very small (rigid base) but that deformations have to be considerable before fully plastic behaviour is found, emphasise the importance of the region between these two modes of behaviour, so graphically indicated by a big question mark in an earlier paper by Davis and Poulos (Ref. D3).

Finally, we have the scholarly paper by Davis and Booker to consider. This gives a clear historic account of the development of solutions to plane strain problems of surface bearing capacity, and gives a satisfying rigorous solution in terms of plasticity theory, in a manner more readily comprehensible to the engineer than most papers on plasticity. Your Reporter has neither the specialised knowledge, nor the temerity to offer criticism. He is left with the feeling, however, that Terzaghi, with his fairly rough 'engineering' approach, was remarkably close to the mark!

In conclusion, a note on jargon may not be out of place. We should, your Reporter feels, not continue to use everyday words in a misleading technical sense merely because they have become enshrined in the literature. Are not the terms "sloping" and "friction", as adjectives applied to piles, less liable to mislead the innocent than "battered" and "floating"?

References:

- D1. GIBBS and HOLTZ - Research on Determining the Density of Sands by Spoon Penetration Testing. Proc. Fourth Int. Conf. Soil Mechanics and Foundation Engg., 1957, Vol. 1, p. 35.

D2. SYMPOSIUM - Major Building Foundations in Auckland. Proc. Fifth A.-N.Z. Conf. Soil Mechanics and Foundation Engg., 1967, p. 269.

D3. DAVIS, E.H. and POULOS, H.G. - The Use of Elastic Theory for Settlement Prediction under Three-Dimensional Conditions. Geotechnique, Vol. 18, No. 1, 1968, p. 67.

Paper by S.B. BROMHAM and J.R. STYLES:

Discussion by L.K. WALKER:

The authors' paper raises several interesting questions related to the interpretation of pile load test data. Their Fig. 5 indicates that pile settlements under constant load can be divided into primary and secondary components. The primary component appears to be small in magnitude (if immediate settlement is not included), while the secondary component can be approximated by a semi-log straight line as in the conventional interpretation of consolidation test data. Secondary settlements shown in Fig. 5 are already a significant proportion of the total after only about 200 minutes.

The writer's specific queries are as follows:

- (a) Are the settlement-time data plotted considered to relate to drained or undrained deformation?
- (b) Do settlements for the long increment (19-hour) continue on a semi-log straight line as might be expected from a secondary compression phenomenon?

With respect to the above points the writer considers that pore pressure build-up around a pile under test load will be rapid due to the small zone of influence around the pile. Thus the settlement-time data should be representative of a drained loading increment. This would suggest that the secondary compression region shown in the authors' Fig. 5 should continue for several days. Data from the long-term increments would be instructive on this point.

One of the major concerns in pile load testing is to determine a suitable load increment duration. If a regular 24-hour increment time is used, there is little doubt that a "drained" load-deflection curve would result. The results from Fig. 5 suggest that an "undrained" load-deflection curve would be obtained if something less than half-hour load increments were used. It is assumed that some difference between the two curves would be anticipated, as is observed in laboratory shear tests. Similarly different ultimate loads should be obtained. Which of the two types of test is required would seem to depend on the type of load application to be expected in the field (i.e., the ratio of dead to live load). In general, the "undrained" test would appear to provide a more conservative result for ultimate load, but a lower estimate of likely settlement under load. Some judgement is obviously necessary in each particular case before a test load programme is defined.

The authors' data should lead to a more fundamental appraisal of test load procedures, and any further comment they can offer would be welcomed.

The Authors in Reply:

To the General Reporter:

1. The results of Index and Classification tests are shown in Table VII.

2. The Friction Ratio is defined as:

$$\text{Friction Ratio} = \frac{\text{Skin friction resistance (lb./in.}^2\text{)}}{\text{Cone resistance (lb./in.}^2\text{)}}$$

3. The correction to 'N' values to allow for vertical effective stress achieves predominance where estimates for density are required at relatively shallow depths. These corrections strictly apply to cohesionless soils, and thus for the very stiff clay it was not considered worthwhile to modify results which have doubtful application. Corrections to the S.P.T. values in the medium sand layer were not required because of the high value of effective overburden pressure.

4. Highly polished, lubricated brass end platens were used in all triaxial testing.

5. One of the factors which determines the shape of the secondary compression portion of the log. time versus deflection curve is the magnitude of the load increment ratio. Fig. 5 shows the deflection curve for the loading from 160,000 lb. to 180,000 lb. i.e. an effective load increment ratio of 1/8. The typical deflection curves associated with the Laboratory Consolidation Test do exhibit the reverse curvature mentioned by the General Reporter, but the load increment ratio is generally unity and the system is clearly one-dimensional in nature.

6. An error has occurred in Table V : the pile capacity from Loading Tests on Test No. 2 should be >300 tons.

7. Isolated direct shear tests, where shear was indicated on (a) a steel/soil interface, and (b) a soil/soil interface, indicated a c_a/c_u ratio of approximately 0.24. Whilst not conclusive it did reinforce the assumption of adopting the average value of $c_a/c_u = 0.25$ for very stiff clays.

To Mr. L.K. Walker:

1. The authors generally agree that the loading durations of 30 minutes or less probably represent "undrained" load-deflection states. The observed differences between the load-deflection curves for

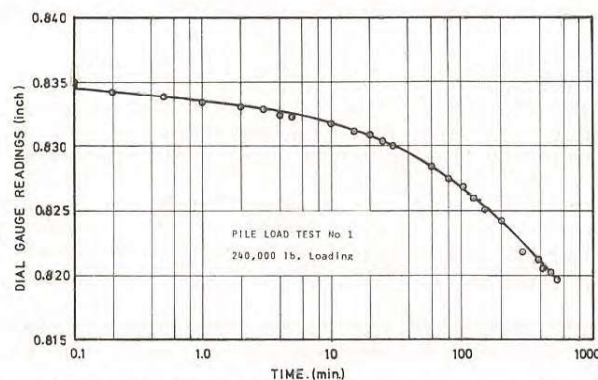


Fig. D1 - Deflection - Log. Time Curve for 19-hr. Load.

TABLE VII

RESULTS OF INDEX & CLASSIFICATION TESTS

Sample No.	Depth	LL %	PL %	PI %	% Sand	% Silt	% Clay	Classification
919092	4'6" - 6'	56	15	41	4	52	44	Silty Clay
093	9'6" - 11'							
094	14'6" - 15'8"	79	21	58	5	36	59	Clay
095	16' - 17'6"	69	14	55				
096	17'6" - 19'	76	15	61				
097	19' - 20'5"	74	20	54	7	36	57	Clay
098	20'6" - 22'	72	16	56				
099	22' - 23'4"	78	18	60				
100	23'6" - 24'11"	63	17	46	9	37	54	Clay
101	25' - 26'4"	71	18	53				
102	26'6" - 27'10"							
103	28' - 29'5"	65	17	48	4	41	55	Clay
104	29'6" - 30'10"	63	15	48				
105	40' - 41'1"	48	14	34	11	51	38	Silty Clay
106	50' - 50'10"	62	18	44	3	49	48	Silty Clay

the short and longer term loadings were small and indicate that for this type of material rapid loading techniques produce completely satisfactory results.

2. Fig. D1 shows complete deflection - log. time curve for the 240,000 lb. loading in test No. 1, which continued for 19 hours. The settlements do appear to conform to a pattern which is typical of secondary compressions.

Paper by N.S. MATTES and H.G. POULOS:

The Authors in Reply:

The General Reporter questions the accuracy of the small deformations measured in the pile tests. The authors feel that much of the possible inaccuracy arising from the use of dial gauges has been eliminated by the use of very rigid mountings, as shown in Fig. 1 of the paper. In addition, a dial gauge with a large face was used so that interpolation to a tenth of a division (i.e. 10^{-5} in.) could be carried out without difficulty.

The question of estimating the soil modulus for full-scale piles is very relevant. Attempts to measure modulus from ordinary laboratory tests do not give acceptable values, as found by Bromham and Styles in their paper. Apart from back-figuring the modulus from a pile load test, the authors consider that the best way of estimating modulus is to use empirical relationships based on previous published load test results. Such a correlation for piles in clay is shown in Fig. D2, where E_s is related to undrained cohesion c_u for both bored and driven piles. The effect of pile installation on E_s can be seen from this figure. It is also interesting to note that the data of Bromham and Styles fits the correlation for driven piles very well.

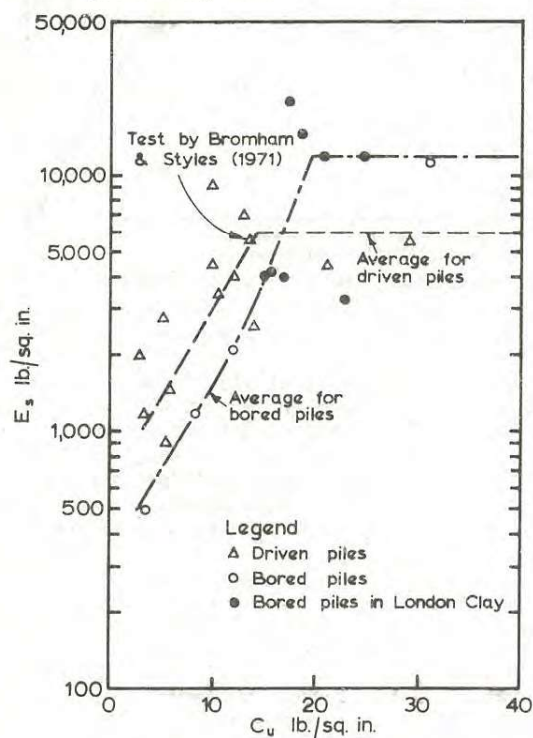


Fig. D2 - Backfigured Soil Modulus E_s for Piles in Clay.

Paper by H.G. POULOS and M.R. MADHAV:

The Authors in Reply:

In reply to the General Reporter regarding how "massive" a pile cap must be to be a fixed-head cap, there appears to be little data available on this question. Davisson (1970) suggests however that in practice, the degree of fixity that can be developed (i.e. the maximum moment in the cap) is about half the value corresponding to a truly fixed-head pile. If this is so, the assumption of a fixed-head pile would rarely be justified, and the assumption of a rigid pile cap which can rotate would be more realistic.

Reference:

DAVISSON, M.T. - Lateral Load Capacity of Piles. Highway Research Record No. 333, 1970, pp. 104-112.

Paper by R.J. MCKENZIE:

Discussion by E. GERMANIS:

The tests were obviously not designed for a full assessment of creep effects on footings under sustained long-term loads at their maximum working values. However, it would be of interest to know whether any change in the creep rates was, or could be, observed during the short time intervals of constant load, when the incremental loading method was used on slab footings.

A change of moisture content in the in-situ clay surrounding the piles and the footing excavations should have an effect on the test results for the adopted loading rates and on the uplift resistance generally. An artificial change of the in-situ moisture content would be desirable to study its effect. It is understood that it is intended that variations in this respect be estimated on the basis of laboratory test results.

For the "cone of earth" method some authors assume a variable angle (5° to 30°) between the vertical and the failure plane. The appropriate angle is to be determined by a test for each type of soil separately.

The advantage of the use of crushed rock in the backfill over slab footings has been clearly proved by the tests described in the paper.

The Author in Reply:

To the General Reporter:

In reply to the General Reporter's comment on introducing a safety factor in design in order to provide additional reserve in the uplift capacity of under-reamed piles under long-term loading, the author stated that the information on which to base such an increase is inadequate. For example, the creep effect under sustained loading is known only

for the short ten-minute periods when the load is held constant during the CIGRE method of test loading. This data cannot be applied to the long-term loading condition which is important in footings for strain towers. Other parameters to be considered in the choice of a load or safety factor would include the large differences in magnitude between the sustained conductor loads and the higher, but infrequent, wind gust loads. In addition, the variability of both the in-situ site conditions at each prototype tower and the footing as constructed must also be considered. As a consequence, footing design is based directly on the 90% value of the uplift capacities determined in the test series and no other factor is applied.

To Mr. E. Germanis:

In reply the author comments that changes in moisture content of the clay generally occurred in the top two or three feet, which results in cracking of the clay in dry periods, whereas the major part of the footing is located below the seasonal zone and moisture conditions, cohesion and tensile strengths of the clay remain virtually constant. A correction is made in the design for piles of shallow depth where the effect of seasonal cracking is significant by the reduction of the pile capacity by an amount equal to the capacity of a similar pile of depth equal to that of the seasonal effect.

Uplift displacements of the test footings due to creep during the 10-minute periods of constant load were measured, and it was noted that the creep rate was generally constant and increased in magnitude at the higher loading levels. In some cases, it was noted that the creep rate reduced during sustained load.

Paper by I.K. LEE and J.R. HERINGTON:

Discussion by Prof. D.H. TROLLOPE:

The results obtained by the authors are of considerable interest and value in that they provide through the medium of carefully conducted experiments an opportunity to compare differing theoretical predictions.

It is perhaps unfortunate that the authors err on p. 293 in quoting the maximum shear stress under no-arching conditions as $0.58 \gamma H$. This is, in fact, the full-arching value and the corresponding no-arching value is $0.15 \gamma H$. Earlier recognition of this might have avoided the unjustified criticism of the appropriate clastic solutions which are contained in the paper.

Fundamentally, clastic mechanics in its simplest two-dimensional form requires recognition of two parameters; the distribution angle (θ) and the arching factor (k).

The distribution angle θ can be calculated from "at rest" type earth pressure tests and is analogous to the Poisson ratio term in conventional elastic theory. Taking the simple four-unit systone (Trollope, 1969) it can be shown that

$$\frac{\sigma_3}{\sigma_1} = \tan^2 \theta$$

Using the authors' quoted value of Poisson's ratio as 0.4 (p. 295) the value of θ is calculated to be $32^\circ 18'$.

If the value of θ is known then the constitutive stress relationship depends only on the arching factor (k).

It is possible therefore to calculate appropriate values of k from the experimental results quoted in the paper.

Consider first the wedge sequence. Fig. D3 shows the calculated values of k and the associated linear stress distributions fitted to the maximum shear stresses in Fig. 3 of the paper. Perhaps the most encouraging feature of this analysis is that the movement of the peak shear stress away from the centre-line with increasing side slope as predicted by the clastic theory corresponds with the trend of experimental results. The fact that the calculated k values exceed 1 can be interpreted to mean either that the material can take tension or that some boundary restraint is present. In the present circumstances the latter explanation is preferred. Frictional restraint along the base, perhaps assisted by some wedging between the independent measuring supports could account for this. The appropriate theoretical elastic shear stress distribution is also plotted from data given in Table I of the paper.

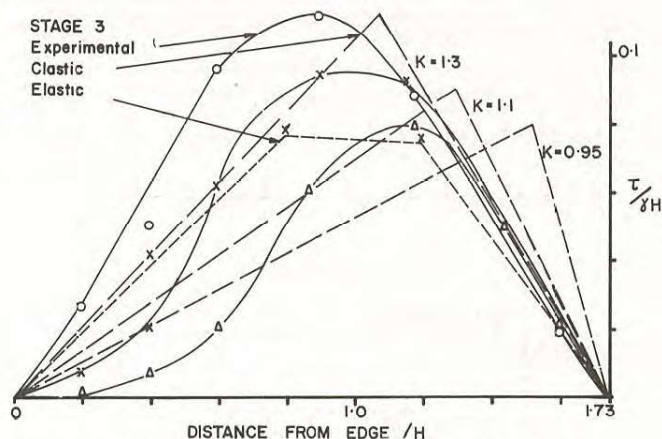


Fig. D3 - Comparison between Clastic, Elastic and Experimental Results - Shear Stress Distribution - Wedge Sequence.

Of even greater practical significance however are the results shown in Fig. D4. It will be seen that with only 0.1 in. central deflection upwards the measured shear stresses have altered to provide remarkable agreement with the no-arching solution. This suggests that the effect of boundary restraint can be nullified by trivial boundary displacements.

Analysis of the layered sequence also provides support for clastic theory. It has been pointed out previously (Trollope and Morgan, 1959) that introduction of the wide embankment profile introduces restrictions on the possible distributions of the arching factor k . The concept of zones of arching developed in this earlier paper has been adapted to the present situation. The results of the approximate k distributions are shown in Fig. D5 and D6. (It should be noted that when discontinuities in the k

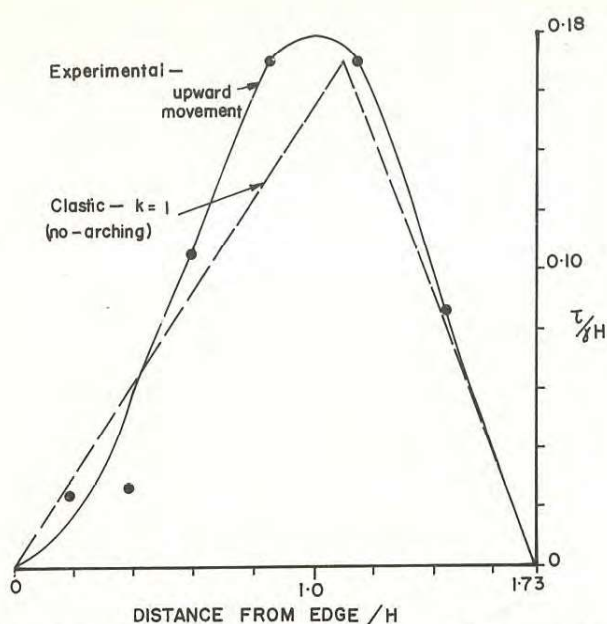


Fig. D4 - Shear Stress Distribution - with upwards movement of base.

zones are introduced, discontinuities are also implied in the stress distributions at the zone boundaries. The present results have been calculated on the assumption that the value of k in the left-hand zone operates on its right-hand boundary).

The only experimental points that do not fit well are those measured at a distance $0.4 H$ from the outer edge. Evidence in Fig. 6 of the paper suggests that these readings were consistently low and this might account for the disparity.

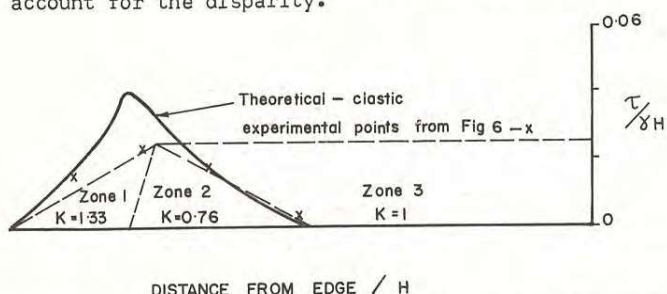


Fig. D5 - Comparison between Clastic and Experimental Results - Shear Stress Distribution, Layered Sequence Stage 1.

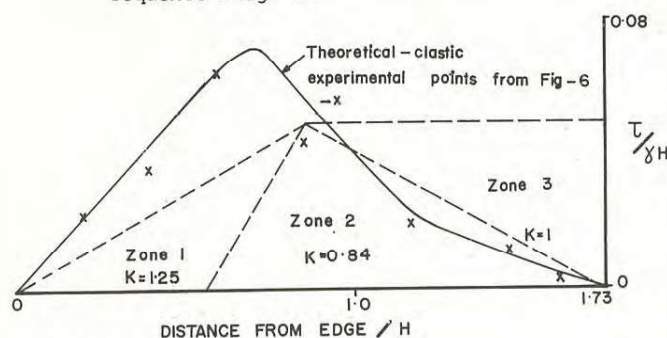


Fig. D6 - Comparison between Clastic and Experimental Results - Shear Stress Distribution, Layered Sequence Stage 2.

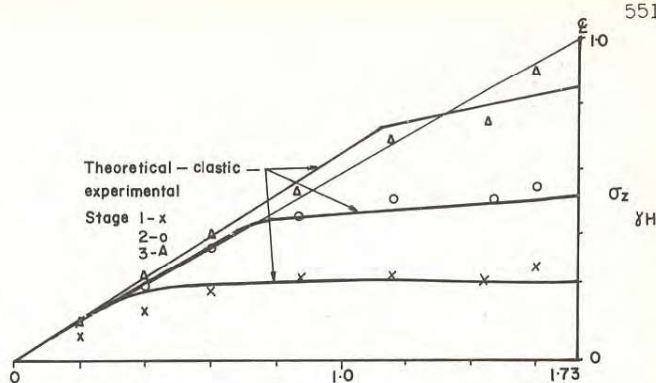


Fig. D7 - Comparison between Clastic and Experimental Results - Normal Stress Distribution, Layered Sequence.

To check further the predictions of clastic theory, the normal stress distribution derived from the k values produced in Figs. D5 and D6 were calculated and are shown in Fig. D7. Again, with the exception of the values at $0.4 H$ from the outer edge agreement with the measured stress is good.

The above analysis demonstrates the applicability of clastic theory to the behaviour of granular materials and particularly emphasises the versatility of the method in dealing with a wide range of geometrical and boundary restraint problems.

References:

- TROLLOPE, D.H. - The Mechanics of Discontinua or Clastic Mechanics in Rock Problems. Chapter 9 in Rock Mechanics in Engineering Practice, (Stagg and Zienkiewicz, Eds.). New York, Wiley, 1969.
- TROLLOPE, D.H. and MORGAN, J.R. - Stress Systems within Simple Slopes of Granular Materials. Civil Engg. Trans. I.E.Aust., Vol. CE1, No. 1, March, 1959, pp. 18-26.

Discussion by B.C. BURMAN:

The granular wedge under self weight loading is a configuration of fundamental interest in the field of practical geomechanics, representing in simplified form such engineering structures as earth dams, road embankments and mine waste tips. The authors have carried out probably the most carefully conducted set of granular wedge experiments yet reported and their results are a significant contribution to this particular field. However, physical models, per se, can deal only with one aspect of the whole problem and it is desirable to have a theoretical framework into which the results of physical experiments may be meshed. The purpose of this discussion is to introduce briefly a numerical model which is believed to provide a viable theoretical basis for such studies.

As part of a study of the mechanics of discontinua the writer has developed a numerical procedure, based on finite element techniques, which allows the modelling of block jointed systems (Burman, 1971). In essence, the behaviour of a jointed mass is described in terms of the translational and rotational displacements of individual block centroids as well as the contact forces between

adjacent blocks. Full account has been taken of the non-linear interaction between blocks with shear behaviour being defined by a general stress-strain relationship which may include work-softening and work-hardening as well as non-linear failure envelopes and dilation. The normal interaction between blocks allows for limited tensile strength of the joints in addition to locking in both compressional and rotational modes.

The granular wedge situation has been modelled numerically by a close-packed hexagonal arrangement of disc units consisting of 15 layers and with 30° side slopes. Vertical symmetry permits the modelling of only one-half of the wedge and boundary conditions restricting all but vertical displacements along the centreline and all three displacement components along the base. The shear stiffness of joints has been taken as a low value to represent shearless contacts and the tensile strength between discs taken as zero. The three stress components, σ_z , σ_x and τ_{xz} , throughout the jointed mass have been calculated as the appropriate average force per unit area in a manner similar to that described by Trollope (1969).

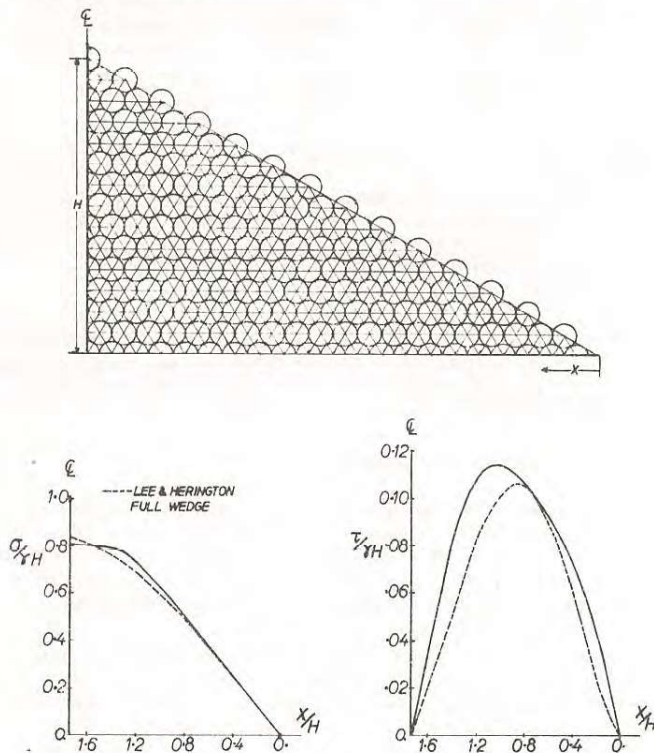


Fig. D8.- Comparison of Normal and Shear Stresses on Base of Numerical Wedge Model and Experimental Results for Sand Wedge (after Lee and Herington, 1971) for Full-Height 30° Wedge Condition with Rigid Base.

For these conditions, the results of three numerical experiments representing self weight loading of the full 30° wedge and close approximations to stage 2 of the authors' wedge and layered sequences are presented in Figs. D8, D9 and D10. The actual configuration of disc units for each analysis have been shown together with a comparison of normal and

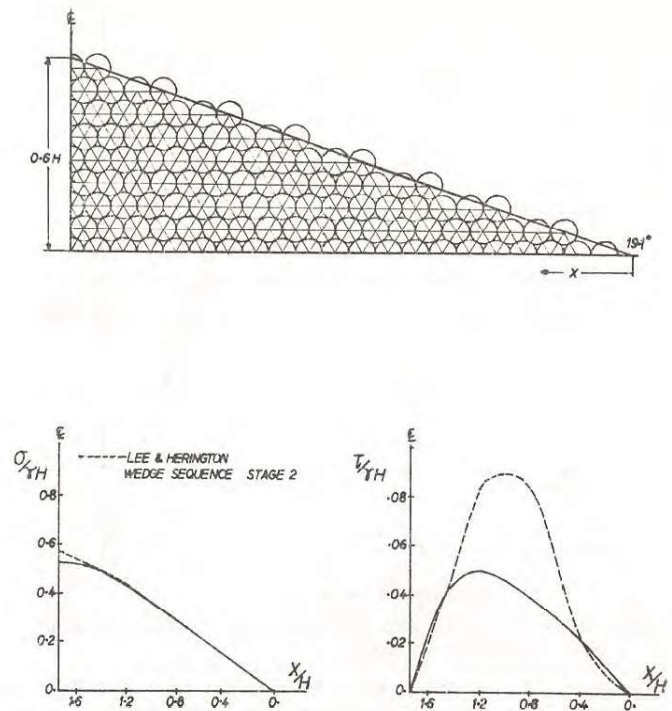


Fig. D9.- Comparison of Normal and Shear Stresses on Base of Numerical Wedge Model and Experimental Results for Sand Wedge (after Lee and Herington, 1971) for Stage 2 Wedge Sequence with Rigid Base.

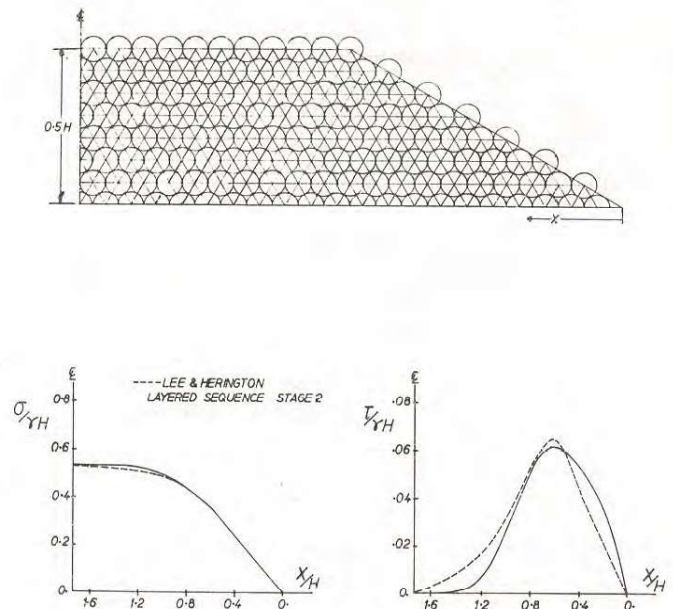


Fig. D10.- Comparison of Normal and Shear Stresses on Base of Numerical Model and Experimental Results for Sand Wedge (after Lee and Herington, 1971) for Stage 2 Layered Sequence with Rigid Base.

shear stresses along the base. It will be seen that there is excellent agreement between predicted and measured stress values in all three cases except for shear stresses in the stage 2 wedge sequence where the numerical model values are approximately 60% of those measured. This latter feature has led the writer to question the accuracy of measured stress components and although definitive conclusions cannot be reached in this regard some doubts remain. In particular it has been possible to verify that the vertical equilibrium requirement was satisfied generally throughout the wedge and in particular along the base for the numerical solutions, viz:

$$\frac{\partial \sigma_z}{\partial z} + \frac{\partial \tau_{xz}}{\partial x} + \gamma = 0$$

The fact that there is good agreement between measured and numerical values of the normal stress component along the base but some divergence between respective shear stress values raises doubts in the writer's mind as to accuracy of measured shear stresses. Although the authors will be unable to check the vertical equilibrium requirement in differential form they may obtain some indication of the reasonableness of measured stresses by considering the vertical equilibrium of free body sections. In this regard, it may be of interest to note that the numerical results reveal a marked degree of regularity in stress distributions along horizontal sections indicating that the differences between finite and infinite wedge configurations are slight.

For the deformed base condition, the numerical model shows a progressive transfer of normal stress from the central regions similar to that reported by the authors. However, the decrease in shear stresses reported by the authors for the concave base profile is in direct contrast to numerical predictions which indicate an increase from $0.12\gamma H$ to $0.22\gamma H$ for a central deflection of $0.02H$. In this regard there is at least qualitative agreement between the writer's finite wedge model and plasticity solutions for the infinite wedge reported by Booker (1969). In view of this correspondence it would be most enlightening to have the authors detail their basis for attributing the anomalous shear behaviour to the finite dimensions of their wedge models.

In conclusion it is most encouraging to note the authors' intention to extend this work to a wider range of base deformation states. Their demonstration of the inability of linear elastic theory to deal with stress distribution in granular materials is a point of immediate practical interest and experimentation along these lines is undoubtedly of considerable practical importance. In this respect, it is suggested that consideration be given to the measurement of additional stress or strain components, preferably horizontal stresses, so that the results may be even more soundly based than those here reported.

References:

- BURMAN, B.C. - Ph.D. Thesis, James Cook University of North Queensland, 1971.
- TROLLOPE, D.H. - The Mechanics of Discontinua or Elastic Mechanics in Rock Problems. Chapter 9 in Rock Mechanics in Engineering Practice, (Stagg and Zienkiewicz, Eds). New York, Wiley, 1969.

BOOKER, J.R. - "Application of Theories of Plasticity to Cohesive Frictional Soils", Ph.D. Thesis, School of Civil Engg., University of Sydney, 1969.

The Authors in Reply:

To the General Reporter:

The authors would like to thank the General Reporter and those who contributed to the discussion of the paper. There were many aspects which could not be detailed due to space limitations and some amplification is necessary to answer certain of the points raised in discussion.

As requested by the General Reporter, photographs of the model are now included. These are shown as Figs. D11 and D12 and show the general arrangement of the model and some details of the measuring strips. Referring to the apparent discrepancy between the shear stress values of the Figs. 3 and 6 and the Tables I and II, it should be made clear that the values of shear stress listed in the tables were divided by γh where h was the centric height of the embankment at any stage, whereas the values plotted in the figures were divided by γH , H being the final centric height.

Professor Taylor's comments that practising engineers may be comforted with the knowledge that the vertical stress beneath an embankment is within 15% of the weight of material directly above has to be qualified, since this only applies to the rigid base situation. There is a progressive decrease in centric vertical stress beneath the embankment as the foundation settlement develops. Earlier work referred to in the paper suggested that this vertical stress could diminish to a value much less than the minimum value of $0.85\gamma H$ shown in the paper since this latter value corresponded to a very small differential settlement.

A correction to the paper was that the value of the peak shear stress for the no-arching case should read $0.15\gamma H$ (p. 293). It was shown that the measured peak shear stress was in fact considerably less than the value predicted by the no-arching "solution", but, more importantly, that the distribution of shear stress was incompatible with this "solution" and, as stated in the paper, "the solution is inapplicable to the rigid base condition when there is no horizontal displacement of the foundation".

In modelling this problem particular attention was paid to the measurement of shear stresses along the base of the embankment as this provides data which is a very critical test of the applicability of any theoretical solution. The shear stress distributions published in the paper provide, to the authors' knowledge, the first opportunity to make such a critical comparison. Earlier work which concentrated on measurement of normal stress did not provide sufficient critical data since the normal stress distribution is rather insensitive to the analytical approach adopted.

To Prof. D.H. Trollope:

In contrast to Trollope's static approach, which requires a pre-existing knowledge of the arching factor(s) for every new situation, the authors have attempted to obtain complete solutions, that is, solutions which satisfy statics and strain compatibility. There is little point in attempting to critically evaluate Trollope's discussion any further, and the reader is referred to the discussion by Davis and Taylor (1962) if he wishes to pursue the limitations of the arching theory as this discussion forms a succinct and relevant statement.

It is well known that the prime difficulty in analysing the behaviour of a soil mass is that the constitutive equations are complex, and therefore any simple mathematical model is necessarily an approximation to the real behaviour. At the present time the authors are examining various models by comparing predicted with measured values. The solutions for the linear elastic model were quoted in the paper to show that even this simplest (and obviously deficient) model led to interesting and revealing predictions. It was evident that the progressive build-up of the shear stress distribution was consistently predicted and even the quantitative correspondence exceeded expectations. This appears to justify the use of this model in a finite element analysis of the soil structure studied, and suggests that better agreement could be achieved by some relatively slight modifications to the model.

As an example of this, mention can be made of a special element based on the joint element developed by Goodman. From the results of direct shear test it is possible to obtain data for the normal and tangential stiffness of the elements as well as the residual tangential stiffness. The joint stiffness matrix can be developed and the joint stresses determined from the calculated displacements. The joint element cohesion, friction, and residual tangential stiffness are read in as data and the shear stress is calculated from the individual normal stress on each joint element. A Mohr-Coulomb criterion is applied if the joint shear stress exceeds the shear strength, that is, K_s is set equal to the K_s (residual) and the problem is recycled. Preliminary results on the embankment show that the normal stress distribution is accurately predicted but, as stated above, this is to be expected due to the insensitivity of the normal stresses to the model used in the analysis. The shear stress is closely predicted within the central half of the embankment but there are deficiencies towards the toe. This model is being further improved by taking into account

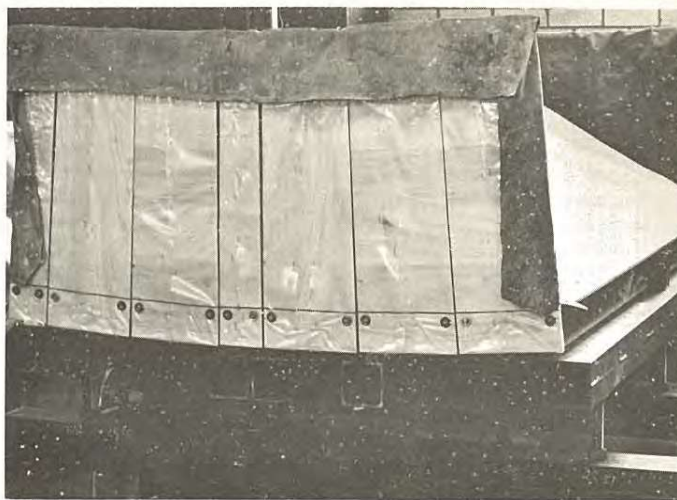


Fig. D11.

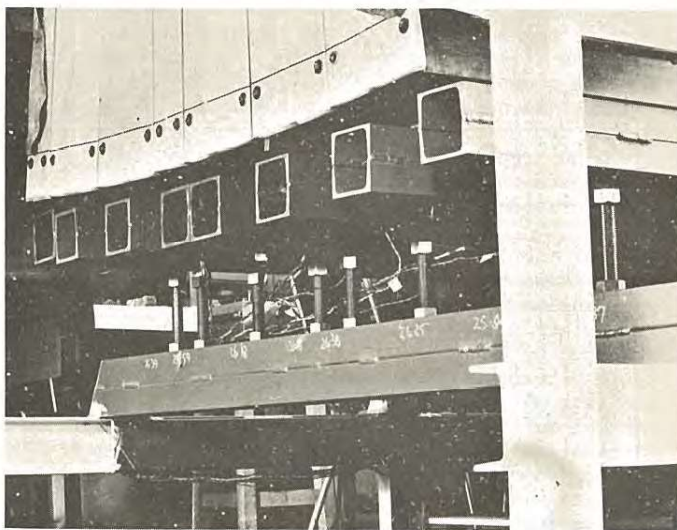


Fig. D12.

variations in models and Poisson's ratio with principal stress ratio.

To Mr. B.C. Burman:

Finally, it would appear that little comment need be made of Burman's discussion as his approach and philosophy appear to be identical with that of the authors.