Foundations of Structures

Fondations de Constructions

a General Subjects and Foundations other than Piled Foundations—Sujets Généraux et Fondations autres que Fondations sur Pieux

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The Chairman

The fourth session of the fourth conference is called to order. This session deals with the foundations of structures other than piled foundations, and it will continue throughout this afternoon. Our Vice-Chairman for this morning's session is M. Toyama of Japan, and our General Reporter is J. Brinch Hansen of Denmark upon whom I shall now call to make his opening remarks.

General Reporter

Of the 44 papers in the present Division, 10 deal with bearing capacity, 8 with stress distribution, 11 with settlements and the remaining 15 with special problems such as swelling clays, collapsible soils, effects of vibration, sand drains and construction methods. The contributions are rather evenly divided between theories, tests and case records.

It is evident that actual soil conditions are often so heterogeneous that no theory can apply, and that we must rely upon experience and judgment; but in doing so, it will be a great help if, at least, we know the correct solutions for a few clear-cut cases, such as cohesionless sand and homogeneous clay. Our main problems are, of course, the prediction of bearing capacity and of settlements, but at present a rather peculiar situation exists in both of these fields.

We have a theory concerning bearing capacity, the results of which agree rather well with model tests and field experience as long as we consider saturated clay and loose sand. For dense sand, however, the actual bearing capacity may be up to ten times the theoretical value. A deviation of this magnitude can hardly be due to faulty theory alone, but may have some connection with our interpretation of test results. However, no satisfactory explanation has been given as yet.

We have also a method of calculating settlements on the basis of oedometer tests, which seems to agree comparatively well with measured settlements, at least for normally consolidated and lightly over-consolidated clays. For heavily over-consolidated clays, however, the actual settlements may amount to as little as 1/10 of the calculated value. The explanation may partly be found in sample disturbance, but may also be derived from the fact that the pore pressures set up under actual foundations where the soil can, to a certain extent, expand laterally will not be the same as in the oedometer. Here is a wide field for new theories and calculation methods.

The two problems of bearing capacities and settlements are evidently the most important ones in this Division, and in view of the considerable deviations between the existing theories and practice it seems a pressing necessity to modify these theories in order to obtain better agreement: I hope that there will be a good deal of discussion on these points.

As a further subject for discussion I have proposed the use of some simple formulae for bearing capacity, taking into account all relevant factors, such as the size, shape and depth of the foundation area, as well as the inclination and eccentricity of the foundation load. In view of the shortcomings of present theories the proposed formulae can hardly be more than semi-empirical for the time being, but even this would represent an enormous improvement upon the rules still prescribed in most building codes.

Another point on which I should welcome discussion concerns the way in which safety factors are introduced in our designs. At present this is done in a rather haphazard way. We apply a safety factor to bearing capacity and another to passive earth pressure, but not one to active earth pressure. We have also a safety factor on sliding and another on overturning, just to mention a few examples. In stability problems the safety factor is usually applied to the shear strength of the soil, and this is a sound principle which in my opinion should be extended to earth pressure and bearing capacity also. In order to obtain a general and rational system, the safety factors on the soil constants should actually be used in combination with other safety factors on some of the loads and on the strengths of the building materials.

In my opinion our main interest and efforts should be centred on the problems I have mentioned here, but there will be, of course, many other topics in the wide field of the present Division worthy of discussion.

V. Mencl (Czechoslovakia)

I should like to discuss Paper 3a/22 by N. N. Maslov. What is the main idea behind his work? If we have a saturated sandy soil and subject it to vibrations or shocks the sand becomes compacted. The decrease in the void ratio is associated with an excess hydrostatic pressure in the pore water. The process can be easily demonstrated, for instance, in the cylinder shown in Fig. 1. It is self-evident that the excess pressure is higher in a loose sand and decreases with increasing permeability. The author has developed elaborate mathematical expressions for the value of this excess pore pressure.

It is also stated that this pore pressure is to be taken into account when computing the stability of dams exposed to shocks or vibrations. In this way the influence of a change in the void ratio during shear, and also the idea of critical void ratio, are excluded from the problem. I think that the excess pore pressure determined in such a way cannot be used in the computation of stability, and that we must always take into account the pore pressure which develops during the process of shear. Therefore we constructed a similar cylinder but at the bottom we put a rotating shear disc. In the piezometric tube near the bottom we obtained the pore pressures shown in Fig. 2. In a dense sand we obtained an excess pore pressure during vibration, but when we rotated the disc this pressure decreased and we obtained suction. In a loose sand we obtained an excess pore pressure during vibration which increased when the

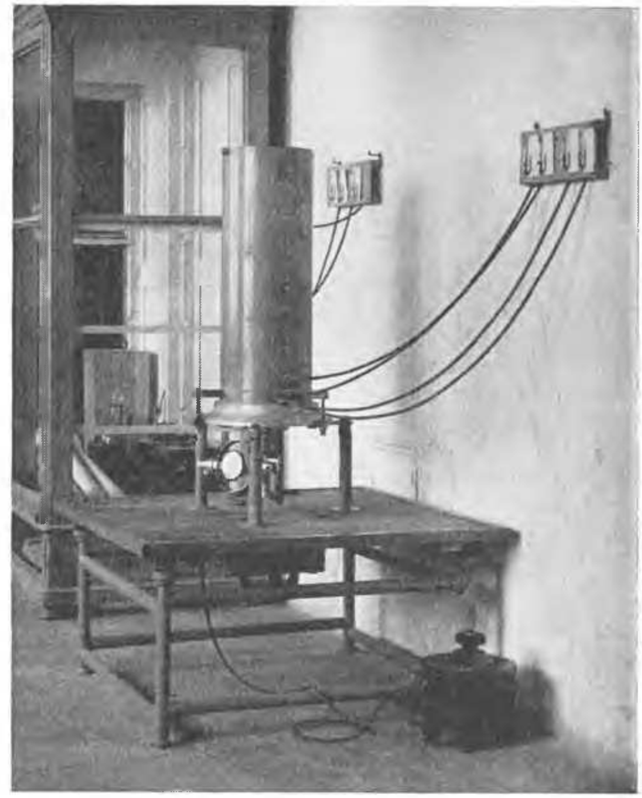

Fig. 1

disc was rotated. Therefore I think that we must always introduce into the solution the excess pressures which develop during vibration plus shear, and that the idea of a critical void ratio should not be discarded.

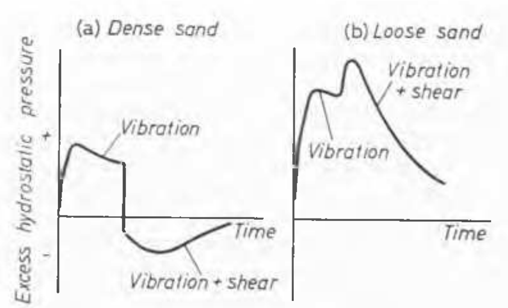

Fig. 2

D. Lazarević (Yugoslavia)

Monsieur le président, mesdames, messieurs, je voudrais donner quelques notes sur le calcul des fondations directes.

Le calcul des semelles dans l'hypothèse d'un sol élastique semi-infini, isotrope et homogène est une approximation brute. Sauf le cas de constructions en acier, pour lesquelles l'hypothèse d'élasticité complète n'entraine aucun reproche sérieux, elle est, quoique inexacte, toujours la base du calcul dans tous les cas de construction en pierre, en béton et en briques. Pour obtenir des résultats satisfaisant les bases du calcul statique, il faut perfectionner la méthode d'évaluation précise des modules de déformation du sol avec une exactitude au moins pareille à celle des matériaux mentionnés.

Dans cette méthode de calcul des semelles sur l'espace élastique semi-infini, il arrive très souvent que les lignes des charges réactives ont des points de singularité, surtout en fin de semelles. Pour obtenir la distribution véritable il faut aussi connaitre les caractéristiques de plasticité du sol. Le problème de similitude parmi l'état de déformation des échantillons et des sols sur place, sous la fondation, est un problème d'exploration urgent.

L'influence de la plasticité et des quelques autres qualités du caractère rhéologique que possèdent presque tous les sols naturels nous donne l'idée d'un étouffement des valeurs extrêmes des charges réactives, spécialement sensibles dans les domaines des fins de semelles. La variation du module d'élasticité des sols enfonction de l'intensité des tensions nous conduit au terme: 'module déformation' et à un étouffement nouveau des valeurs prononcées des charges réactives dans les domaines intermédiaires entre les fins des semelles.

Quelques auteurs ont résolu un grand nombre de problèmes, qui, quoique idéalisés, n'en sont pas moins intéressants pour une meilleure connaissance de la nature du comportement des semelles étudiées sur l'espace élastique semi-infini. Il existe encore un plus grand nombre de solutions obtenues dans l'hypothèse du comportement des semelles comme les corps élastiques qui plongent dans le sol qui n'est que pseudo-liquide, de densité égale au coêfficient de réaction du sol. Il serait très intéressant de contrôler le comportement des semelles en échelle naturelle, sur des sols aux caractéristiques suffisamment préciées au point de vue de la distribution des charges réactives sous la semelle, et de la distribution le long de la semelle des moments fléchissant conséquent.

Tous les résultats du calcul des semelles sur l'espace élastique semi-infini sont obtenus au moyen de suppositions simplifiantes. Une de ces suppositions, de la plusgrande importance au point de vue de l'influence sur les résultats statiques, est la conception de la rigidité de l'ossature du bâtiment. Il nous semble légitime de chercher les solutions des problèmes hyperstatiques des éléments sol-semelle-ossature sans les séparer les uns des autres. Seulement, par l'étude de l'intégrité de la fonction statique de cet ensemble on peut approcher l'ingénieur des résultats désirables.

Dans le cas de bâtiments à plusieurs étages il faut les traiter, dans la mesure du possible, comme des constructions spacieuses. On doit déterminer la rigidité de l'ossature en tenant compte, non seulement des éléments du système principal mais aussi des éléments secondaires. Les destructions faites au cours de la guerre nous ont montré que les rigidités calculées par la manière habituelle sont des valeurs très petites en comparaison de celles que possède la construction réelle d'une conception spacieuse. Cette rigidité supplémentaire présente un grand intérêt pour la détermination de la flexibilité du système complexe 'semelle-ossature'. La déformabilité du sol et la flexibilité du système conçu comme système spacieux nous donneront toujours la distribution des charges réactives tout-à-fait différente si on la compare à celle calculée de la façon habituelle. Nous croyons que ce phénomène est facile à constater dans les cas des mesures recommandées ci-avant.

A Belgrade quelques bâtiments atteignent des hauteurs allant jusqu'à 55 m sur fondations directes. Ils nous ont donné des résultats excellents au point de vue du comportement des fondations comme en ce qui concerne le comportement des constructions elles-mêmes.

Dans un des bâtiments une bombe à détruit complètement un poteau supportant une charge de l'ordre de 260 tonnes. Aucune fissure n'a été constatée aux éléments de l'ossature en béton armé ni aux dalles à nervures. Quand c'était possible nous avons toujours employé comme fondations les blocs solitaires. Ils ont un petit nombre de paraètres qui doivent être définis avec précision pour que l'ingénieur puisse les utiliser correctement quand il étudie la construction de l'objet.

Pendant l'étude d'un objet à construire il faut recommander l'exercice de notre intuition d'explorateur et d'auteur, mais nous ne sommes pas encore suffisamment habitué à cette exigence.

B. O. PRAMBORG (Sweden)

In Paper 3a/35 K. H. ROSCOE presents an investigation on foundations subjected to overturning moments. He suggests that the earth pressure distribution should preferably be assumed to have the shape given in his Fig. 8.

In fact the suggested distribution has already been used in order to calculate the stability of block foundations in transmission lines which often are subjected to overturning loads. In the *Amer. Inst. elect. Engrs.* Paper 57–153 of 2 October, 1956, by ZETTERHOLM and PRAMBORG of the Swedish State Power Board, formulae are given to calculate such foundations and the expressions for the side-bearing capacity are based on the simplified pressure distribution that Roscoe has suggested. The formulae have been checked with full-scale tests and good agreement has been obtained between theoretical calculations and test results. However, these tests on block foundations have given as a result that considerable movements of the foundation are essential and the angle of tilt must be about 1/100 to obtain the maximum side bearing capacity of the foundation. If the superstructure cannot take up such movements the suggested pressure distribution is not realistic and the dimensions of the foundation have to be increased.

T-K. TAN (China)

The General Reporter has correctly pointed out that the settlement in two- or three-dimensional cases is due to instantaneous deformation, consolidation and secondary time effect. He states that the rate of settlement in practical cases is greater than the calculated rate; but I cannot support his view that this greater rate of settlement will probably be due to two- or three-dimensional consolidation. Consolidation is caused by hydrostatic stresses, and these hydrostatic stresses in more dimensional cases are always less than those in the one-dimensional case under the same draining conditions. I am of the opinion that this phenomenon might more appropriately be ascribed to the flow or secondary time effect of the clay skeleton under the influence of deviatoric stresses. From experiments I have calculated that the viscosity of engineering clays may vary from 10^{13} to 10^{15} poises, and in medium stiff layers it is of the order of 10^{14} poises. The influence of the secondary time effect therefore is not a negligible quantity.

According to my theory (TAN, 1953, 1954, 1957a) the settlement is caused by two main factors: the settlement due to deviatoric stresses (instantaneous deformation and continuous shear flow) and, secondly, settlement due to consolidation and to decelerating volume flow. I have derived mathematical expressions for the settlements and they are being tabulated now and will soon be published (TAN, 1958).

The weakness of all mathematical theories is that generally speaking the theory is too complicated and, secondly, the physical constants cannot be measured. Therefore I have developed two types of apparatus to measure the visco-elastic quantities.

In Fig. 3 you see the compression plastometer. Two types of experiments are possible. The first is with constant load for which deformation can be measured as a function of time and, secondly, the relaxation of stress can be measured as a function of time. In this manner it is possible to measure the coefficient of shear viscosity.

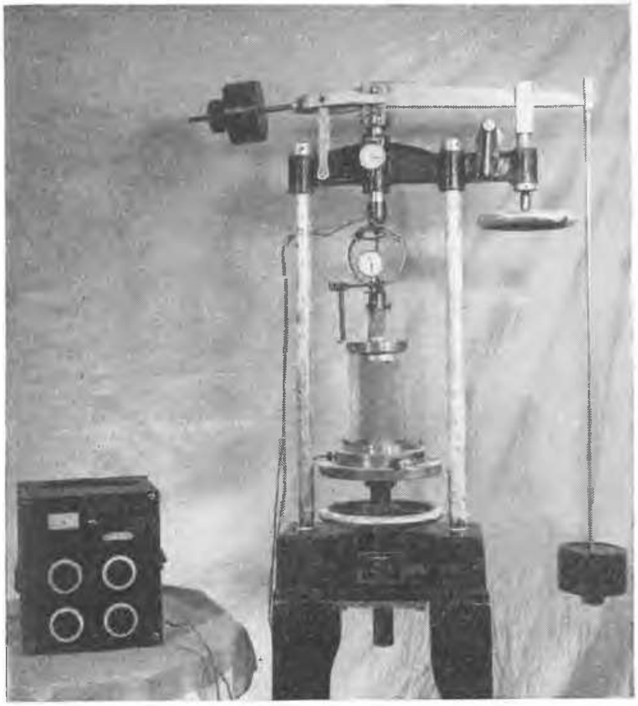

Fig. 3 Compression plastometer for the measurement of rheological properties of soil

Plastomètre à compression pour mesurer les propriétés rhéologiques des sols

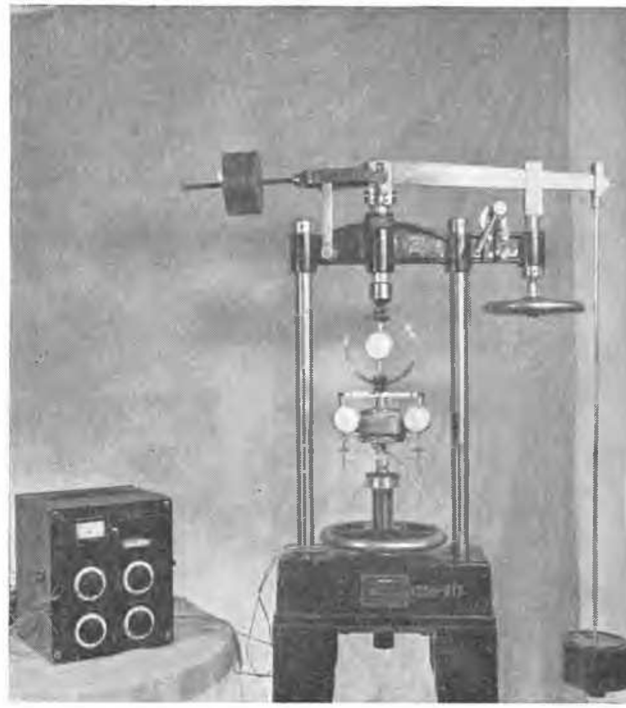

Fig. 4 New Type of oedometer provided with gauges for measurements of the lateral pressure and stress-relaxation as a function of time

Nouveau modèle d'oeudomètre avec instruments pour mesurer la pression latérale et la réduction des efforts en fonction du temps

The second type of apparatus, Fig. 4, is a new oedometer in which the lateral stresses can be measured as a function of time. It is also possible to measure the coefficient of consolidation as well as the elastic part $\nu_{(t)}$ of the coefficient of lateral expansion which is also a function of time.

I should now like to draw your attention to the interesting paper by J. MANDEL (3a/21) concerning the consolidation secondary time effect in the one-dimensional case where he assumes a Bingham body. This theory can be directly derived from my theory for the one-dimensional case by changing the stresses P in P—yield value. For example, Mandel's equation 36 is identical with equation 3 of my paper (1957b) 'Secondary time effects and consolidation of clays', when for q, $P-P_1$ will be taken and in β the generalized operator-form ψ will be inserted. Spoken in terms of the generalized model which I have shown in the second session, it means that the piston should be considered to have a constant friction. With regards to the Maxwell body, solutions have been given for several boundary conditions (TAN, 1954). For the coefficient of volume change J. Mandel assumes a power function $m_v = at^{\alpha}$. In my view this assumption should be limited to small values of time, and it has the physical disadvantage that for large values of time the settlement in the one-dimensional case increases to an unlimited extent. The definition of the degree of consolidation in this case may raise confusion, as no ultimate settlement can be the result. In the choice of m_v we are subjected to the restriction that m_v should be limited, and that the stresses should relax to a constant value, as it has been measured in our laboratory in Harbin. As I showed in the discussion during the second session, the assumption of a Maxwell body is sufficient to get a settlement which proceeds linearly with the log of time after the hydrodynamic period.

References

TAN, Tjong-Kie (1953). *Proc. 3rd International Conference on Soil Mechanics and Foundation Engineering*, Vol. 3, 169, Zurich

— (1954). Investigations on the rheological properties of clays. *Dr thesis Delft Techn-Un*

— (1957a). *Scientia Sinica*, VI, 1, p. 203 (Publ. Academia Sinica)

— (1957b). *Secondary time effects and consolidation of clays*, Publ. Academia Sinica, Inst. of Civ. Eng., Soil Mech. Lab., Harbin, China

— (1958). *Chinese J. Mech.*, Vol. 1. In press

G. G. MEYERHOF (Canada)

When concluding Paper 3a/26 on the Ultimate Bearing Capacity of Foundations on Slopes, I regretted the absence of published information on the magnitude of the ultimate load in practice. I am therefore glad to see that H. RAEDSCHELDERS and M. WALLAYS (3a/32) have described a full-scale foundation close to failure on a slope. However, an analysis of this problem using the proposed bearing capacity theory is made difficult by the variable soil conditions encountered and the absence of shear strength data of the soils encountered.

On the assumption that the two thin clay layers and the sand strata can be replaced by a uniform cohesionless soil ($c=0$), the minimum angle of internal friction for theoretical stability is found to be $\phi=40$ degrees. This estimate includes the effect of active earth pressure on the wall resulting in an eccentric and inclined foundation load. The average static cone resistance of about 100 kg/cm^2 measured below the footing level indicates a compact state of packing of the sand for which an angle of internal friction of 40 degrees appears to be reasonable (MEYERHOF, 1956). Further, the original inclination of the slope was $\beta=39$ degrees and thus provides a lower limit of the actual average angle of internal friction of the soil. After the slope had been flattened to its present inclination of $\beta=23$ degrees, the theoretical factor of safety against bearing capacity failure of the footing is estimated to be 1·3 using the same method of analysis. This factor of safety would ensure stability, as shown by Raedschelders and Wallays' settlement observations.

J. Kérisel's investigation (3a/15) in a test cylinder in sand provides welcome information on the bearing capacity and mechanism of failure of deep foundations in the field. The observed ultimate load corresponds to an angle of internal friction $\phi = 32\frac{1}{2}$ degrees according to my analysis (Meyerhof, 1951), which seems to be a rather low angle for compact sand. On the other hand, J. Kérisel observed an increase of the bearing capacity factor N_q with depth which is greater than I estimated; I had previously (1951) suggested for circular footings a critical depth of 10 times the foundation width with roughly linear increase of the factor N_q within that depth. On that basis I have also drawn attention to the care needed in extrapolating the results of cone penetration tests to shallow foundations and welcome J. Kérisel's emphasis of that requirement.

References

Meyerhof, G. G. (1956). Penetration tests and bearing capacity of cohesionless soils. *J. Soil Mech. and Found. Div., Amer. Soc. Civ. Engrs.*, Proc. Paper 866, **82**, No. SM1

— (1951). The ultimate bearing capacity of foundations. *Géotechnique, Lond.*, 2, 302

O. D. Zetterholm (Sweden)

The safety factor has as its purpose the decrease of the risk of collapse of a structure, at least when the cost and damage of a collapse are great, and that is mostly the case when dealing with buildings. The safety factor must therefore be chosen so high that collapse will be avoided. The calculation of a foundation is based on earth pressure theories and subsoil explorations. The soil characteristics when determined with conventional methods have always certain errors depending, among other things, on the fact that different test methods give different results and that the soil characteristics vary in different parts of the site within reasonable limits.

A way of introducing a suitable safety factor is to study the resulting error stemming from soil characteristics which are more or less uncertain. The safety factor can, according to Kjellman, W. and Wästlund, G. (1940, Säkerhetsproblemet i byggradskonsten. *Proc. No. 156 Swedish Research Academy*), be defined as the ratio between the uncertain quantity's probable maximum value and its value necessary for equilibrium.

An investigation based on this definition of the safety factor has been made by the Transmission Line Department of the Swedish State Power Board, introducing the resulting error of all the small errors made in determining the soil characteristics. The investigation deals with the safety problem for block foundations in transmission lines which are subjected to overturning moments. As the problem of settlement for transmission lines is rather special the margin of error can be taken rather fine. Consequently the safety factor has been found as low as about 1·3. For superstructures other than transmission towers, for example, buildings and bridges, the margin of error must be wider and the safety factor in these cases will therefore be greater. This investigation will be published as a paper in *Trans. Amer. Inst. elect. Engrs.* probably at the beginning of next year.

T. K. Chaplin (U.K.)

The General Reporter has proposed discussion of the development and use of semi-empirical formulae for bearing capacity calculations, and also discussion of the way of introducing safety factors into a limit design method for foundations. Paper 3a/6 by L. Bjerrum and A. Overland gives a suitable example to which we can apply such problems.

First, where the soil strength underneath a foundation varies (Chaplin, 1954), an analysis can be made in variable cohesive soil of the combined problem of bearing capacity and lateral flow effects. My empirical method was derived from an earlier limit design method by Meyerhof and Chaplin (1953). Such an analysis can, I feel, well be attempted on L. Bjerrum and A. Overland's problem.

Secondly, a safety factor depends on four main considerations: ignorance of the true ultimate load; the load deformation curve up to the ultimate; the sensitivity of the structure, etc., to damage by deformation; and the sensitivity of the clay and other functions of the soil.

Considering these in turn we can establish ultimate loads, particularly in uniform clay, with fair accuracy by bearing capacity theory. The load versus deformation curve for a foundation on clay can be estimated at least roughly, using the work of Skempton (1951) and Meyerhof (1951). On the last two points, in a paper which I gave to the Midland Soil Mechanics and Foundation Engineering Society in October 1956 (Chaplin, 1957), I considered the effect on the desirable factor of safety of this and other factors.

Paper 3a/11 by Ishii *et al.* is of particular interest for the results quoted from Aoyama's work on the relative performance of fixed and floating rings in the consolidation test. The load necessary to reach the same void ratio for Yokkaichi clay was 1·46 times higher in the fixed ring than in the floating ring. This is even higher than the ratio of about 1·23 found by Lewis (1950) at the Road Research Laboratory, Harmondsworth, when he tested remoulded London clay in 10 cm × 1 cm rings.

References

Chaplin, T. K. (1954). Discussion of paper by Prof. A. W. Skempton. *Proc. Instn. civ. Engrs.*, Pt 1, 3 (5), 610

— (1957). *Proc. Midland Soil Mech. and Found. Engg. Soc.*, **1** (1), 4

Lewis, W. A. (1950). *Road Research Note No. RN/1349/WAL*

Meyerhof, G. G. (1951). *Géotechnique, Lond.*, **2**, 301

— and Chaplin, T. K. (1953). *Brit. J. App. Phys.*, **4** (1), 20

Skempton, A. W. (1951). *Bldg. Res. Cong., Lond.*, Div. 1, 180

K. H. Roscoe (U.K.)

I should like to correct one or two erroneous impressions which readers of the General Report on this Division may have concerning recent work at Cambridge on footings for superstructures which are designed according to the plastic theory. My Paper 3a/35 represents only a minor portion of this work, most of which has been summarized in the *British Welding Journal* of August 1956 and January 1957.

My only reason for speaking this morning is that this Journal is not generally read by soil mechanicians and I suspect that it may not have been seen by the General Reporter. He describes the tests as 'model tests' which, indeed, they possibly are; but a model test conjures up to my mind something on the laboratory table. While not claiming that the scale of the tests was large, they were carried out in the field. A view of the test site is shown in Fig. 5. The structure under test consists of two similar 'saw-tooth' portal frames connected together by purlins to represent 1 bay of a single-storey building. The span of each portal frame was 16 ft. The vertical loading tanks can also be seen, each capable of taking a load of 15 ton. The portals rested upon reinforced concrete pier-type foundations. Each pier was $1\frac{1}{2}$ ft. in diameter and 3 ft. deep and the top of one of them is shown in Fig. 6.

The problem of designing these piers entailed consideration of a three-dimensional rupture surface, or zone, in the soil. Fig. 7 represents the form of such a surface if the pier is constrained to rotate about ground level. All the theories, except one, known to me when designing the piers in 1953 assumed a two-dimensional rupture surface corresponding to *ff'bb'cc'dd'* in Fig. 8. The only three-dimensional theory,

Fig. 5 General view of test site, showing portal frame and loading mechanism

Vue du site, la structure en portique et méthode de charge

due to Fordham, assumed a similar zone of failure of the soil but endeavoured to take account of the forces on the plane sides *afbcde* and *a'f'b'c'd'e'* of the sector. Elevations of some

Fig. 6 A pier before loading

Pieu avant l'essai

of the shapes assumed in the different theories for the arc *cd* in Fig. 8 are given in Fig. 9.

During the experimental work, after loading the portal

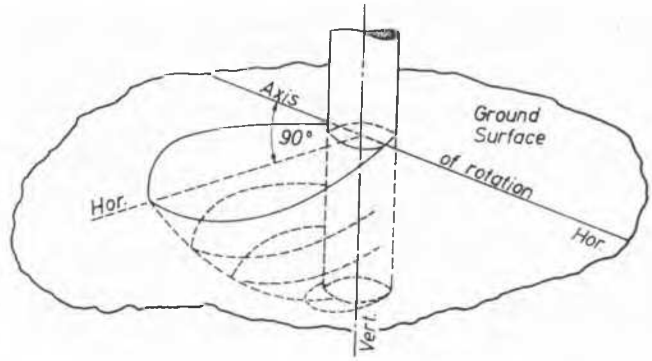

Fig. 7 Embedded pier compelled to rotate about ground level. Form of actual failure surface

(By courtesy of British Welding Journal, Vol. 3, No. 8, 1956)

Rotation au niveau du sol d'un pieu enfoncé. Forme de la surface observée de rupture

frames to collapse, the upper part of the superstructure was removed and the individual piers were tested separately until the soil failed. Four of the piers were constrained to rotate

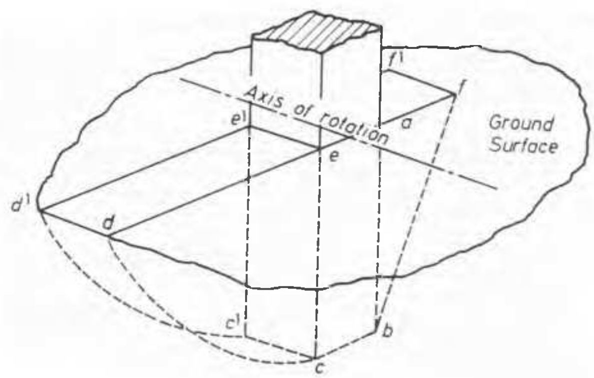

Fig. 8 A theoretical failure surface

(By courtesy of British Welding Journal, Vol. 3 No. 8, 1956)

Une surface théorique de rupture

about ground level and the average moment M required to cause soil failure was 10·1 ton·ft. This measured value and also that of 3·0 ft. for dimension y in Fig. 9 may be compared with the values predicted by seven theoretical methods as shown in columns 3 and 4 of the table below. Column 5 of

	Full-scale test			*Model test*
1	*2*	*3*	*4*	*5*
Method	δ' degrees	M ton·ft.	y ft.	M lb.in.
I Spiral centre at O	—	14·2	14·0	330
II Spiral centre at O_2	—	6·0	8·0	118
III Rankine	0	4·0	6·1	95
IV Hansen	20	9·0 —	3·0 —	179 162
V Code of practice	20	10·2	>6	175
VI Caquot and Kérisel	20	10·9	>6	205
VII Fordham	0	4·7	3·0	70
Measured values	—	10·1	3·0	175

(By courtesy of British Welding Journal, Vol. 3, No. 8, 1956)

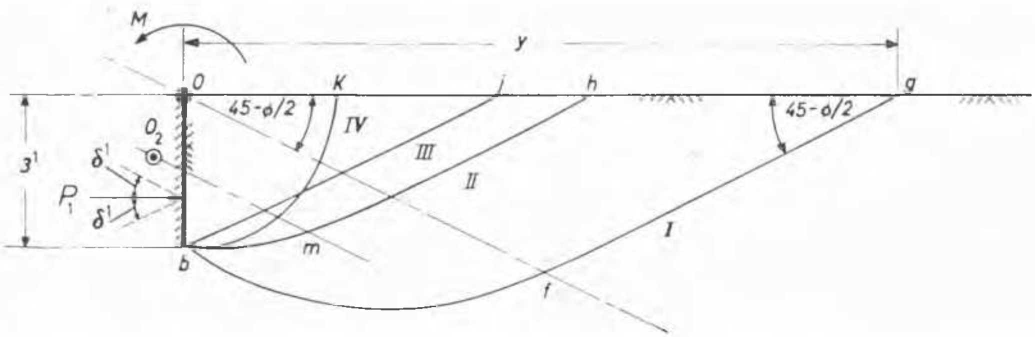

Fig. 9 Elevation of theoretical rupture surfaces (after K. H. Roscoe and A. N. Schofield)
Schema des surface théoriques de rupture

the table also shows theoretical values of the moment required for soil failure in a preliminary model test in the laboratory using a footing 6 in. deep. The measured value was 175 lb.in. Both tests favour method V for predicting M but not y.

I can see no reason why any of these theories should give agreement between predicted and measured values. It is no reflection on any one of them, except method VIII, that they

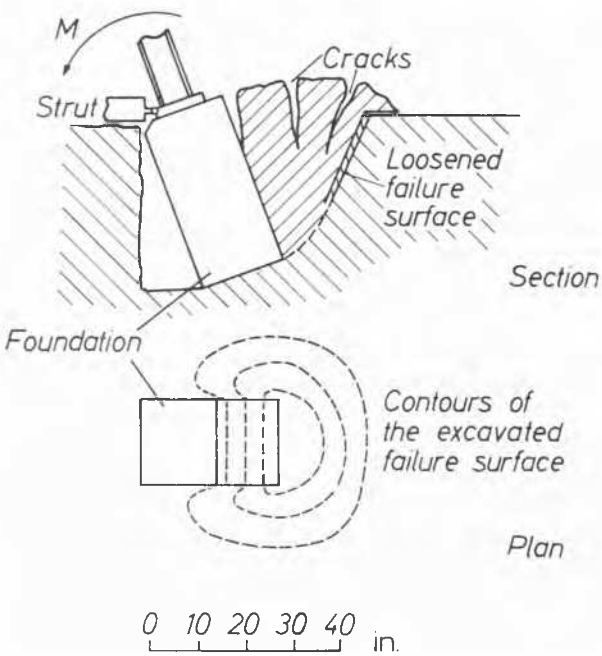

Fig. 10 Views of a typical failure surface
(*By courtesy of British Welding Journal*, Vol. 3, No. 8, 1956)
Vue typique d'une rupture

do not, since the theoretical failure surface is of the form shown in Fig. 8 while the actual measured failure surface is as shown in Fig. 10.

In this particular problem it was found that a tied pier would have to be only 3 ft. deep, whereas a free pier would, in order to do the same job, have to be 6 ft. deep at least. I do not want the conclusion to be drawn that tied piers are always twice as strong as free piers. Each case must be treated on its merits.

Y. Tcheng (France)

Monsieur le président, mesdames, messieurs, je voudrais tout d'abord félicitér le rapporteur général pour son excellent exposé.

Il y a signalé qu'il avait mesuré la résistance à la rupture à l'aide de fondations en modèles réduits sur du sable sec. Les valeurs de ces résistances à la rupture mesurées en laboratories sont de plusieurs fois supérieures aux valeurs théoriques.

Depuis 1953, nous avons également entrepris, au Centre expérimental du bâtiment et des travaux publics à Paris, des essais en modèles réduits de la résistance à la rupture des fondations à deux dimensions en milieux pulvérulents (sable, bille de verre etc. . . .). Les résultats expérimentaux ont été égaux de 1·5 à 2 fois la résistance théorique.

Nous pensons que les raisons principales des écarts entre les résultats d'essai et les calculs sont probablement les suivantes: 1° La densité du sable augmente sous la charge; cette augmentation n'est d'ailleurs pas constante le long de la ligne de rupture; par ailleurs, la rupture s'obtient toujours après un certain tassement du sable. Il est par conséquent, très difficile, dans le cas pratique, de séparer la charge de rupture superficielle et celled'une fondation ancrée dans le sol. Pour éviter les inconvénients du modèle réduit, nous avons entrepris une série d'essais *in situ* avec des fondations d'assez grandes dimensions; la puissance de charge utilisée atteint parfois plus de 20 tonnes. Les fondations sont d'ailleurs de formes assez diverses: circulaires, annulaires, carrées, rectangulaires; nous avons même des quadrillage de semelles filantes. Nous espérons pouvoir obtenir des résultats très prochainement que nous communiquerons lors du prochain congrès. Nous pouvons néanmoins déjà signâler que les résultats expérimentaux sont toujours supérieurs aux valeurs théoriques.

A. Lazard (France)

En complément de ma communication 3a/19, je veux indiquer la comparaison de mes formules avec 2 séries d'essais Anglais, intéressantes à divers titres, l'une dans l'argile du Sud-Ouest de Londres (Figs. 11 et 12), l'autre dans du sable du Nord de Londres (Fig. 13).

La Fig. 12 donne la comparaison des formules avec les essais de Mr. Simpson de la British Insulated Callender Construction Ltd.; on voit que le résultat est extrèmement satisfaisant.

La Fig. 11 donne: d'une part les valeurs des cohésions = mesurées à l'essai de compression simple = dans des prélèvements effectués tous les pieds; et d'autre part la comparaison avec la formule des Electriciens convenablement aménagée. On voit que les résultats sont moins satisfaisants que dans la Fig. 12, surtout si on prend les cohésions des prélèvements soit tous impairs, soit tous pairs. Les résultats sont encore plus mauvais avec les valeurs mesurées au moulinet (vane test) et au pénétromètre (Proctor needle).

La Fig. 13 donne la comparaison des formules A et B avec les essais faisant l'objet des communications de K. H. Roscoe en 3a/35 et le British Welding Journal. Comme il fallit s'y

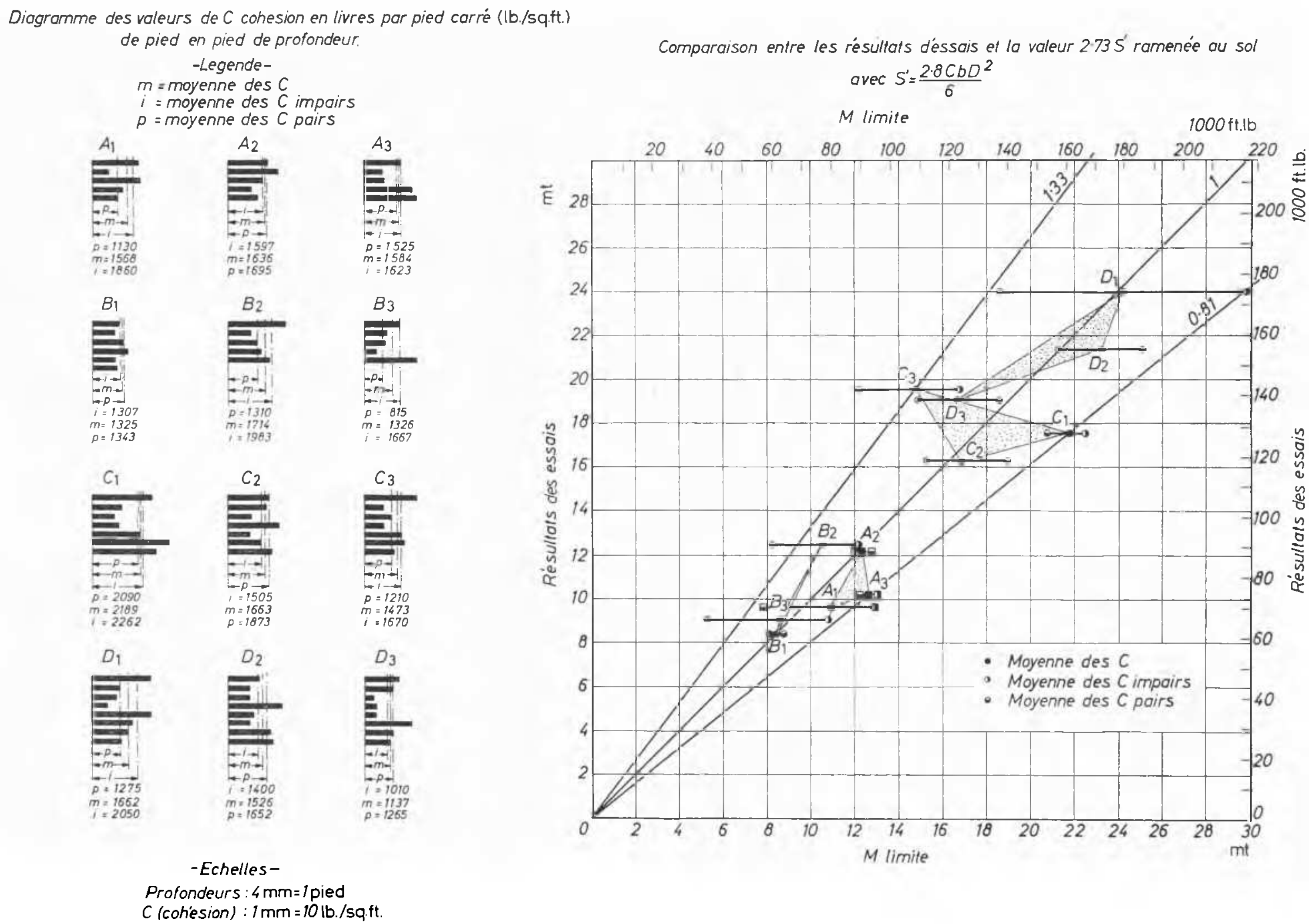

Fig. 11 Diagramme des valeurs de C-cohésion—en livres par pied carré (lb./sq. ft.) de pied en pied de profondeur: m=moyenne des C; i=moyenne des C impairs; p=moyenne des C pairs. Comparaison entre les résultats d'essais et la valeur $2{\cdot}73S'$ ramenée au sol avec $S'=(2{\cdot}8CbD^2)/6$

Diagram of the values of C in lb./sq. ft. per ft. of depth: m=mean of C values; i=mean of odd C values; and p the mean of even C values. Comparison between the test results and the value of $2{\cdot}73S'$ reduced to a soil of $S'=(2{\cdot}8CbD^2)/6$

Ce tableau doit être consideré enjointement avec la **Fig.** 12. This table is to be read in conjunction with Fig. 12.

Forme des fondations et dimensions	*No.*	*Angle de renversement*	*Moments obtenus aux essais, ramenés au niveau du sol (x)*		*Formule A*			*Formule B*		
					T.H. limite (y)		$\frac{x}{y}$	*T.H limite (z)*		$\frac{x}{z}$
			ft.lb.	m kg	ft.lb.	m kg		ft.lb.	m kg	
■ 2′×2′ D=5′	A_1	5° 07′	70,000	9,700	72,800	10,070	0·96	70,900	9,800	0·99
	A_2	7° 41′ 10° 46′	85,000 89,000	11,700 12,300	73,000	10,100	1·16 1·22	70,900	9,800	1·20 1·25
	A_3	7° 21′ 10° 20′	72,000 74,000	9,950 10,200	73,000	10,100	0·99 1·01	70,900	9,800	1·01 1·04
ϕ=2′ D=5′	B_1	4° 36′ 7° 36′	57,000 61,000	7,900 8,400	63,300	8,800	0·90 0·96	70,900	9,800	0·80 0·86
	B_2	5° 02′ 8° 22′	84,000 90,000	11,600 12,400	63,300	8,800	1·33 1·42	70,900	9,800	1·19 1·27
	B_3	7° 57′ 11° 20′	60,000 66,000	8,300 9,100	63,300	8,800	0·95 1·04	70,900	9,800	0·85 0·93
ϕ=2′ D=6′ 6″	C_1	5° 02′	127,000	17,600	108,600	15,000	1·17	126,200	17,450	1·01
	C_2	6° 01′ 7° 40′	115,000 118,000	15,900 16,300	106,700	14,800	1·08 1·10	126,200	17,450	0·91 0·93
	C_3	6° 48′ 9° 08′	132,000 141,000	18,200 19,500	106,700	14,800	1·24 1·32	126,200	17,450	1·05 1·12
ϕ=2′ D=8′	D_1	6° 05′ 8° 09′	165,000 174,000	22,800 24,000	168,900	23,300	0·98 1·03	183,720	25,400	0·90 0·95
	D_2	6° 05′ 9° 59′	147,000 155,000	20,300 21,400	168,900	23,300	0·87 0·92	183,720	25,400	0·80 0·84
	D_3	5° 19′ 8° 13′	117,000 138,000	16,200 19,100	167,500	23,100	0·70 0·82	183,720	25,400	0·64 0·75

Ce tableau doit être consideré enjointement avec la Fig. 13. This table is to be read in conjunction with Fig. 13.

No.	*Forme de la base*	*Dimensions de la base ϕ ou e×b*	*Proffeur D*	*Hauteur de tirage H*	*Poids du poteau*	*Moment limite*				
						obtenu aux essais	*formule A*		*formule B*	
							avec D′=0	*avec D′=0·3 m*	*avec D″=0*	*avec D″=0·3 m*
		m in.	m in.	m in.	kg lb.	m kg ft.l b.	m kg ft.lb.	m kg ft.lb.	m kg ft.lb.	m kg ft.lb.
6XA	●	0·419 *16½*	0·914 *36*	2·26 *89*	52 *115*	1,205 *8,736*	2,416 *17,475*	1,968 *14,235*	1,893 *13,692*	1,202 *8,694*
6XE	●	0·419 *16½*	0·914 *36*	2,39 *94*	91 *200*	1,316 *9,542*	2,434 *17,605*	1,983 *14,343*	1,917 *13,866*	1,218 *8,810*
6YA	●	0·419 *16½*	0·914 *36*	2·23 *87½*	52 *115*	1,335 *9,677*	2,416 *17,475*	1,968 *14,235*	1,888 *13,656*	1,198 *8,665*
6YE	●	0·419 *16½*	0·914 *36*	2·54 *100*	54 *120*	1,180 *8,557*	2,416 *17,475*	1,968 *14,235*	1,942 *14,046*	1,235 *8,933*
7XA	■	0·432 *17×17*	0·914 *36*	2·39 *94*	54 *120*	3,183 *23,072*	> 5,756 *>41,633*	> 4,688 *>33,908*	> 3,852 *>27,862*	> 2,512 *>18,169*
7XE	■	0·432 *17×17*	0·914 *36*	2·53 *99½*	54 *120*	3,059 *22,176*	> 5,756 *>41,633*	>4,688 *>3,308*	> 4,000 *>28,932*	> 2,544 *>18,400*
7YA	■	0·432 *17×17*	0·914 *36*	2·375 *93½*	91 *200*	2,410? *17,472?*	> 5,786 *>41,850*	> 4,712 *>34,082*	> 3,948 *>28,556*	> 2,508 *>18,140*
7YE	■	0·432 *17×17*	0·914 *36*	2·49 *98*	54 *120*	3,770 *27,328*	> 5,756 *>41,633*	> 4,688 *>33,908*	> 3,988 *>28,845*	> 2,536 *>18,343*

Nota: les chiffres penches donnent les valeurs en unités Anglaises. Pour les essais No. 7 on a pris dans le calcul les valeurs minima suivantes= $K \geqslant 2$ et $p_2 \geqslant 12$.

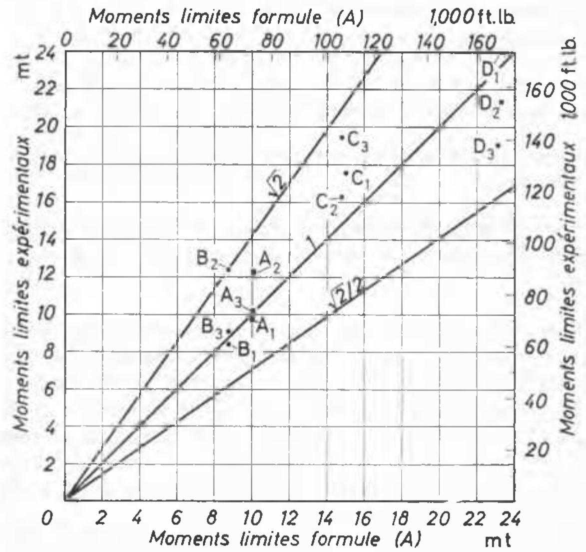

Fig. 12 Results de l'application aux fondations de Mr. Simpson des formules françaises *A* et *B* Fevrier 1957. Comparaison entre les résultats d'essais et la formule française AM lim = $27{\cdot}45 M_B{}^{2/3}$

Results of the application to Mr. Simpson's foundations of the French formulae *A* and *B*. of February 1957. Comparison between the test results and the French formula AM lim $= 27{\cdot}45 M_B{}^{2/3}$

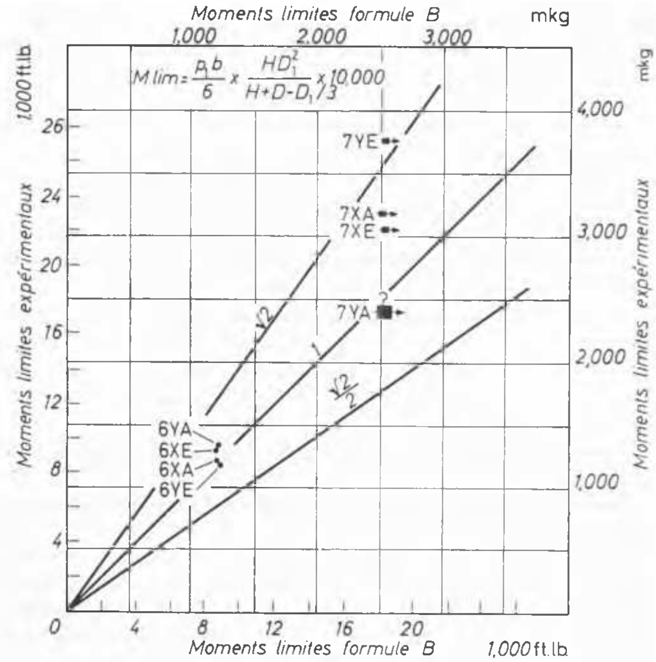

Fig. 13 Essais de K. H. Roscoe et A. N. Scholfield. Résultats de l'application aux fondations des formules françaises *A* et *B*. Comparaison entre des résultats d'essais et la formule française *B*, avec $D''=0{\cdot}3$ m

K. H. Roscoe and A. N. Scholfield's tests. Results of the application to foundations of the French formulae *A* and *B*. Comparison between the results of tests and the French formula *B*, with $D''=0{\cdot}3$ m

attendre avec des fondations aussi peu profondes (3 pieds seulement) la formule *A* donne des valeurs un peu trop élevées.

Pas contre la formule B est excellente

Les calculs sont faits une fois avec mort terrain nul, une fois avec un mort terrain de 1 pied, parce que l'article du British Welding Journal mentionne une couche superficielle d'humus de 1 pied.

Les fondations 7 ont été calculées avec les valeurs minima de $K=2$ et $p_1=12$, correspondant aux meilleures butées réalisées dans mes propres essais. Il était évident que la butée exceptionnelle réalisée par Roscoe correspondait à des valeurs supérieures.

Aujourd'hui nous disposons d'un ensemble de 230 *essais en vraie grandeur*, tous exécutés dans les conditions très variées. Sur ves 230, un seul (1) a donné une valeur nettement inférieure à ce que les formules *A* et *B* permettaient de prévoir.

Nous ignorons la raison profonde pour laquelle la nature des terres ne paraît pas intervenir. Souhaitons qu'elle soit découverte d'ici le prochain Congrès.

J. Kérisel (France)

Monsieur le président, messieurs, en conclusion de notre séance d'hier, le M. Vargas demandait s'il y avait effectivement une relation entre la résistance de pointe du pénétromètre et la force portante des fondations, larges ou étroites. Je voudrais intervenir à propos de cette intéressante question.

A une profondeur donnée *D* dans un milieu de densité homogène de densité γ, la surcharge est γD et nous avons convenu de désigner par $N_q \times \gamma D$ l'une ou l'autre des quantités précédentes dont nous nous posons la question de savoir si elles sont égales ou différentes.

Puisque le pénétromètre est descendu à la même profondeur que la fondation, ce qui m'est paru essentiel est d'étudier plus particulièrement la variation de N_q avec le diamètre *d* du pénétromètre, toutes choses égales d'ailleurs.

Nous avons indiqué, Fig. 12 de notre article 3a/15, les premières de nos expériences en sable de Nemours, très serré avec teneur en eau de 1·5 per cent avec pénétromètres de 5, 10 et 20 mm.

Depuis lors nous avons fait des experiences *in situ* dans la forêt de Fontainebleau et à Dunkerque, toujours dans des sables serrés, mais avec des teneurs en eau supérieures.

Sur le diagramme, nous avons porté en abscisses l'expression sans dimension égale au rapport de la profondeur *D* au rayon hydraulique $d/4$ de la section du pénétromètre ou encore $4D/d$.

En ordonnées, nous avons porté les valeurs de N_q. Pour une même profondeur relative on constate encore que N_q est d'autant plus grand que le diamètre est plus petit. Ceci tient-il à l'effet de la grosseur des grains dont les plus gros ne dépassent pas 2/10 mm dans tous les cas où a des équilibres de poinçonnement? La question est ouverte. Nous nous bornons pour le moment à l'étude expérimentale. En tous cas, cette constatation est à rapprocher des observations du E. C. W. A. Geuze concernant la pente du diagramme du pénétromètre qui variet en raison inverse du diamètre au voisinage d'une couche résistante.

Nous constations aussi que N_q pour un même diamètre croit, puis décroit, ce qui veut dire que la pression $\gamma D \times N_q$ a tendance à se stabiliser à partir d'une certaine profondeur, résultat à rapprocher des observations du H. Zweck.

Finalement, pour éliminer le paramètre teneur en eau qui augmente N_q très notablement, nous sommes revenus au laboratoire des ponts et chaussées et nous avons recommencé les expériences avec du sable de Nemours sèché à l'étuve et nous avons multiplié les diamètres en utilisant ceux de 5, 10, 20, 40, 60 et 80 mm. Les conclusions demeurent les mêmes. Pratiquement, à une même profondeur absolute, on obtient les valeurs suivantes de N_q, suivant le diamètre:

diamètre	5 mm	10	20	40	60	80
N_q	600	430	330	240	160	97

Pour un mètre de profondeur absolue les chiffres sont les suivants:

d	40	60	80
N_q	180	130	98

Les intervalles se resserrent maisle sens de la variation reste le même.

Pour des sables non serrés, les variations sont beaucoup plus faibles.

En conclusion, les résultats du pénétromètres sont à minorer, d'autant plus qu'on les applique à des fondations plus larges et à des milieux de plus en plus serrés. Ceci est conciliable avec les conclusions de C. VAN DER VEEN et L. BOERSMA (3b/15) qui, d'une part n'opèrent pas en milieu serré et, d'autrepart, se tiennent en deça des pressions extrêmes du cône pour faire leur comparaison.

Je les rejoins sur leur proposition de la normalisation du diamètre. J'ajouterai même qu'il est essentiel de normaliser le système de mobilisation de butée de pointe, c'est-à-dire le déplacement relatif de celle-ci, la butée croissant avec celui-ci.

G. DE JOSSELIN DE JONG (Netherlands)

The General Reporter has pointed out that for the determination of slip line fields only some simple solutions are available and the more complicated cases, especially those taking account of the weight of the medium, need tedious numerical treatment.

I should like to draw attention to a graphical construction which permits a simple and straightforward determination of the shape of the slip line field. This method is an extension of the method which Prager in 1953 developed for materials with $\phi = 0$ and consists essentially of the construction of the characteristics in the same way as the solution of Kötter's equations does mathematically.

The characteristics in the physical plane are the slip lines, and in the stress plane the paths followed by the pole are taken as the other characteristics.

For materials with ϕ unequal to zero the procedure is based on the rule, that:

If a slip line is followed in the physical plane the pole will follow a path that is oriented in the direction of the conjugated slip line.

The introduction of the self-weight of the medium entails the following rule:

The vertical distance between the pole paths equals the horizontal projection of points in the slip-line field times γ the specific weight.

With these rules we can construct the whole slip-line field starting from stress conditions along the boundaries in a simple way, because the only thing to do is to draw parallel lines.

The construction applied to a shallow footing is given in Fig. 14. This solution is identical to Lundgren's solution obtained mathematically in 1953. Fig. 15 gives the bearing capacity factor N_γ compared with the value given by Terzaghi as a function of depth divided by breadth.

The General Reporter also pointed out that we cannot be satisfied if the movements of the system are not accounted for.

Application of the hodograph in the way Prager does would give the next rule:

The endpoint of the velocity vector for points along a slip line follows the direction of the conjugate slip line in such a way that dissipation of energy is positive.

However, for materials with ϕ unequal to zero the rotations which are not discriminated for $\phi = 0$ give an additional complication. A unique solution of the velocity field is obtained only if the rotations required by the boundary are satisfied by one exclusive rotation. If there is a range of possible rotations the solution is undetermined. If no other rotations required by one part of the boundary can be met by another part there is an internal contradiction, which indicates that another slip-line pattern has to be adopted.

Fig. 16 shows the velocity distribution for the shallow footing if the movement of the foundation is restricted to a

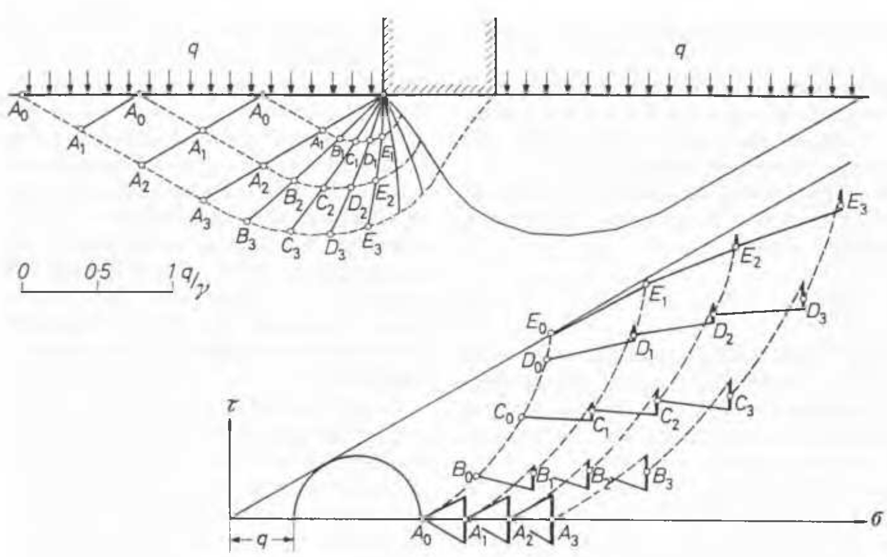

Fig. 14

translation only, which makes an angle between + and − $\{(\pi/4)-(\phi/2)\}$ with the vertical. The additional rotation can only be zero here and so the velocity distribution is uniquely determined. In both cases the slip-line field is the same, the resultant force is the same, but the movement is quite different.

In these solutions the velocity field conserves volume and the internal dissipation energy equals the work done by the external force. The solution is kinematically consistent.

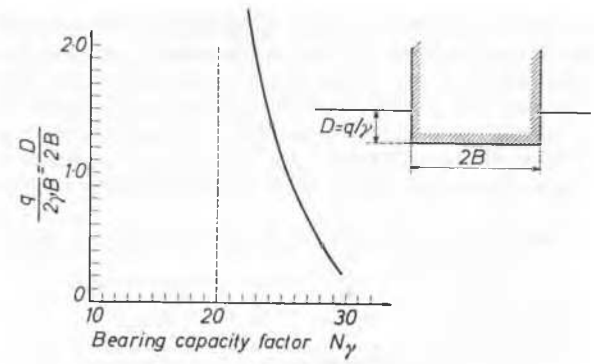

Fig. 15

Special attention must be drawn to the friction character of soils which distinguishes them from materials with a plastic potential. A material with $\phi=0$ follows the principle of de Saint Venant who inferred that the orientation of stress tensor and deformation tensor coincide. This coincidence does not hold for a material with $\phi\neq 0$ where there may be a difference in orientation from $-\frac{1}{2}\phi$ to $+\frac{1}{2}\phi$.

Another consequence is the absence of a plastic potential.

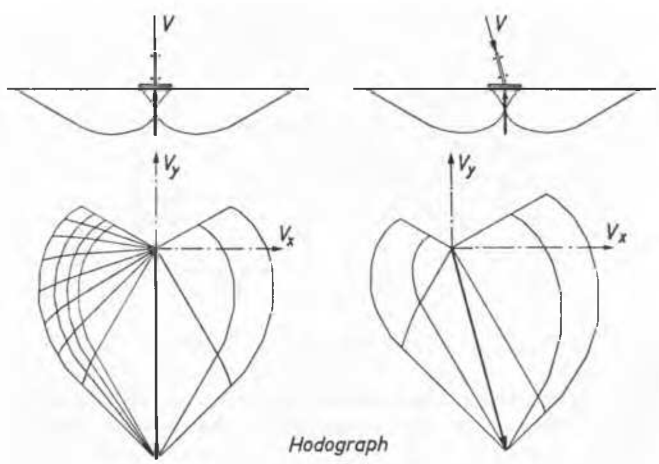

Fig. 16

It has been established by Drucker and Prager, in 1952, that if such a material should have a plastic potential this would entail a volume dilatation during plastic flow. In reality the dilatancy is less then computed from this theory. So we infer that only a part of the grain contacts may have a plastic potential but the majority has not got one. This absence of a plastic potential prohibits the use of the attractive theorems developed in limit design.

J. Feld (U.S.A.)

I want to comment first on Paper 3a/31 by D. E. Polshin and R. A. Tokar. The tabulations of recommended criteria of differential settlements for various types of structures are a new and important contribution to foundation engineering. During a careful check on 14 seven-storey reinforced concrete frame apartments where foundation provision was partly inadequate and strengthening was provided, it was found that no structural distress in the concrete floors or the brick walls, in the form of one storey curtain walls, was noted with differential settlements between adjacent columns of about $0{\cdot}004L$. This is twice the value noted as item 2(a) in Table 2 of 3a/31. Some cracks in the brickwork were formed in one building where the differential settlement between two adjacent exterior columns exceeded the value of $0{\cdot}004L$.

There is, however, a more important point to be considered in a study of this nature. Structural frames of multi-storey height are generally much more rigid than the footings, even where the foundation is a mat or combined footing. A differential settlement upsets the normal distribution of load reaction. Normally it operates beneficially: the stiffer footing receives more than its share of building load and the settled footing is relieved of loading. This tends to balance unit loading nearer to the capacity of the local soil to support the loading at equal settlements. However, cases have been observed where the structure is so stiff that settlement under one footing causes a complete transfer of loading to the adjacent column, or line of columns, with structural failure due to overloading. Under such conditions the permissible differential settlements may be structurally unsafe. A description of such cases is given by me in articles published in *Proc. Amer. Soc. civ. Engrs.*, Separate 632 (1955) and *J. Boston Soc. civ. Engs.*, p. 108–127, 1956. I question whether item 2(b) of Table 2 is a misprint, a value of $0{\cdot}007L$ seems excessive even for sand and clay in hard condition for permissive differential settlement.

I should like now to say something about Paper 3a/9 by H. Grasshoff. This subject can be approached from either a scientific or an engineering point of view. The scientific study can show the variation in bending moments resulting from a great number of assumed relative rigidities. The engineering approach is only interested in the answer to two questions: (1) is the design safe, (2) is the design economical.

For individual footings, generally square, carrying a single load, the usual criteria of design dimensions are the shear and bond stresses. Since their values are maximum at the sections close to the column face, where the total must be the same, no matter what the distribution of contact pressure, there is no value in assuming or computing any distribution except in the simplest one. This is especially true for footings on fairly rigid soils. A slight gain in reinforcement can be obtained for large footings if reduction in contact pressure with deflection is assumed. I described this in Section 26, Vol. III of the 1956 *American Civil Engineers Practice* (New York; John Wiley).

However, if code or specification limitation exists on the maximum unit base pressure, any modification from assumed uniform contact pressure must increase the size of the footing to keep the maximum value below the required maximum. Such size increase will more than overcome the saving in reinforcement costs.

From the criterion of safety, uniform distribution has resulted in footing designs that have served the desired purpose with no recorded unsafe or excessive deflection conditions. As far as economy is concerned, the uniform distribution design is equal to the design from any other assumption, unless a higher unit pressure is permitted in the middle areas of spread footings.

Such permission, in my opinion, is justified because the centre areas of loaded soils are more restrained to lateral flow and therefore can carry greater load concentration at equal deformation vertically. In the U.S.A. the general code controls on maximum computed values make it almost impossible to take advantage of such added strength.

As far as combined footings are concerned, there is little evidence that existing designs on the basis of uniform distribution are not structurally safe and satisfactory. Where the footing area is chosen to provide uniform support at the maximum permissive unit value, any one of the four cases described by the author would require wider footings to keep the maximum unit at such value. For the contact pressure summation must equal the sum of the loadings.

Unit contact pressures of two times the average for the footings on soft and on stiff clay (Figs. 7 and 8) and three times the average for the footings on loose and on firm sands (Figs. 9 and 10), would require increase in footing widths which would seriously modify the indicated savings from bending moment reductions. Optimum economy requires a change in footing shape, wider where the contact pressures are greater; but this entails the increase in costs from the simple rectangular plan in formwork and reinforcement details.

Where footing areas are larger than necessary because of some architectural or foundation reasons, such as in full floor area mats, I have used an assumption recommended by Meyerhof, of arbitrary assignment of areas symmetrically positioned under each load to provide bearing at the desired contact pressure value and designing moments from such distribution.

Building frames of multiple stories are extremely rigid, beyond any computed values. This has been indicated by the way that structures have resisted collapse when some footings and columns have become non-operative from extreme settlement or loss due to blast explosion. Cases of complete load transfer from one line of column supports to adjacent columns have been found when exterior conditions cause relative settlements of less than 1 in. Cases of no apparent injury to both steel and concrete frames have been noted where columns are found to have a relative settlement as much as 2 in. with respect to their neighbouring supports.

Paper 3a/9 is an extremely interesting analysis of the problem and covers the subject well. The discussion is in no way intended to question the scientific validity of the paper, merely to point out that the practical economical aspects of the problem must not be disregarded.

E. de Beer (Belgium)

In his contribution (3a/33) P. E. Raes outlines the imperfections of Andersen's method for determining the bearing capacity under a footing. These imperfections are well known. Indeed, Andersen's method is an extreme method, and thus only one equilibrium condition is considered. Furthermore, it is not a 'pure' extreme method as it necessitates a more or less arbitrary assumption concerning the distribution of the soil reactions along the assumed slip surface and considers but one parameter for determining the location of that surface.

In his method Andersen makes the same assumptions with regard to soil reactions as those made in the method of the vertical slices of Fellenius. The same imperfections as those mentioned by P. E. Raes for the method of Andersen exist also for that of Fellenius. Nevertheless, this latter method has been used extensively for controlling the equilibrium of slopes. As the method of Fellenius gives acceptable values in cases where the forces due to the weight of the soil are predominant, the same can be said for the method of Andersen.

Up to the present time no exact method seems to exist for determining the bearing capacity under a footing established on a soil subject to gravity. Some methods involve surfaces which are kinematically possible, but which do not fulfil all static conditions. Other methods fulfil all the static conditions but involve sliding surfaces which are kinematically impossible. All methods which actually exist for determining the bearing capacity of soils which are subject to gravity, even if they are more intricate and are in some way more exact than the crude method of Andersen, are nevertheless all based on some more or less arbitrary assumptions and are not at the same time kinematically and statically possible. For that reason, as long as the exact statically and kinematically possible solution for a soil subjected to gravity has not been found, we have adopted the results of Andersen's method in all cases where this method gave lower values than other methods based on kinematically possible surfaces, and which did not respect all static equations. We have employed this method since 1948. Since then many improvements have been made in connection with the problem of the bearing capacity of footings in soils subject to gravity (particularly by Meyerhof, Lundgren and Mortensen, Brinch Hansen and the revised formulae of Caquot–Kérisel). It is thus interesting to compare the results obtained with these different methods with those obtained by the combination of Buisman–Mizuno–Andersen, which we have employed since 1948.

The bearing capacity factors obtained by different methods

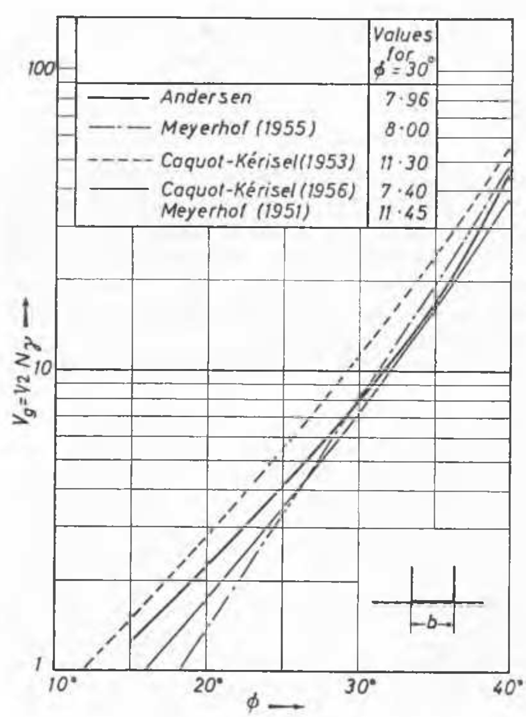

Fig. 17

for a footing established on the surface of a soil subject to gravity, when $c=0$, and for different values of the angle of friction ϕ are shown in Fig. 17. The values obtained for that factor with the crude method of Andersen are almost the same as those obtained by Meyerhof in the latest improvement which he has introduced to his method, and which is based on the results of Lundgren and Andersen.

Fig. 18 gives the bearing capacity factor $d_g/b\gamma_k$ as a function of the angle of friction ϕ, when $P_b/\gamma_k b = 0{\cdot}5$. In these formulae the symbols have the following meaning:

D_g = the bearing capacity (t/m or t/ft.)
d_g = the bearing capacity per unit of surface (t/m^2 or t/sq. ft.)
b = the total width of the footing (m or ft.)
γ_k = the volume weight of the soil to be introduced for the calculation of the effective stresses
P_b = the overburden pressure at the foundation level on both sides of the footing.

The values obtained with the combination of Buisman–Mizuno–Andersen in the case $P_b/\gamma_k b = 0{\cdot}5$ are a little higher than those obtained by Meyerhof or other methods. This result could be expected. Indeed, the results of the methods of Meyerhof, Caquot–Kérisel, etc., in the cases where $P_b \neq 0$ and $\gamma_k \neq 0$, are based on the application of the principle of superposition, which gives to low values, i.e. values on the safe side.

On the contrary, the application of the combination of Buisman–Mizuno–Andersen considers for each case one well defined sliding surface, and does not need the introduction of the principle of superposition. In the case of $P_b/\gamma_k b \neq 0$ the application of the method of Andersen gives higher values for the bearing capacity than those obtained by other methods, contrary to the statement of P. E. Raes that this method gives lower values always. This statement is probably based on the results of the example included in the paper. It must however be stated that the method used in the example differs from the method of Andersen for the following reasons: First, in the example the slip surface consists of one circle. In the method of Andersen the assumed slip surface consists of two circles with radii r and $r+D$. The value of the length D is

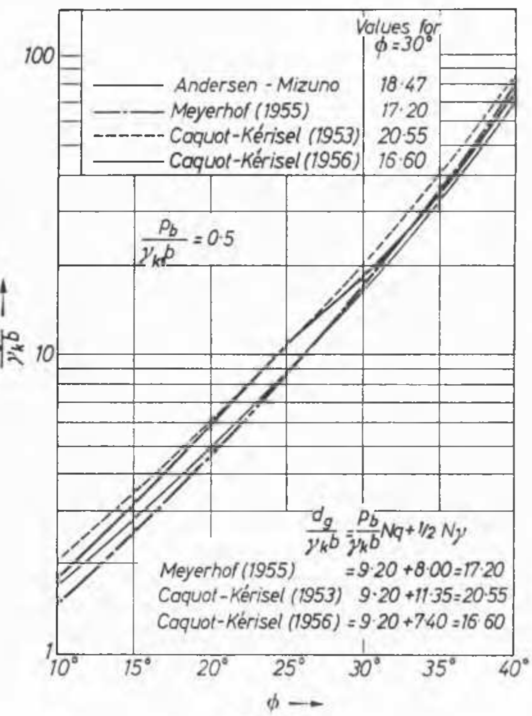

Fig. 18

$$D = \frac{P_b}{\gamma_k} = \frac{P_0}{\gamma}$$

With $\gamma = 0$ one gets $D = \infty$.

For a slip surface with $r+D=\infty$ the corresponding bearing capacity factor $V_b = N_q$ is also infinite. This clearly indicates that, in the case of a soil without weight, the method of Andersen gives much higher values for the bearing capacity factor than the real ones. Furthermore, the very big discrepancy between the infinite value and the real value shows that the method of Andersen, applied to the case where the forces due to the gravity are zero, has no longer any practical meaning.

Secondly, in the method of the example the three equilibrium conditions are considered. The method is thus no longer an extreme method as in the case of the Andersen method.

Furthermore, in the example the arbitrary assumption is made that the resultant of the soil reactions is tangential to the friction circle $r \sin \phi$; but actually this resultant R is outside the circle $r \sin \phi$ and its lever arm is $K \sin \phi$ with $K > 1$. The value of the factor K depends on the distribution of the soil reactions along the slip surface. When the angle in the centre is $2\theta = \pi$, the formulae of Ohde indicate that, with a uniform distribution $K = 1{\cdot}625$ and with a parabolic distribution $K = 1{\cdot}064$. In the case considered the distribution is neither uniform nor parabolic, as it is not even symmetrical. The exact value of K is not known, but it is certain that K is much higher than 1.

The values of the bearing capacity factor V_b as a function of K, for $\phi = 30$ degrees, are indicated in Fig. 19, together with the value of $\rho = r/b$ when the method given in his example by P. E. Raes is followed. For $K = 1{\cdot}24$ the value V_b found with the circular surface is the same as the classical value $V_b = 18{\cdot}4$

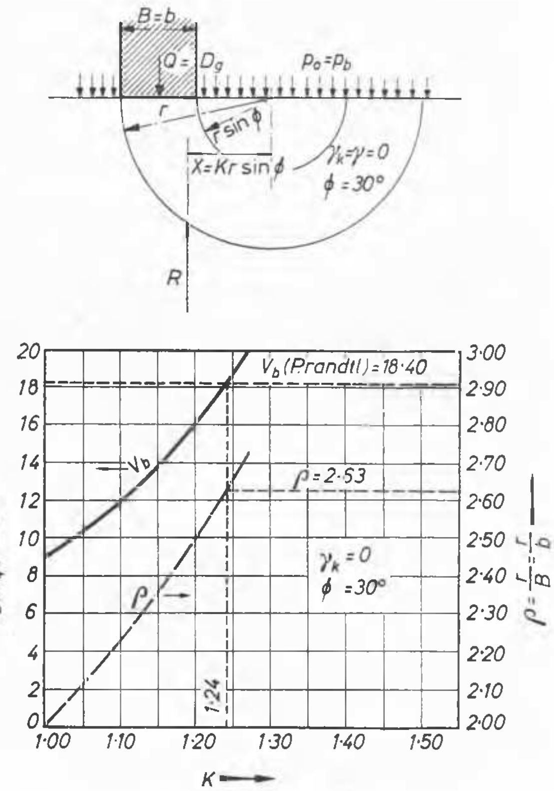

Fig. 19

found with the slip surface of Prandtl. As the circular surface is not the true slip surface, it must give for V_b a higher value than 18·4 indicating that the value of K is certainly higher than 1·24. The radius of the critical circle is larger than $r = \rho b = 2{\cdot}63b$. To each arbitrarily assumed distribution of the soil reactions along the circular slip surface corresponds a value of the factor K and, as is indicated in Fig. 19, of the bearing capacity factor V_b.

In Figs. 20 and 21 there are given respectively for $\phi = 30$ degrees as a function of the factor K, the values of the factor $\rho = r/b$ and the factor $d_g/\gamma_k b$, for different values of the factor $(P_b/\gamma_k b)$. From these figures it can be seen that the value of $d_g/\gamma_k b$ is very sensitive to the value of K; thus, when the method of the example of the Paper 3a/33 is followed, it is necessary to know exactly this value, and by taking $K = 1{\cdot}00$ a very big error on the safe side can be made.

It is therefore clear that the method followed in the example given in the paper is not to be mistaken with the method of Andersen. The conclusion that the results obtained are considerably on the safe side pertain to the method of P. E. Raes who arbitrarily adopted $K = 1$ in a method which differs from that of Andersen for the two reasons already given.

On the contrary, with the method of Andersen the bearing capacities obtained are equal or higher than those given by other methods. This is clearly shown: first, by the graphs of Figs. 17 and 18; secondly, by the fact that the unaltered method of Andersen for the case $P_b/\gamma_k b=\infty$ instead of the much lower real value ($V_b=18{\cdot}4$ for $\phi=30$ degrees); and thirdly, by the fact that in the case $\phi=0$ and $p_b=0$ the method of Andersen gives a bearing capacity factor $V_c=6{\cdot}28$ instead of the exact value 5·12.

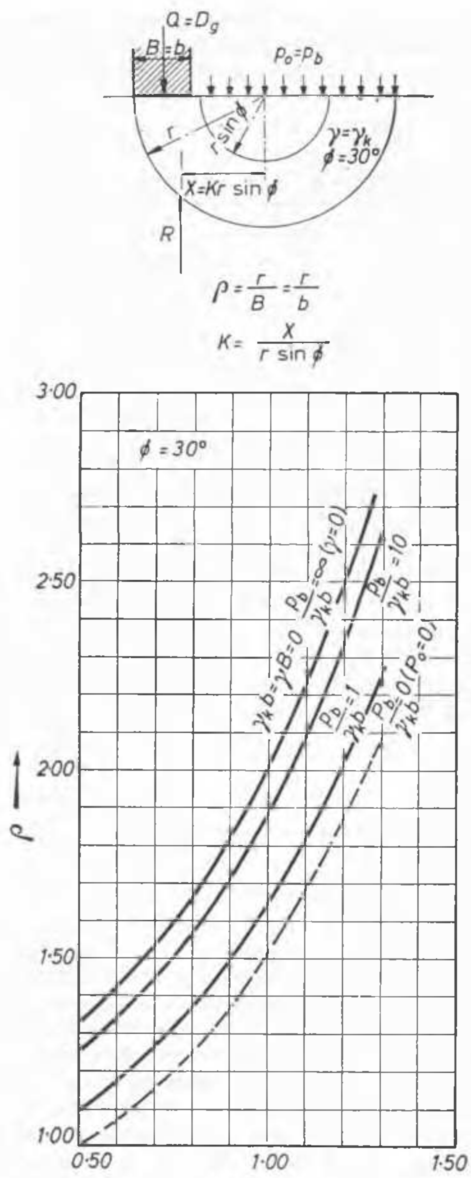

Fig. 20

As the method of Andersen gives higher values than the exact ones in the known extreme cases it may be expected that this will also hold in the intermediate cases, for which no exact solution exists. This is important for making the following reasoning. In the cases where the method of Andersen gives higher values than those obtained by other approximate methods, the results of Andersen should of course be discarded. In the cases where Andersen gives lower values than those obtained by other methods, the results of these other methods should, for reasons of safety, be rejected as they are higher than the values of Andersen which are already possibly too high. Thus, our aim is not to state that the method of Andersen gives the exact values, but that in a certain range it can be used to eliminate the results obtained by other methods.

With this comparative method we obtained the N_γ values indicated on Fig. 17. It appears that they are very close to those which have recently been published by Meyerhof and Caquot–Kérisel. Three different ways of approaching the problem now exist; as they give nearly the same results it may be stated that they support one another.

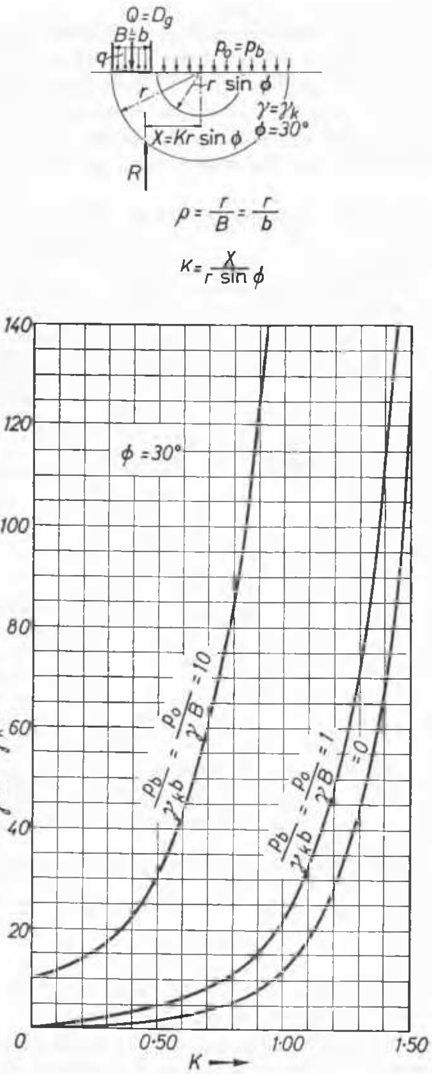

Fig. 21

G. F. Sowers (U.S.A.)

I should like to add some brief words of dissent with regard to the discussion on the bearing capacities of sands.

During the last ten years I have made plate load tests on some of the sand deposits in the south-eastern part of the U.S.A. Large areas of the States of North and South Carolina, Georgia, Florida, Alabama and Mississippi are underlain by deposits of clean, uniform sands. These sands are medium fine grained, angular quartz. They are largely beach deposits and terrace deposits of Pleistocene and recent age. Plate load tests were made using plates from 1 ft. square to 3 ft. square: the plates were free to rotate upon loading. At the time the tests were carried out undisturbed samples of the sands were secured and tested in the laboratory to determine their relative density and shear resistance. The relative densities ranged from 30 to 70 per cent, and the angles of internal friction from 33 to nearly 40 degrees. The bearing capacity was calculated from the shear test results using Terzaghi's general shear failure analysis. In all cases it was found that the computed bearing capacity was approximately 30 per cent greater than the measured bearing capacity.

A. Lazard (France)

M. le Rapporteur général ayant demandé une discussion au sujet des Facteurs de Sécurité, je voudrais souligner les deux points suivants:

(1) Au cours de la Conférence Européenne de Stabilité des Talus, il y a eu de nombreuses discussions sur ce sujet et je ne me suis pas trouvé d'accord avec beaucoup de participants. J'ai d'ailleurs exprimé mon opinion dans un article dans le revue française *Travaux*, Septembre 1955.

Je veux simplement noter que, de temps en temps, on trouve des exemples dans lesquels on ne divise pas C et tan ϕ par un unique coefficient F. J. Brinch Hansen a lui-même donné l'exemple dans son beau livre. Hier après midi, au cours de la visite du Laboratoire de l'Imperial College, A. W. Skempton nous a déclaré qu'il n'avait pu expliquer un glissement de talus dans la Région Londonienne qu'en admettant $C=0$ avec un ϕ constant: ce qui revient à prendre $F=\infty$ pour la cohésion et $F=1$ pour le frottement. L. Bjerrum en fait autant pour expliquer des glissements en Norvège. C'est aussi un peu la tendance de F. Walker dans son rapport général de la Division 6, quand il indique qu'aux U.S.A. il ne tient compte que du frottement pour construire une digue stable.

(2) Mon Directeur, Robert Lévi, qui est un spécialiste bien connu du calcul des marges de sécurité par le moyen des calculs probabilistes dans les constructions en Acier et en Béton, a récemment prononcé devant le Comité Français de Mécanique des Sols une Conférence relative à l'Introduction des Calculs Probabilistes en Mécanique des Sols. Il a demandé qu'on lui soumette une formule pour la traiter selon ses conceptions. A mon retour je compte lui donner une formule de N. Janbu (que nous discuterons Lundi) et j'espère que dans quelques mois un article pourra être publié dans *Géotechnique* faisant connaître aux mécaniciens des Sols des conceptions probabilistes entièrement nouvelles pour eux.

R. Pietkowski (Poland)

There is one further problem among the many problems associated with soil mechanics to which I should like to refer. It is the problem of temperature penetration in soils, and particularly in connection with soil heaving on roads. This problem has been examined by many scientists and I would particularly mention the work of Gunnar Beskow of Sweden. Secondly there is the question of freezing of foundations under cold storage plants; this was dealt with by Stefan of Vienna and more fully by Ruckli of Switzerland in his book. It might be added that some special cases such as the freezing of ground under skating rink foundations have been illustrated in my paper (3a/30). Thirdly, the problem of temperature penetration in soil arises in the case of heating of soils under certain buildings in which industrial plants are housed.

In my consulting practice I have encountered all these problems, and as I have not been able to find any answers in existing literature relating to all cases of soil conditions it has been necessary for me to examine every case myself.

The problems of soil freezing and heating are complicated by changes in the state of water, by the very different rate of heat capacity of the soil mineral skeleton and of the water. For instance, the change of temperature of 1° C. of 1 kg of dry sand with porosity of 35 per cent will require about 0·15 kcal. The same change of 1 kg of water requires 1 kcal, and a change of 1 kg of ice into water requires 80 kcal. A change of 1 kg of water into vapour requires 539 kcal. The variety of these figures illustrates, from a thermodynamic point of view, the influence of water content and of its changes of state. One can readily appreciate that the water present requires large quantities of heat and makes the processes of heat penetration significantly slower. In nature quite different processes are found in dry soil, wet soil and in soil with steady capillary supply of water during freezing. Nevertheless, it is possible to examine all these problems in terms of figures and functions of time. The question of soil heaving under skating rink foundations, which is one danger, is dealt with in my paper, and is examined on the basis of Fourier's law well known in thermodynamics. The full work treating on various cases of thermal penetration in soils will be published in Polish together, I hope, with an English summary. I have devoted myself to these problems with great interest for many years, and I take the liberty of recommending this topic for study by others. There is still much work to be done.

R. H. G. Parry (U.K.)

The use of standard loads in the oedometer may lead to an error in settlement calculations.

When a clay is deposited and consolidated it follows line AB on the voids ratio–consolidation pressure plot, the pressure scale being logarithmic. If uplift and erosion follow it moves from B to C which is the condition at which the engineer finds it. Further loading causes it to move along curve CD.

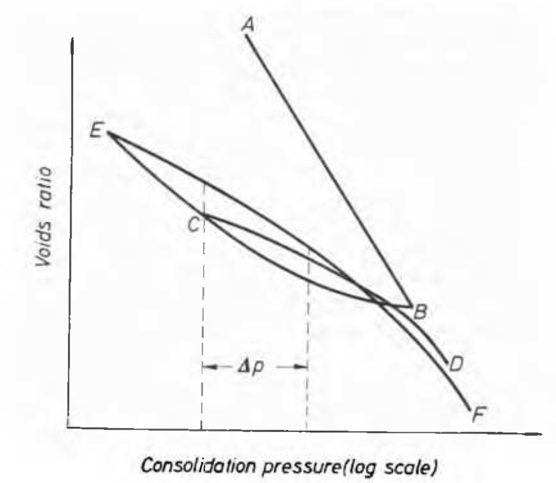

Fig. 22

In the laboratory, however, a standard first load is often used (a common value is ¼ ton/sq. ft.). The sample, under this load, moves to point E if it has access to free water. Further loading causes it to move along curve EF. The coefficient of compressibility, M_v, is taken from the curve EF_1 based on a selected load increment Δp. In fact the value of M_v should be taken from curve CD.

The application of standard loads, then, as used in many laboratories is likely to lead to an over-estimation of consolidation for over-consolidated clays and, perhaps, an under-estimation for normally consolidated clays. The first load should normally be equivalent to the effective overburden pressure.

P. W. Rowe (U.K.)

I agree with the General Reporter's views regarding the use of safety factors with the following reservations in the case of composite structures and deformation problems. For example, in Paper 3a/35 the steel frame failed before the foundations, which were pulled subsequently to failure. If smaller foundations had been used in order to ensure simultaneous composite failure, the greater degree of angular rotation associated with the soil strains at failure would have led to larger moments in the steel structure. It does not follow that standard factors of safety applied to the soil and steel respectively will necessarily lead to economical design and this appears to be a deformation problem.

It is well known that the ultimate bearing capacity of foundations on sand increases with foundation width whereas the allowable bearing pressure must be decreased in order to control the settlement. Deformation controls design for foundations wider than $2\frac{1}{2}$ ft. and this is a case where a safety factor applied to the limiting shear strength has little meaning.

T-K. Tan (China)

The measurement of the 'elastic modulus' $E(t)$ which is a function of time is complicated. The measurement of the coefficient of lateral expansion $\nu(t)$ is much more intricate, as in addition to the increasing time effect of lateral pressure also the decreasing time-function due to consolidation will be measured. The measurements have been performed in a new type oedometer in which the ring is elastically supported, and in which it is possible to measure lateral stresses as a function of time.

The curves have the following tendency:

(1) First decreasing until a certain minimum and then increasing again, approaching a certain limit, which however has not been measured accurately, as the tests have only been

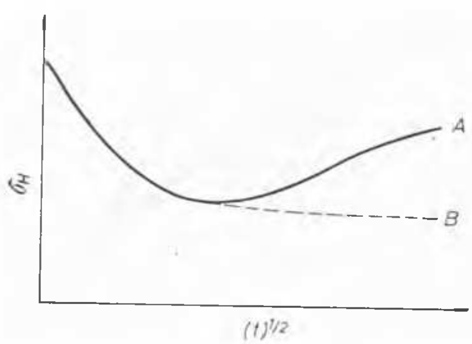

Fig. 23

carried out for about one week. The first part of the curve is linear with $(t)^{\frac{1}{2}}$, which is in agreement with the prediction from my theory, as shown by curve A in Fig. 23.

(2) It has been observed that the minimum occurs after the 'primary hydrodynamic period' has been passed.

According to G. P. Tschebotarioff an ultimate neutral rest-value will occur; this tendency, however, I have observed only for high permeable clays with high water content within the short period of a few days, as in curve B.

This experimental phenomenon throws a completely different light on the neutral earth pressure against bulkheads, retaining walls, etc., and shows clearly the effect of lateral creep or flow, which until now has been completely ignored in the current theories. This phenomenon shows that the deviatoric stresses after application of the load are increasing (due to consolidation) and gradually decrease due to relaxation under unconfined compression.

A. Vesić (Yugoslavia)

In discussing Paper 3a/9 by H. Grasshoff, J. Feld left us with the impression that the design of foundations with the assumption of a uniform distribution of pressure should be always safe and economical.

We can agree to a certain extent that such an assumption is on the safe side, but we cannot agree that it is also always economical. If we have, for instance, a foundation raft supporting a very rigid superstructure (Fig. 24) then with a distribution of pressures obtained after H. Grasshoff (curve a)

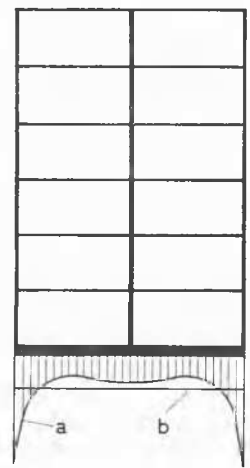

Fig. 24 Distribution of pressures under a foundation supporting very rigid superstructure (curve a). Curve b shows the corresponding uniform distribution

Répartition des pressions sous une fondation supportant superstructure trés rigide (courbe a). La courbe b représente la répartition uniforme correspondante

the bending moments are much reduced compared to those computed with the assumption of uniform distribution of pressures (curve b). A considerable economy of reinforcing steel can be obtained assuming non-uniform distribution.

This non-uniform distribution is introduced only for the design of structural members of the foundation under working conditions, and has nothing to do with the estimation of bearing capacity. The latter is based on the considerations in the plastic range, for which the assumed distribution curve is of quite another shape.

General Reporter

When listening to this important and interesting discussion today I was reminded of the words of a famous physicist who, some years ago when wave mechanics were being developed, said: 'It is a deplorable fact that physics is becoming too difficult for physicists'! I have the impression that a similar thing is happening here, and that soil mechanics now are becoming too difficult for soil engineers! Under varying site conditions we find wide disagreement between various measurements, which is, perhaps, not surprising; but what is surprising is that we find similar discrepancies in tests made under controlled laboratory conditions. Of course, we have eventually to reach agreement between test results if soil mechanics is to deserve the title of a Science, and I think we shall reach that agreement, although it may take a few more conferences to do so. On account of all this, I think that I may be excused if I do not attempt to sum up the discussion in any way.

K. Terzaghi, President (U.S.A.)

The subject of this session has been most thoroughly and expertly covered in the general report by J. Brinch Hansen, and that applies also to his general report on the second part of the discussion dealing with foundations. Therefore, all I can do to make some contribution to the subject is to describe a few incidents and observations which have been instrumental in helping me to form my own attitude towards foundation problems in general.

Many years ago when I still considered loading tests on bearing plates an essential part of subsoil exploration at the site for foundations, I had to design the foundations for an apartment house. The area to be occupied by the building had the shape of the letter U. The subsoil consisted of sand, sandy gravel and sand mixed with some silt, and it appeared to be relatively homogeneous; therefore I decided to establish the structure on spread footings. The design was preceded by six exploratory borings and four loading tests performed at the level of the base of the footings. Because both the test borings and the loading tests furnished fairly identical results I felt justified in assuming that the subsoil was homogeneous in all horizontal directions. The settlement observations on the finished structure showed that my assumption was justified for the whole area except for one corner footing which settled 2 in. more than the others and the shear cracks caused by the excess subsidence of this footing were an ugly sight. Since that mishap I always took great pains to find out prior to designing a footing foundation the location of the softest spots within the area to be occupied by the structure. In connection with these efforts I learned to appreciate the standard penetration test.

The standard penetration test was mentioned repeatedly during this conference—sometimes in a complimentary and sometimes in a less complimentary vein. Therefore it may be appropriate to say a few words about the role which I assign to this test in my own practice.

I consider the test merely as a means for obtaining preliminary information concerning the degree of homogeneity of the subsoil of structures to be established on sand and as a basis for estimating the upper limiting value for the settlement of the footings. The next step depends on economic considerations. If the building is relatively small it is more economical to design the footings on the basis of the upper limiting value of the settlements than to make further investigations. On the other hand, if the structure is large and the loads to be carried by the footings are heavy it is indicated to supplement the results of the interpretation of the penetration data by loading tests to be performed in those locations where the standard penetration tests revealed the presence of the loosest and the densest portions of the subsoil.

If the footings of a building are to be established on clay the data required for the design of the footings should be secured from the results of soil tests on Shelby tube samples. The design of the foundation for the Shamrock Hotel in Houston, Texas, is an example. The hotel rests on a stratum of stiff clay with great depth. The layout of the building called for establishing the base of the footings at depths ranging between about 13 and 30 ft. and within this range of depth the unconfined compressive strength of the clay increased more or less erratically from 2·1 to 5·0 tons/sq. ft. The number of footings was 250 and their size ranged between 5×5 and 20×20 ft.

In order to derive any benefit from a theoretical settlement investigation it would have been necessary to determine the subsoil condition for each one of these footings and the time and expenditure involved in such an operation would have been prohibitive. Therefore I assigned to the 'allowable soil pressure' such a value that the settlement of the largest footing will not exceed the maximum tolerable differential settlement. Thus the problem was reduced to computing the settlement of the largest footing and this was done on the assumption that the subsoil conditions at the site of the footing were identical with the most unfavourable ones disclosed by the boring results.

A third problem which has required my attention from time to time is the forecast of the topography of the bowl of subsidence produced by the consolidation of clay strata due to local lowering of the water table by pumping. This problem presents itself in connection with the design of the foundations for pulp and paper mills.

The operation of such mills requires large quantities of water. If the water is obtained by pumping from deep-seated aquifers, the ground surface located above the bowl-shaped depression in the piezometric surface of the aquifer subsides. The ratio between the surface subsidence and the corresponding lowering of the piezometric surface is roughly the same at every point of the subsiding area.

For economic reasons the centre of gravity of the area occupied by the well system is located as close to the mill as conditions permit. Therefore the mill buildings are located on the slope of the bowl of depression produced by draining the subsoil. Yet a slight tilt of the base of the buildings containing part of the mechanized equipment may interfere with the operation of the machinery. Therefore it is necessary to estimate in advance the maximum slope of the sides of the bowl of subsidence and to adapt the layout of the well system to the findings. The computation of the subsidence is inevitably based on the simplifying assumption that the clay stratum subject to consolidation is homogeneous.

A few years ago I faced a problem of this kind in southern Texas. The water supply system consisted of two rows of deep wells. The aquifers were located between the depths of 600 ft. and 1,000 ft. and were separated from each other and overlain by beds of stiff clay. The boring records indicated that the clay strata were fairly homogeneous. In the same geological province there were similar water supply systems which had been in operation for 20 years and detailed records had been kept of the resulting surface subsidence. In order to get information regarding the errors involved in assuming, in my subsidence computation, that the clay strata were homogeneous, I consulted the available records, with the following results. The curves of equal subsidence had no more than a remote resemblance to the theoretical curves, and the maximum slope of the sides of the bowl of subsidence was up to six times as great as the slope computed on the basis of assuming that the clay strata are homogeneous.

On account of the uncertainties involved in theoretical settlement forecasts of any kind, I am happy to learn from the papers which have been presented at our conference that the collection of case records is being rigorously pursued.

N. Janbu (Norway)

The results of a large number of comprehensive full-scale tests for determining the limit overturning moment of isolated foundations have been reported by A. Lazard in Paper 3a/19. Furthermore, the stability of free and tied piers has been studied both theoretically and experimentally by K. A. Roscoe in Paper 3a/35.

In this connection it is believed to be of supplementary interest to give a brief account of some model tests on free mast foundations carried out at the Norwegian Geotechnical Institute during the spring of 1957.

Several model mast foundations in dry, uniform, fine sand were tested to failure by tilting produced by a gradually increasing horizontal force P. The dimensions of the foundation, the relative density of the sand and the depth-width ratio D_f/B were varied appreciably. For the interpretation of the test results a simple working hypothesis for determining the limit moment was adopted, Fig. 25.

If A is the point of application of the resultant foundation pressure at failure, moment equilibrium requires, when utilizing the notations defined in Fig. 25,

$$P(H+D_f) = bGB+m\gamma D_f^3L \qquad \ldots. \ (1)$$

in which $b \leqslant 0{\cdot}5$.

This simple equation shows that the external moment $P(H+D_f)$ is resisted by two components, one due to weight bGB and one due to earth pressure $m\gamma D_f^3L$. Equilibrium in horizontal and vertical directions, which would require a closed force polygon in Fig. 25a, will not be discussed in detail because of its minor importance.

The model tests were carried out for the purpose of determining the dimensionless factors b and m. The tests have so far been carried out in the laboratory for dense and loose sand, corresponding to average relative densities of roughly 80 and 25 per cent respectively. The results of a few supplementary field tests on a much larger scale are also available.

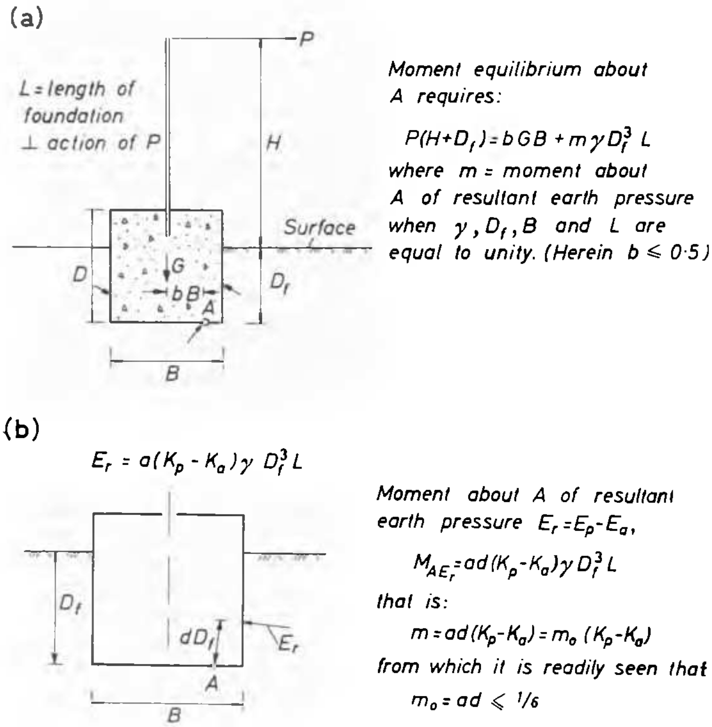

Fig. 25 Illustration of the definition of m and m_0. (a) Working hypothesis; (b) notation and derivation

Illustration de la définition de m et m_0. (a) Hypothese de travail; (b) notation et dérivation

The test results are given in Fig. 26 on a dimensionless scale as obtained when dividing equation 1 by GB,

$$\frac{P(H+D_f)}{GB} = b+m\frac{\gamma D_f^3L}{GB} \qquad \ldots. \ (1a)$$

This shows that the intercept at the ordinate in Fig. 26 is equal to b, while the inclination of the straight lines drawn through the observation points define m. From Fig. 26 it is quite apparent that the relative density of the sand has a considerable influence on the value of m (i.e. on the resisting earth pressure moment) but only a very small influence on b (i.e. on the resisting moment due to weight). On the basis of the test results tentative values of b and m are suggested for sand, as shown in the table:

Tentative values of b, m and m_0 (equation 2)

Relative density	*Approx.* $\phi°$	b	m	m_0	*Remarks*
Loose	33±	0·40	0·5–1·0	0·075	
Medium	39±	0·45	1·5–2·0	0·100	$\gamma D_f^3L \geqq 0{\cdot}2GB$
Dense	45±	0·50	2·5–3·0	0·125	

For comparison, it is observed that the model test on a free, rough, square pier, as described by K. A. Roscoe in Paper 3a/35, leads to a value of m of about 1·5, which corresponds to a medium dense sand according to the table of tentative values. This is a very interesting supplementary test result, since the important parameter $\gamma D_f^3L/GB$ must be about 10 in Roscoe's test while the maximum value of this parameter in Fig. 26 is just 3·2.

The most important value in equation 1 is m, for which one can derive a very simple formula on the basis of the idealized conditions shown in Fig. 25a

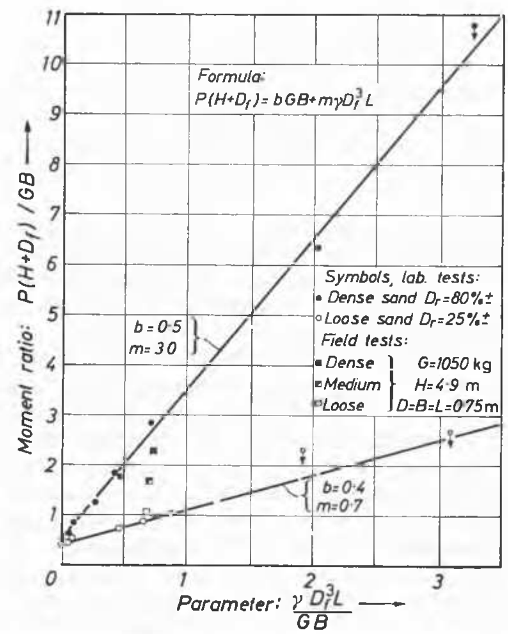

Fig. 26 Test results

Résultats des essais

$$m = m_0(K_p-K_a) \qquad \ldots. \ (2)$$

From classical earth pressure theory it is readily seen that $m_0 = ad \leqslant \frac{1}{6}$ for a linear earth pressure distribution with depth. However, assuming rough ($r=1$) walls the test results indicated a somewhat smaller value of m_0 as was expected due to the non-linear pressure distribution. It is also apparent that the m_0 value depends to a certain degree on the relative density, and possibly also on the D_f/B ratio. Tentative values of m_0 as interpreted from the tests are given in the table.

On the basis of deformation measurements it was possible to observe that the centre of rotation moved considerably during each test, and usually in a different fashion for the various relative densities.

The test series are being continued to enable a more detailed study of the parameters b, m and m_0. The desirable aim would be to obtain a continuous variation of b and m as functions of ϕ, and other typical characteristics which may influence the values of these factors.