Earth Pressure on Structures and Tunnels

Poussée des Terres sur les Ouvrages et Tunnels

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The Chairman

I hope that you have come back refreshed by your week-end and ready to bring this conference to a conclusion which will be in conformity with the splendid progress made last week.

I now call on the General Reporter to present a summary of his report.

General Reporter

Monsieur le président, mesdames, messieurs, les points particuliers sur lesquels je voudrais revenir au début de cette séance de travail sont les suivants:

On commence aujourd'hui à avoir de sérieuses données concernant la grandeur des contraintes de butée ou de poussée derrière ou contre les ouvrages et on vérifie que ceux-ci sont en assez bon accord avec les différentes théories plastiques, mais à une condition essentielle; c'est que les ouvrages permettent des déplacements suffisants.

Les expériences fondamentales du Karl Terzaghi, en 1934 sur la poussée et celles du G. P. Tschebotarioff et de M. Johnson, en 1954, sur la butée, montrent que dans le premier cas ces déplacements doivent être de quelques millièmes et dans le deuxième cas de quelques centièmes de la hauteur du mur en contact, ceci n'étant qu'un ordre de grandeur pour le cas particulier des translations parallèles et des massifs de sable.

Mais, bien souvent, les ouvrages ne permettent que partiellement ou pas du tout le développement de ces déplacements, et d'autrepart les massifs argileux interviennent avec des propriétés afférentes à leur viscosité et leur rhéologie.

Il en résulte que tout l'art de l'ingénieur des sols revient à prévoir, en fonction de la conception, plus ou moins rigide des ouvrages, quels sont les efforts réels en fonction de déformations limitées et aussi, comment ces efforts évolueront dans le temps.

Les contributions expérimentales apportées à ce congrès sous la rubrique de la session 5 marquent un pas en avant dans le domaine essentiel de la mécanique des sols.

Je voudrais à cet égard citer, parmi d'autres, celle du P. W.

Rowe sur les palplanches, du L. Bjerrum sur les fouilles coffrées, celles du K. S. Lane sur les tunnels et, enfin, les observations du A. W. Skempton sur le comportement à long terme des argiles surconsolidées derrière les murs de soutènement.

On arrive ainsi à concevoir la cohésion et le frottement non plus comme deux paramètres indépendants mais comme deux variables associées pouvant prendre dans le temps une série de couples de valeurs.

Je souhaite que, pour répondre aux point 2 et 3 de discussion suggérés dans mon rapport, nous soient apportés à cette tribune un grand nombre de résultats expérimentaux qui nous donnent à la fois les deux aspects, contraintes et déformations, observés *in situ*.

Nous sommes efforcés, avec votre président de séance et avec W. H. Ward, pour clarifier la discussion, de sérier les problèmes en adoptant l'ordre suivant: tunnels, palplanches, silos et théorie générale.

Puis-je, par ailleurs, me permettre une suggestion?

Dans les diverses communications, j'ai vu apparaitre, pour caractériser les caractéristiques physiques de la cohésion frottement et leur évolution derrière les ouvrages, les notations les plus variées:

$$\begin{array}{ll} C' - \phi' & C_u - \phi_u \\ C_e - \phi_{ce} & C_{cu} - \phi_{cu} \\ C_r - \phi_r & C_d - \phi_d \end{array}$$

Je suis sûr que les divers interpellateurs conçoivent beaucoup plus clairement leurs notations que certains des membres de ce congrès et je pense qu'il serait bon qu'ils précisent la signification de leurs notations au cours de leurs interventions afin que nous puissions en retirer le meilleur bénéfice.

Enfin, dernier point, depuis mon arrivée à Londres, j'ai pu prendre connaissance des récentes publications du V. V. Sokolovsky. Elles constituent une très importante contribution aux problèmes que nous traitons aujourd'hui et notamment à la théorie des poussées et butées. Je tenais à réparer cette lacune de mon rapport général.

H. B. Sutherland (U.K.)

Two papers have been presented to the conference relating to the measurement of stresses in tunnel linings. Paper 5/7 by K. S. Lane deals with the effect of lining stiffness on tunnel loading while Paper 5/13 by W. H. Ward and T. K. Chaplin is concerned with tunnels in London clay.

Measurements are being made in a tunnel in Scotland under somewhat different conditions from those described in these two papers. The tunnel has recently been constructed under the River Clyde. It is 12 ft. in diameter and constructed of cast-iron segments. There is about 40 ft. of sand above the tunnel. When planning these observations we had hopes that we should be able to get a condition whereby the tunnel was completely surrounded by sand, since there are few, if any, readings of tunnel pressures through sand strata. Unfortunately the preliminary bore holes could not be located as the engineers desired, due to a navigable channel. The tunnel has 40 ft. of sand above it and that sand extends over parts of the face, but the lower part of the tunnel is located in shale.

The tunnel was driven using a shield, and compressed air up to a maximum pressure of 40 lb./sq. in. was required. Sixty vibrating wire strain gauges were inserted in the cast-iron segments at the surface; these segments were then lowered through the vertical air-lock into the shaft and built in compressed air. We have managed to measure the changes in stress as the air pressure was dropped in steps to zero pressure. The corresponding changes in the tunnel diameter have also been measured, using a 12 ft. stick with a micrometer head, so that we have a full-scale loading test on the tunnel brought about by the drop of compressed air.

The robustness and sensitivity of vibrating wire strain gauges has been well proved during this investigation. They can be read to the order of 15 to 20 lb./sq. in. in cast-iron. These gauges were subjected to some rough treatment in the construction of the tunnel, as can well be imagined, under these conditions, but so far 59 out of the 60 are still in operation, and quite a number of these are now embedded in the concrete and still work quite satisfactorily.

The vibrating wire principle has also been applied to the design of a pressure gauge to measure the variation in pressure of the water immediately outside the tunnel lining. We have two loading conditions which constitute loading tests, namely the dropping of compressed air and the stress reaction to that condition, and also the variation in stress as the tide varies. We are now measuring the pore water pressure immediately outside the tunnel lining to find the variation in pore pressure in the sand which is causing the stress variation which is found.

The pore pressure gauge which has been developed for this purpose works on the vibrating wire principle. First of all we had normal Bourdon gauges, but it was very difficult to read these at intervals; it meant climbing in and out of the tunnel, a matter of 100 ft., each time, and so we devised a gauge to fit into our general observation system with the vibrating wire gauges. The principle is to feed the water through the tunnel lining into a chamber at the end of which is a diaphragm. Connected to the diaphragm is a vibrating wire, and the deflection of the diaphragm produces a change in frequency of the vibrating wire. The sensitivity of the gauge is of the order of 1/40 or 1/50 lb./sq. in. We have also installed a pressure release valve to check the zero of the gauge. We find the zero, close the release valve and build up the pressure again, and so we have a constant check on the zero to check any possibility of drift.

Those are a few of the points which we are covering in the investigation. The measurements and analysis are still continuing.

V. Mencl (Czechoslovakia)

Permettez-moi de discuter la communication 5/5 de B. Kujundžić, sur l'anisotropie des massifs rocheux. B. Kujundžić a trouvé que sous la pression de l'éau ou celle de vérins plats, les déformations des diamètres de galerie sont les plus grands dans la direction verticale, tandis que dans la direction horizontale elles sont moins accentuées alors que dans la direction sous un angle de 45 degrés elles sont encore moindres (Fig. 1). Il explique le phénomène par le fait que le rocher a une stratification horizontale.

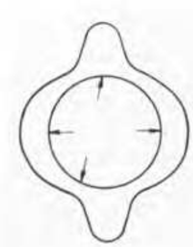


Fig. 1

Permettez-moi de vous communiquer que nous avons aussi observé le même phénomène dans les rochers sans stratification et qu'on peut aussi l'expliquer d'une autre manière.

Sous les forces verticales de pesanteur une voûte se forme dans le rocher. Cette voûte est compressible et je la dessine comme un ressort (Fig. 2). C'est parce qu'elle est compressible qu'on peut expliquer qu'il se forme au dessus de la galerie une zone de traction, ou du moins une zone sous tension.

Aux reins cette voûte est plus proche de la surface du rocher. Mais il y a aussi une pression horizontale dans le rocher, 'pressure at rest', pour laquelle le phénomène est le même mais avec une rotation de 90 degrès et avec une intensité plus faible.

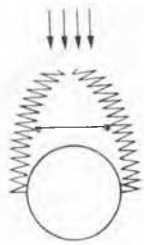


Fig. 2

Les résultat de ces deux phénomènes est que la voûte dans le rocher s'approche de la surface de la galerie aux points *a* (Fig. 3), tandis que dans la direction verticale et la direction horizontale, il y a des zones décomprimées. Je crois que chaque ingénieur qui a travaillé à la construction des tunnels

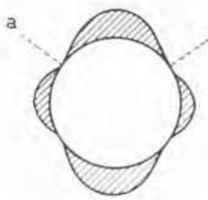


Fig. 3

peut confirmer que c'est dans les point *a* que l'on obtient toujours les plus grandes pressions s'il s'agit de rochers mauvais, ou le phénomène de 'bergschlag' s'il s'agit des rochers fragiles.

Je crois que cette répartition des zones décomprimées autour de la galerie joue aussi un rôle important dans les phénomènes trouvés par B. Kujundžić.

J. L. Serafim (Portugal)

In the part dealing with tunnels in the General Report to Division 5 there is no mention of phenomena which we consider to be very important—the internal stresses in rock and the rock pressure against tunnel linings and roofs of underground powerhouses.

The knowledge of rock pressure seems to be very important: for instance in tunnels used as penstocks the pressure of rock against the tunnel linings can take a great part of the water pressure, and probably explains the good behaviour of many non-reinforced concrete linings. The internal stresses in rock can also explain explosions which are often observed in hard-rocks. Most of the theories used in the design of linings of tunnels assume that stresses or pressures are due to the weight of the rock; however this generalized assumption is probably not correct, because in many cases stresses much greater have been observed. Mining engineers and geologists attribute high internal stresses mainly to tectonic forces.

For the measurement of existing stresses in deep rock in Portugal the strain relief method has been used by drilling cores in the surface of the caverns; strain measurement being made by means of electrical strain gauges. For checking the accuracy of this method big prisms of granite from the site were loaded in testing machines and cores were drilled with diamond drills. SR-4 strain gauges outside and inside the base of the cores were used (Figs. 4 and 5). Measurements by this method were made in a tunnel in granite at Caniçada, in the surge chamber of the powerhouse; the results observed at various points are shown in Fig. 6. From these measurements it can be seen that the vertical stresses are, on the average, about twice as big as the horizontal stresses. This means that before the tunnel excavation the field of stress was hydrostatic, because when a circular tunnel is driven in an infinite body subjected to a hydrostatic field of stress, a factor of stress concentration of two is obtained.

In the case of Picote powerhouse a rather different method was used. Instead of cores being drilled, samples of rock with strain gauges fixed on to their surface were taken out of the rock manually, this being easier and cheaper, and avoiding heating and wetting of the gauges.

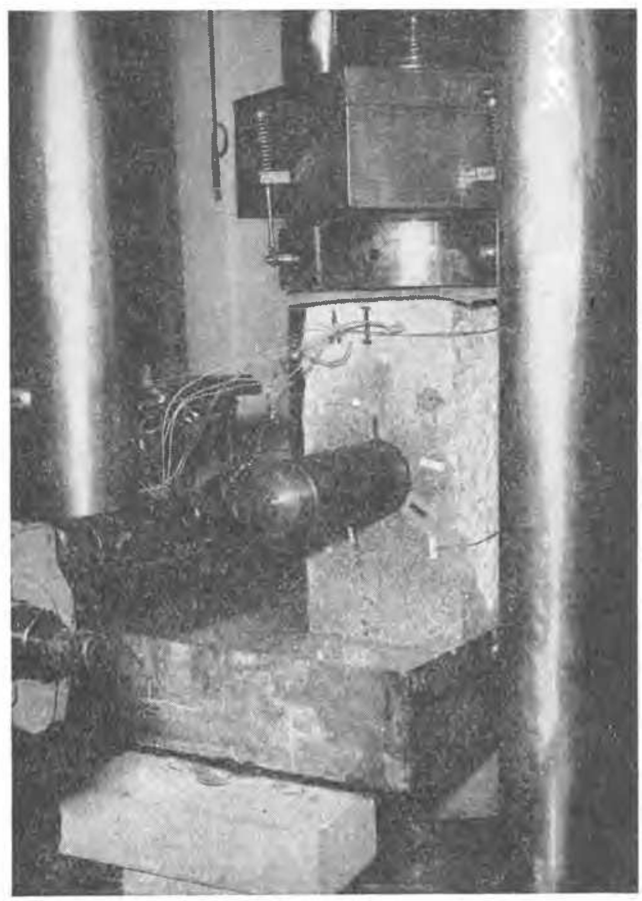


Fig. 4 Drilling a core in a prism
Echantillonnage au moyen d'une sondeuse à carottage

Here the English foil gauges (Saunders Roe) were used (Fig. 7). They proved to be very stable even in a humid atmosphere when fixed and waterproofed with epoxy cement. In the case of this powerhouse some interesting results were observed. After the excavation for the roof was completed, the stresses in the rock were measured. On the downstream side, the average of the observed stresses was 92 in the vertical direction and 52 in the horizontal direction, but on the upstream side stresses as high as 400 kg/cm^2 were observed (Fig. 8), the average being about this was attributed to the presence of a vertical fault near the cavern.

In the meantime the roof and the beams for the crane were concreted and two rails were fixed in these beams. After excavation of the lower part of the powerhouse, a movement of these rails was noticed, 1 cm in one rail and 3 cm in the other.

Strain meters were also placed inside the concrete and important strains were also observed after the excavation

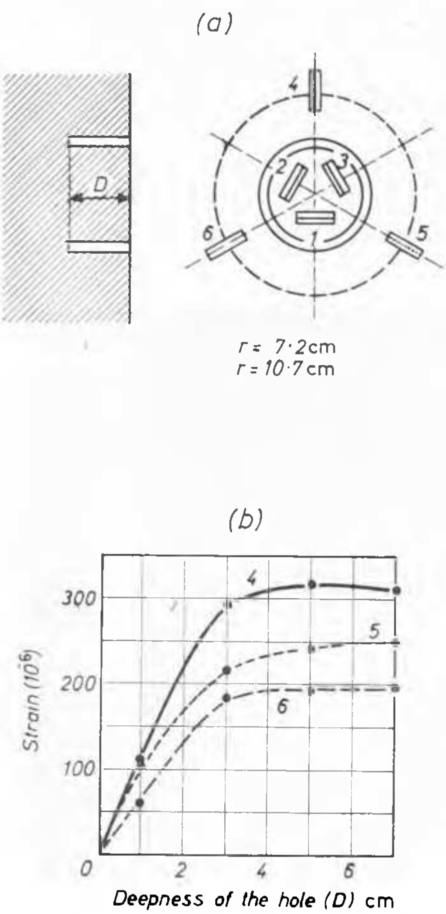


Fig. 5 Results from drilling and inserting strain gauges: (a) position of strain gauges; (b) evolution of strains in hole No. 2

Résultats du carottage et du placement des extensomètres: (a) position des extensomètres; (b) evolution des deformations dans le trou No. 2

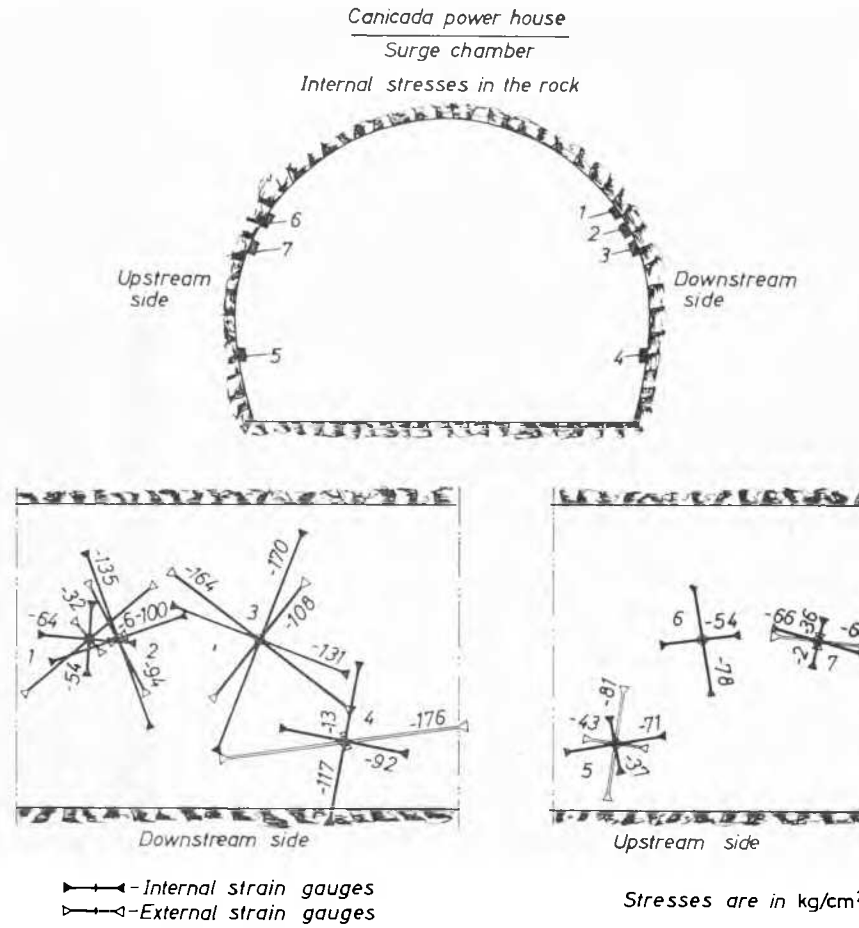


Fig. 6 Internal stresses measured in the rock of surge chamber of the Caniçada powerhouse

Deformations internes mesurées dans une chambre d'evacuation de la station hydroélectrique de Caniçada

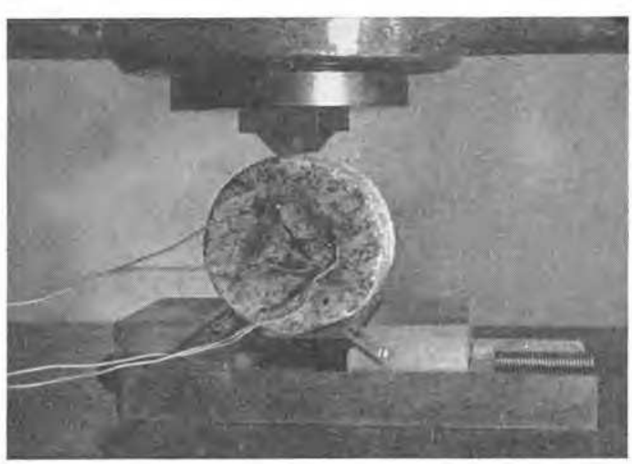


Fig. 7 Foil gauges in a sample of granite taken from the rock of Picote powerhouse

Extensomètres placés sur un échantillon de granit provenant de la roche de la centrale électrique de Picote

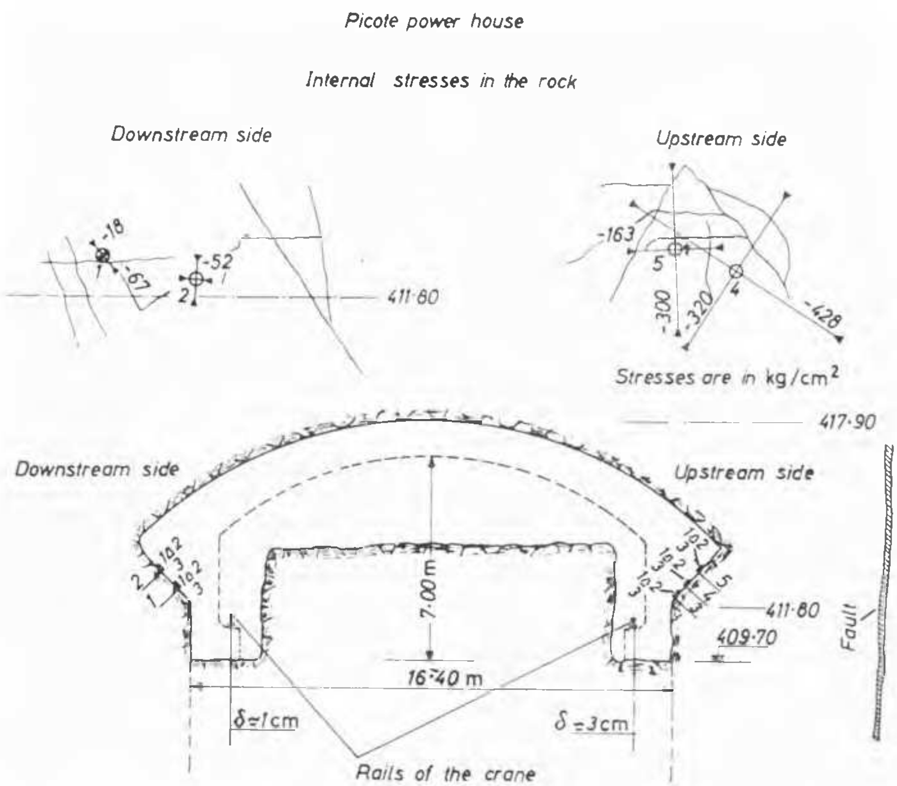


Fig. 8 Internal stresses measured in the rock of Picote powerhouse
Contraintes internes mesurées dans la roche de la centrale électrique de Picote

(Fig. 9). They proved to be in agreement with the measurement of the stresses in the rock and the measurement of the displacement of the rails for the crane. These results prove

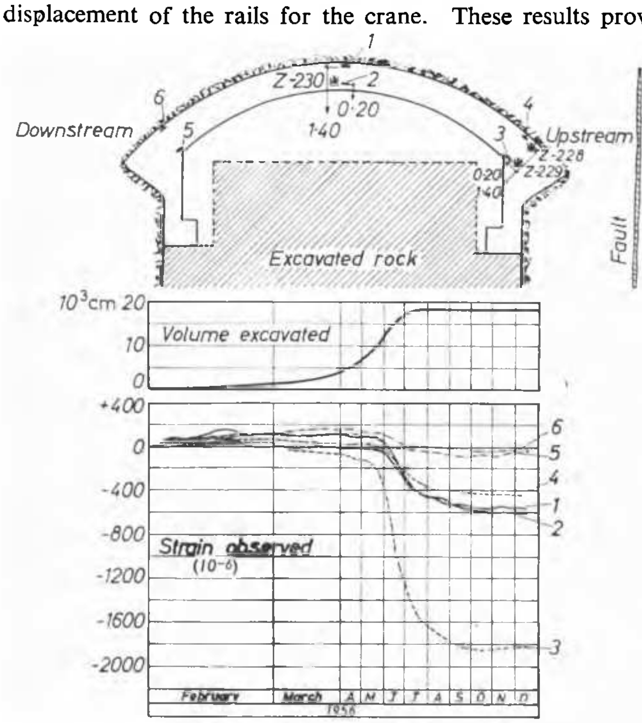


Fig. 9 Strains in the concrete roof of Picote powerhouse
Deformations de la toiture en béton de la centrale electrique de Picote

that it is becoming necessary, for the interpretation of the phenomena of rock pressure and internal stresses in rock, to make many other similar measurements.

D. Lazarević (Yugoslavia)

Monsieur le président, mesdames, messieurs, les déformations des massifs en roches, comme conséquences des variations de température, provoquent quelquefois des contraintes d'une telle grandeur qu'il faut les classer comme des contraintes primaires. Ce domaine de déformation, malheureusement, n'est pas exploré dans une mesure désirable, car il manque spécialement de mesures expérimentales.

L'institut hydrotechnique de Belgrade a consacré son attention à ce problème. Au point de vue théorique, il a été mis au point par des spécialistes de la thermodynamique. Les essais sur place sont en préparation. Le but des essais consiste à obtenir des caractéristiques mécaniques nécessaires pour le contrôle des procédés habituels d'analyse des tensions des revêtements, provenant de la variation de température des eaux circulantes dans les galeries en charge.

Le parallélisme mécanique nous semble possible au point de vue de déformation entre l'anisotropie et l'anisothermie, mais, heureusement, en sens inverse: les schistes de bonne conductibilité se déforment vite et ils ont de grandes déformations sous l'action de la température, mais ils subissent de petites déformations sous l'action de la pression hydrostatique sur les parois de la galerie en charge.

Les méthodes habituelles de calcul ne tiennent compte, ni du phénomène d'anisotropie ni d'anisothermie.

Le nombre de phénomènes d'influence funeste est très grand, spécialement dans le cas de calculs statiques des revêtements de galeries en charge. Pour cette raison on doit chercher une explication rationnelle sur l'imperméabilité des revêtements de galeries en fonction. Il nous semble prudent, non d'abandonner les recherches expérimentales des phénomènes présentant des inconvénients mais à consacrer plus d'attention aux phénomènes favorables. Le soin des constructeurs d'augmenter la sécurité des objets cache parfois les voies qui conduisent au but: la mise en lumière des phénomènes complexes dans leur totalité. Pour le calcul des revêtements l'anisotropie et l'anisothermie donnent les contraintes additionnelles qui augmentent les contraintes calculées par les méthodes courantes. L'étanchéité des revêtements étant vérifiée et reconnue satisfaisante, nous ne sommes pas obligé, contrairement aux prévisions des calculs, d'abandonner l'exploration des phénomènes défavorables dont l'existence est confirmée par l'expérience. Nous croyons qu'il est utile d'examiner de plus près le béton de revêtement et son état complexe des tensions, surtout au point de vue du gonflement du béton dans les conditions données. Dans beaucoup de cas le gonflement de la roche même est exceptionnellement grand. Tous ces gonflements provoquent des précontraintes naturelles intensives dans les revêtements. Ces précontraintes de pression absorbent les contraintes d'extension, provenant des déformations des roches sous l'action de la pression hydrostatique d'eau, des variations de température et des moments fléchissants qui sont la conséquence de leur anisotropie et anisothermie.

Dans quelques cas de roches exceptionnellement mauvaises nous avons constaté une tendance à refermer sous la pression géologique, l'ouverture percée de la galerie, en exerçant des pressions extérieures quasi-hydrostatiques très intensives. Dans les zones de galeries en matériel caractérisé comme 'bonne roche, dure et dense' nous avons eu de très grandes pertes de charges et dans les zones de roches de Verfene, presque molles, les revêtements en blocs préfabriqués en béton maigre et une membrane de gunite légèrement armée nous ont donné des résultats extraordinaires. Dans ces parties de galerie la consommation de ciment en injections de contact et de stabilisation a été extrêmement petite. La pression hydrostatique des essais de galerie en charge est montée jusqu'à 8 kg/cm^2.

F. J. M. de Reeper (Netherlands)

In Paper 5/7 K. S. Lane describes tunnelling in clay shale and compares the load on a flexible lining with that on a stiff one. The shortening of the vertical diameter of the flexible lining was about four times that of the stiff one. This means that the vertical load borne by the flexible lining was only one-sixth of that of the stiff one, and the remaining five-sixths had to be borne by the clay shale material in the side walls of the tunnel. For that reason we can expect a higher, or at least an equal, horizontal load against the flexible lining as compared with the stiff one.

The phenomenon has also another aspect. The shortening of the vertical diameter was accompanied by a lengthening of the horizontal diameter of the same extent, so that the side walls of the flexible tunnel lining thrust about four times as deeply into the clay shale as did those of the stiff one. This should cause a greater horizontal load on the flexible lining than on the stiff one; nevertheless K. S. Lane measured on the flexible lining a horizontal pressure which was only about one-third of that on the stiff one. Has he an explanation of this phenomenon?

In Paper 5/5 B. Kujundžić describes the results of his investigations of anisotropy in rock masses, obtained by measuring radial deformations in circular galleries when exposed to the action of internal radial loads. In explaining his results he assumes symmetry in the deformation of the vertical diameter, so that the sinking of the floor and the raising of the top are of the same extent. From our experience in mining we know that the rock in the roof is often not so solid as that in the floor, owing to a slight lowering of the fissured rock before lining; also, the filling up of the cavities and fissures at the face of the roof rock by the concrete of the lining is often not so perfect as in the floor and the side walls, so that by applying an internal vertical pressure the raising of the top of the lining may be more than the sinking of the floor.

My second remark on the same paper concerns the fact that the measured radial extension under 45 degrees was less than that in the horizontal and vertical directions. If the tunnel is deep enough and the horizontal stress in the virgin rock is appreciably smaller than the vertical stress, then the rock mass in the side walls will be fissured by the high vertical tangential

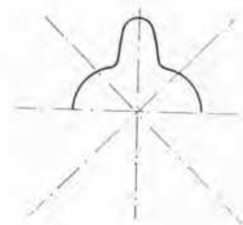


Fig. 10

stress, which may be much higher than the tangential stresses in the roof and the floor rocks. It is apparent that after concreting the lining the stiffness of the system will be smaller in the horizontal direction than at 45 degrees, so that the horizontal displacement by loading of the lining will be greater.

It is not my intention to depreciate the effect of anisotropy of rock, but I wish to put forward these experiences which have not been mentioned before.

T. R. M. Wakeling (U.K.)

The Metropolitan Water Board is engaged on the measurement of earth pressures acting on tunnels driven through stiff clays, particularly London clay, and interim results are available for the first two years after construction. The tunnels are shield driven and lined with pre-cast concrete segments, there being 12 segments to each ring. There are plain butt joints between adjacent segments with no bolts or other tensile connections.

By means of wedge-shaped key segments driven into position by the shield rams, each lining ring is expanded tightly against the circular surface cut in the clay by the shield. In this way a pre-stress is injected into the lining.

The hoop thrust is measured at various positions round the test rings by vibrating wire load gauges.

Earlier experiments on this type of lining have been described by Tattersall, Wakeling and Ward (1955) (Investigations into the design of pressure tunnels in London clay; *Proc. Inst. Civ. Engrs.*, July).

Observations have been made on a tunnel 9 ft. internal diameter with the lining generally 6 in. thick; the depth of the tunnel varying between 60 and 180 ft. below surface.

Load gauges have been inserted into 19 rings, where the effect of differing depths and differing initial pre-stress could be studied.

Summarizing the results so far obtained, the initial pre-stress was varied between 0·06 and 0·55 of the total overburden pressure, and at about the end of two years the pressure on the lining had increased to values between 0·33 and 0·76 of the total overburden. The smaller lining pressures were in general respective to the smaller initial pre-stresses.

These results may be compared with those obtained by W. H. Ward and T. K. Chaplin (Paper 5/13) for tunnels in similar strata but lined with bolted cast-iron segments. Their results have shown that the full total overburden pressure was reached in no more than 30 years after construction. For the Metropolitan Water Board tunnel, the present rate of increase of lining pressure would require a considerably longer period, assuming that the total overburden pressure is in fact reached.

The disagreement is probably caused by the effect of adjacent tunnels, and the smaller depth to diameter ratios in the cases described by W. H. Ward and T. K. Chaplin. Further research on these effects is required, and any similar observations on old isolated tunnels would be particularly valuable.

In Paper 5/7, K. S. LANE concludes that a flexible tunnel lining of steel ribs attracts less earth pressure than a rigid concrete one. However, it is interesting to note that his results show no significant difference between the average pressures on the two forms of lining until the end of the first six months, when the stress in the surrounding soil was increased by the embankment construction.

Earth pressures in clays continue to change for some considerable period, and there is no evidence to show that the flexible lining would continue to attract a smaller load. The use of flexible tunnel linings to limit bending stresses is standard tunnel practice but it is probably prudent to restrict K. S. Lane's conclusions concerning the magnitude of the pressures until further evidence is available.

The lining of the Metropolitan Water Board tunnel is flexible but it offers considerable resistance to thrust. In the section where the initial pre-stress was varied the results to date indicate that the earth pressure is increasing more rapidly on those rings that at present carry the lower earth pressures. This suggests that ultimately the earth pressures on all the rings might well become uniform.

It is tentatively considered therefore that for tunnels driven through clays, the creep and swelling properties of the clay might well be more important factors in determining the ultimate pressures acting on a tunnel lining, although the strength, flexibility, and method of construction considerably influence the earlier conditions.

W. H. WARD (U.K.)

There are three points that I want to mention in relation to pressures on tunnel linings. First, I wish to add a little information to Paper 5/13 by T. K. CHAPLIN and myself on the Existing Stresses in Several Old London Underground Tunnels. At site **B** the measured stresses were very high and the technique of measuring the existing stresses at that site involved cutting the cast-iron segments. Since writing the paper we have been measuring the stresses locked up in other segments during the casting process. These stresses can be quite high, particularly in the flanges where values up to 5 ton/sq. in. compression have been measured; hence the stresses due to the earth load at site **B** are likely to be somewhat less than the values given in our Fig. 5 by an amount which cannot be determined. Even if this correction were made there is still likely to have been noticeable bending in the segments at this site.

My next point arises out of the contributions to the discussion by the General Reporter, T. R. M. Wakeling, and from Paper 5/7 by K. S. LANE. It is unfortunate that K. S. Lane is not with us. I believe that T. R. M. Wakeling and I hold much the same views about how the earth load develops on different types of tunnel lining in London clay. We are both very interested in K. S. Lane's observations on the Garrison Dam tunnel, because it seems that the shales under Garrison Dam are rather similar to London clay, though the latter is probably weaker. In K. S. Lane's steel-lined tunnel the average circumferential thrust is only of the order of 20 per cent of the overburden pressure whereas in the stiff concrete lining the loading approximates to the full overburden pressure: that is after a period of only two years.

In London, too, we have this sort of difference between the jacked-in-place pre-cast concrete lining and the grouted cast-iron lining. As T. R. M. Wakeling has pointed out, the former lining does not support the full overburden pressure after two years, whereas the full overburden develops on the grouted cast-iron lining in a matter of about three weeks. The latter has been demonstrated at two sites by stress observations, and many thousands of diametrical strain measurements made many years ago in the City and South London railway confirmed this result (JONES, I. J. and CURRY, G., 1927, Enlargement of the City and South London Railway Tunnels, Reply to Discussion by Curry, *Min. Proc. Instn. Civ. Engrs.*, **224**, 231). It thus appears that in London a grouted lining develops the full overburden pressure rather quickly, whereas a lining which initially is not a precise fit in the clay develops very little pressure, and if it ever develops the full overburden pressure it takes many years to do so. What I should like to know from K. S. Lane is whether the space between his steel lining and the clay shale was tightly grouted up or not, because I have a hypothesis at the moment that the rapid development of load is associated with the complete filling of the void between the lining and the clay and the action of grouting.

The General Reporter suggests, if I understand him correctly, that the Fort Union clay shale is behaving as if $C'=0$ in the case of K. S. Lane's steel-lined tunnel at Garrison Dam. I do not understand this conception. The shear strength parameters C' and ϕ' refer to conditions in the ground when the strains are sufficient to mobilize continuously its full shearing resistance, and I doubt very much whether the strains at any stage of construction are large enough. Moreover, when a tunnel lining has ceased to change its shape and is in a state of equilibrium, it seems to me that none of the shear strength parameters normally used in soil mechanics can determine the load carried by the lining. Exactly the same remark could be made about the earth load on a retaining wall which is in equilibrium.

P. W. ROWE (U.K.)

We have reached the point with sheet piling where model tests, field measurements and theory all give bending moments which decrease both with increase in the flexibility of the pile-soil system and with the stability number. For practical purposes all present information, as far as I am aware, is broadly in agreement. I propose, therefore, to confine my remarks to design and to give one new reason why I consider that we should take failure as being at the first yield-point of 15 ton/sq. in. for steel, rather than at the ultimate plastic collapse moment of the section.

For a given degree of fixity below the dredge level and above the anchor, the deflected shape of the piling necessary to induce the first yield stress is a function of the properties of the wall material alone. This deflection causes a large volume increase behind the wall. With loose sand backfill the deflection is accompanied by a gradual subsidence which decreases fixity above the anchor and increases the bending moment. This process is controlled and stable. With dense sand, however, the maximum angle of shearing resistance, ϕ_{max}, is reached at much smaller deformations, after which ϕ decreases and the active pressure increases. The total active load can even double in value, and this can commence before the first yield-point of the wall material.

The ultimate bending resistance of concrete walls is about double the first yield moment; for steel walls the ratio is less. Consequently the increase in active pressure coupled with decrease in anchor fixity combine to increase the bending moment at a faster rate than the wall resistance can develop by bending. I have observed on models catastrophic failure commencing just before the first yield-point. I am convinced, therefore, that design should be based on the first yield stress, as described by the General Reporter.

M. S. KAPP (U.S.A.)

I wish to apologize for the fact that our measurement readings are not ready in time for this meeting but I propose to show some illustrations indicating the instrumentation for deflection

Fig. 11

measurements being taken on the steel bulkhead, and also the progress of construction—the results of all measurements will be made available at a future date.

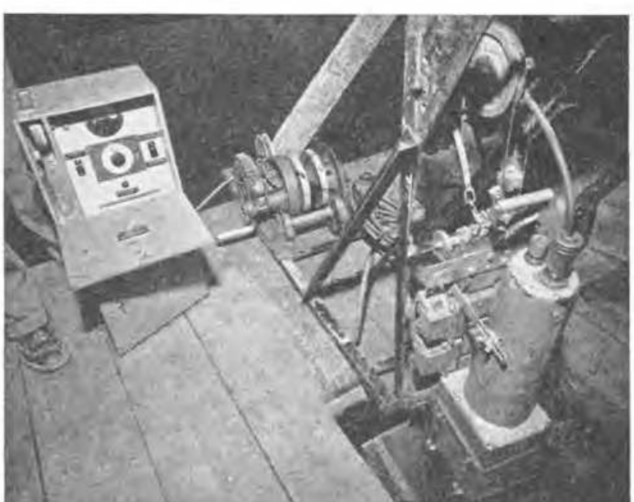


Fig. 12

Fig. 11 shows the steel bulkhead and anchorage under construction and the specially built platform used in making deflection readings. Tie rods have been installed but as yet the fill between the anchorage and the bulkhead has not been placed to grade.

Fig. 12 shows the Wiegmann Slope Differential Instrument used in making deflection measurements and its associated equipment. All equipment was made available to The Port of New York Authority by Princeton University.

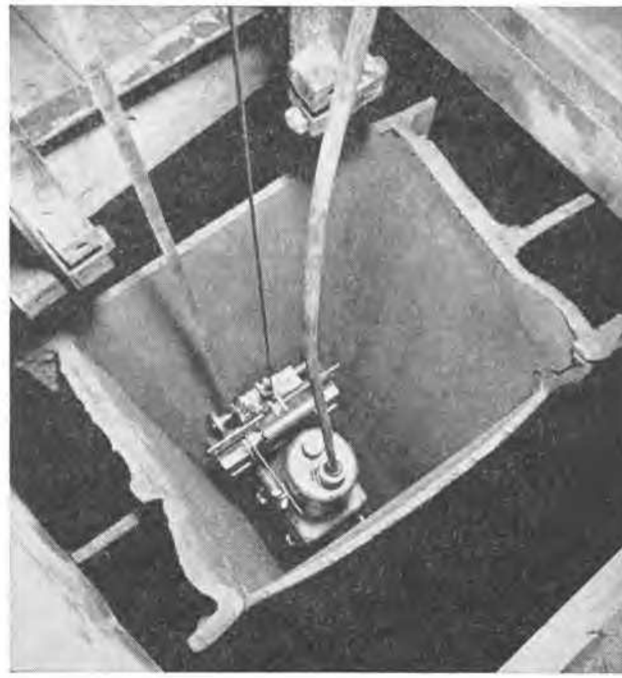


Fig. 13

Fig. 13 shows the Slope Differential Instrument running down the special box pile. This box pile is a permanent part of the steel bulkhead and has the same moment of inertia as the steel sheeting which makes up the bulkhead. This special pile will allow us for the first time to measure deflections below the dredge line.

M. BUISSON (France)

Monsieur le président, mesdames, messieurs, cette discussion se rapporte à la communication de A. CAQUOT (5/1) sur les silos.

Dans sa communication, A. Caquot a bien voulu faire allusion aux résultats obtenus par mes collaborateurs et moi-même concernant les surpressions en cours de vidange des silos.

Ces résultats seront vraisemblablement publiés au début de l'année prochaine. Certains devront être confirmés. Je vais en résumer quelques uns.

L'expérience a montré que dans les silos à parois lisses, notamment ceux exécutés avec des coffrages métalliques glissants, des surpressions importantes se produisent à la vidange. Elles entraînent des ruptures du fait que la limite élastique des aciers est dépassée. La méthode de calcul n'envisageait que le remplissage, ce qui ne laissait pas une marge de sécurité suffisante.

Les mesures que nous avons effectuées sur des ouvrages sains et sur d'autres fissurés ont montré que les déformations peuvent dépasser le double des prévisions et ce maximum se produit en géneral, dans le tiers central des parois.

D'un autre côté, dans des silos construits de telle façon que le frottement δ sur paroi égale certainement le frottement interne ϕ aucune surpression notable n'est enregistrée.

Mais ces mesures sont très coûteuses; elles ne permettent pas de faire varier les différents paramètres, ce que permettent les modèles réduits. Parmi les paramètres à étudier figurent notamment: la densité, le frottement interne ϕ, le frottement sur parois δ, la raideur de ces parois, la vitesse de vidange, l'excentricité de l'orifice, la forme du sol (carré, circulaire, rectangulaire), etc. ... L'observation des modèles réduits permet de mieux comprendre le mécanisme des opérations. Les déformations et les pressions ont été mesurées. Les premières mesures permettent de vérifier les secondes, en employant une méthode expérimentale de lignes d'influence.

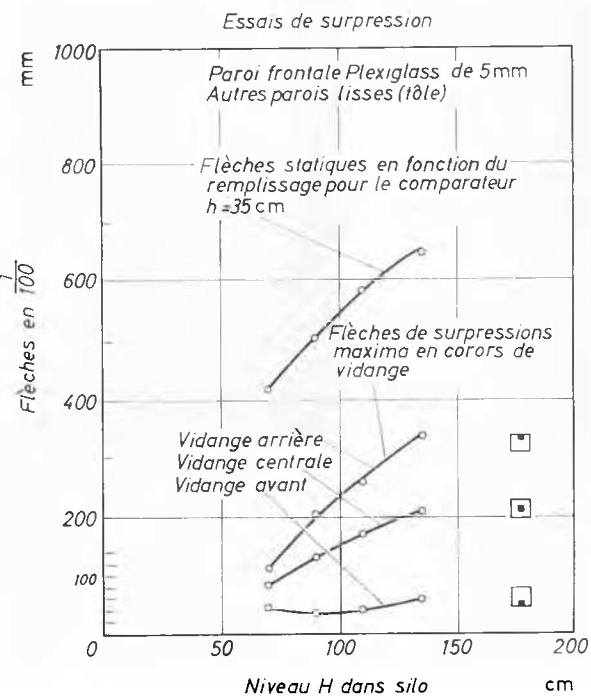


Fig. 14 Le silo modèle réduit avait 38 cm, de côté et 150 cm de profondeur. Les parois étaient en tôle lisse et la paroi frontale en Plexigalss. Le coefficient de frottement du blé sur la tôle et le Plexiglass est le même et est d'environ 15° — Les flèches sont mesurées au comparateur sur l'axe vertical de la paroi transversale à 35 cm au-dessus du fond pour diverses hauteurs de remplissage initiales *H*.
Les courbes représentent: (1) courbe supérieure: La flèche statique stabilisée au remplissage; (2) courbes inférieures: Les flèches supplementaires données par la vidange pour les trois cas suivants: vidange avant, vidange centrale, vidange arrière

The model silo measured 38 cm by 150 cm. The walls were made of smooth steel sheeting and the front wall was made of Perspex. The coefficient of friction of the grain against the steel sheets and the Perspex is the same, i.e. about 15° — the deflections were measured with a comparator on the vertical axis of the cross wall, at a height of 35 cm from the bottom, for various levels of filling *H*.
The curves represent: (1) top curve: the static deflection stabilized on filling; (2) lower curves: the additional deflections resulting from emptying in the three following conditions: from the front, from the middle, from the back

Les mesures confirment entièrement les résultats donnés précédemment. Les surpressions diminuent quand le frottement sur paroi augmente. Lorsque la vidange commence, les grains, de plus en plus éloignés de l'orifice se mettent en mouvement dans une zône détendue dont la forme, au voisinage de la paroi, évoque celle d'une flamme. En dehors de cette zône, les grains paraissent immobiles. Ils le sont vraiment en-dessous et le long des parois. Lorsque δ est faible, le mouvement relatif des grains entr'eux n'apparaît pas au-dessus de la flamme. Toute la masse descend ensemble. Lorsque δ augmente, la forme de la zône de rupture s'allonge et atteint la

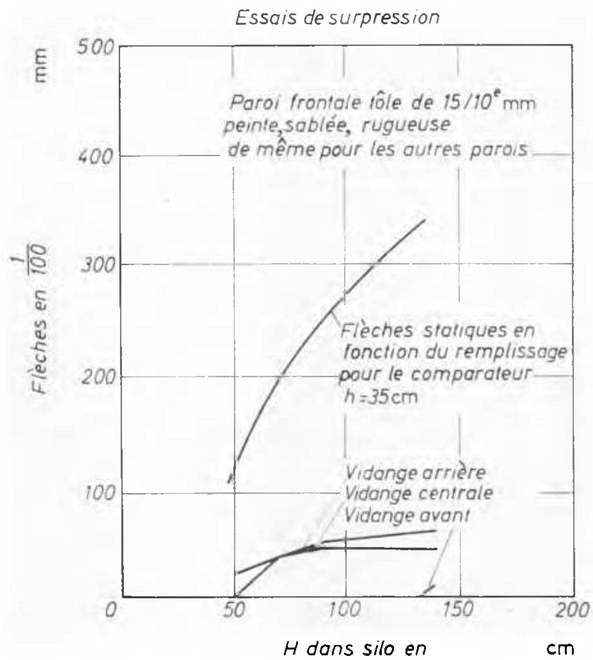


Fig. 15 Les circonstances sont les mêmes que sur la figure précédente, sauf que toutes les parois étaient en tôle sablée rugueuse
The conditions are the same as for the previous figure but all the walls were made of rough sanded sheeting.

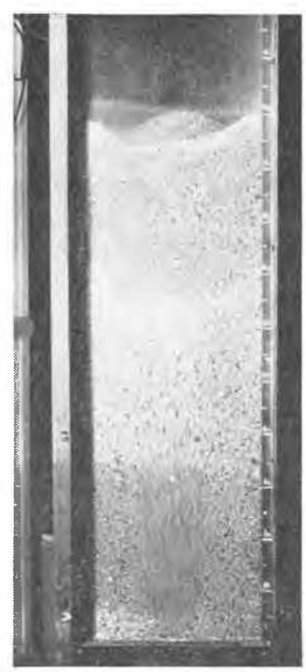


Fig. 16 Même silo — remplissage initial 135 cm — Trois parois en tôle sablée rugueuse — paroi frontale en Plexiglass lisse — Pose de 5 secondes en cours de vidange — hauteur de surface du blé: 100 cm vidange avant, adjacente à la paroi en Plexiglass — orifice de 2 cm × 2 cm
Same silo—initial filling 135 cm— three walls of rough sanded sheeting—front wall of Perspex—5 sec exposure during emptying—height of wheat level: 100 cm. Emptying from the front, near the Perspex wall—opening 2 cm × 2 cm

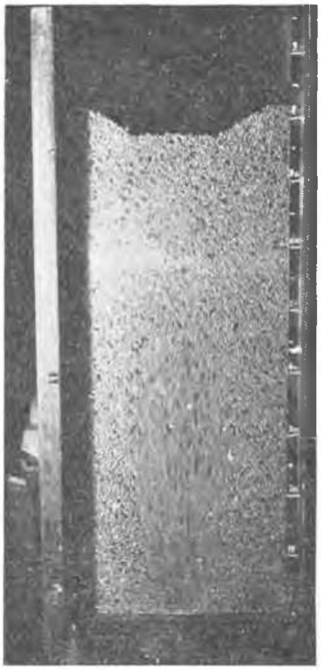


Fig. 17 Même silo et disposition que dans la figure précédente. Hauteur de blé: 80 cm
La photo montre la transformation de la partie supérieure qui se creuse — le mouvement des grains n'est pas visible sur la partie haute de la paroi frontale, mais se produit à l'intérieur
Same silo and test arrangement as before. Height of wheat level: 80 cm.
The picture shows the change in the top part which sinks in at the centre—the movement of the grains is not visible at the top of the front wall but takes place within the mass

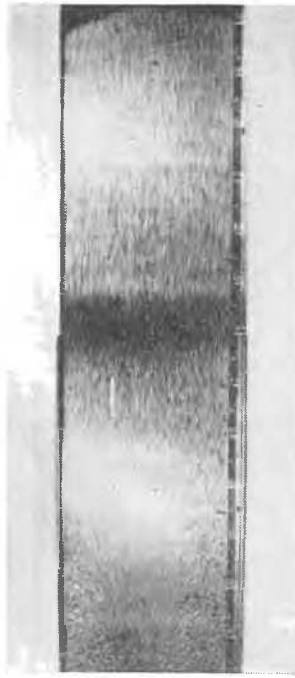


Fig. 18 Mêmes dispositions que dans les deux figures précédentes, sauf que trois parois sont en tôle lisse et une en Plexiglass.
La photo montre l'écoulement général de tout l'ensemble, sauf les deux coins inférieurs. Il s'agit de deux photos superposées
Same arrangement as with the two previous figures but three of the walls are made of smooth sheeting and one of Perspex.
The picture shows the general flow of the whole mass except in the two bottom corners. Two pictures have been superimposed

Fig. 19 Silo de 20 × 20, rempli de luzerne, à frottenment interne inférieur à celui du blé — 3 parois en tôle lisse, paroi frontale en Plexiglass — Les 3 parois de tôle sont munies de petites consoles continues de 35 mm environ de saillie tous les 20 à 25 cm. Ce dispositif provoque un écoulement central — les grains sont immobiles vers les parois laterales. Vidange par orifice avant, adjacent à la paroi en Plexiglass
Silo measuring 20 × 20, filled with lucerne with an internal friction lower than that of wheat—3 walls of smooth sheeting, front wall of Perspex—the three metal walls are provided with small continuous 35 mm ledges at distances of 20–25 cm. This arrangement produces a central flow—the grains remain immobile at the walls. Emptying from the front, near the Perspex wall

surface, c'est-à-dire, qu'elle devient une cheminée lorsque $\delta = \phi$. Dans tous les cas, le maximum de pression n'est atteint qu'après que le phénomène est devenu permanent. Pratiquement, la durée d'établissement atteint une demi-heure à une heure dans les silos eux-mêmes, entre une demi-minute et quelques minutes dans les modèles réduits, et la durée d'établissement du régime permanent varie évidemment en sens inverse de la vitesse de vidange.

Dans certains silos, des phénomènes spectaculaires périodiques sonores se produisent qui engendrent des vibrations des parois et du fond du silo. Ces bruits cessent lorsque le niveau de la matière atteint le haut de la zône de rupture, ce que nous avons constaté dans les modèles réduits.

On constate aussi que les plus fortes surpressions correspondent aux parois opposées à l'orifice de vidange. Mais, en ce qui concerne les parois voisines de celui-ci, elles sont plus fortes en haut de la paroi, plus faibles en bas, au-dessus de l'orifice de vidange. Les pressions, en cas de vidange centrale, sont plus faibles que sur la paroi opposée à l'orifice de vidange (en cas de vidange excentrée).

Il apparaît aussi que la pression de remplissage diminue lorsque la paroi est plus flexible; et cela est très conforme à ce que nous connaissons déjà. L'influence de la flexibilité de la paroi n'est pas encore dégagée nettement à la vidange.

Nous avons commencé également de réaliser un programme de recherches destinées à calculer le plus économiquement possible les organes horizontaux ou verticaux placés dans la masse en mouvement. Comme vous le savez, ces organes sont le siège d'efforts intenses.

Tout cela sera publié au début de l'année prochaine, sous la signature de mes collaborateurs, MM. Bourgine, Didelin et Gendre, et de moi-même.

Nous n'avons pas encore pu procéder à des comparaisons avec la théorie de A. Caquot.

Je m'excuse de la sécheresse de cet exposé et vous remercie de votre attention.

S. M. Hueckel (Poland)

Monsieur le président, mesdames, messieurs, complémentairement à mon rapport 5/3 concernant les essais sur la butée contre les plaques carrées, je voudrais vous présenter très brièvement les résultats des essais supplémentaires effectués dans notre laboratoire.

Nous avons notamment recommencé une série d'essais avec du sable sèché artificiellement, c'est-à-dire, du sable privé complètement de cohésion. Nous avons constaté qu'en ce cas la butée contre les plaques carrées se charge bien exactement avec la puissance 3 de la dimension de la plaque, le rapport de profondeur de la plaque à sa hauteur étant constant et inférieur à 5.

Ceci confirme que la butée dans un sable sans cohésion est directement proportionnelle au volume et, par conséquent, au poids du sable en mouvement et que dans les cas pareils les résultats des essais sur modèles réduits peuvent être extra — polés même jusqu'à l'échelle de 1/1.

Au sujet de la première question examinée par J. Kérisel, il me semble que le coëfficient de sécurité concernant la butée ne peut pas être adopté généralement mais que sa valeur doit toujours être choisie selon le déplacement — ou la déformation — admissible de la construction en question. Dans le cas des ancrages des palplanches dont il s'agit, dans nos expériences, le coëfficient de sécurité est égal à 2, auquel correspond le déplacement de la plaque 5 fois moindre, environ, que le déplacement au moment de la rupture, semble être juste.

Dans les autres cas, par exemple dans le calcul de la stabilité d'un corps massif enfoncé dans le sol, le coëfficient 2 peut être insuffisant, le déplacement d'un corps massif étant moindre que le déplacement d'un ancre.

Je voudrais signaler à cette occasion que les expériences décrites dans mon rapport ne forment qu'une partie des essais sur les ancrages effectués dans nos laboratoires. Nos études concernent aussi les autres formes d'ancrages comme les dalles horizontales, grilles verticales et ancrages élastiques comme celles formées de cables d'acier etc. ... .

J. Brinch Hansen (Denmark)

In Paper 5/4 N. Janbu has made an interesting attempt to apply the generalized method of slices to earth pressure and bearing capacity problems. His method will probably be valuable in cases where other methods fail, that is, for variable soil contents and pore water pressures; but in simpler cases such as those dealt with in the paper Janbu's method seems to be considerably less accurate than the best of the methods already available. I shall prove this by means of a few examples.

For $\phi=45$ degrees the coefficient of passive earth pressure on a perfectly rough wall is, according to Janbu, about 24, whereas the correct value is 22 for the surcharge and 18 for the self-weight of the soil. Incidentally, I cannot agree with N. Janbu in calling it 'slightly incorrect' to assume that these two coefficients are equal. Strangely enough, N. Janbu's values for a relative roughness $r=2/3$ agree perfectly with the correct values for surcharge by full roughness ($r=1$), which may suggest an error in his theory.

As regards the bearing capacity factors found by N. Janbu, his N_q is correct. Concerning N_γ, the absolutely exact values are, in my opinion, not known yet, but for $\phi=30$ degrees the most recent investigations seem to indicate an N_γ of about 15, whereas N. Janbu finds 23. Again, the difference is considerable, and on the unsafe side.

For the problem of inclined foundation loads, which has been solved previously by G. G. Meyerhof, E. Schultze and myself, N. Janbu has developed a formula which, in addition to the usual factors, contains a new factor N_h expressing the effect of the inclination. By assuming a fixed rupture figure, independent of the inclination of the foundation load, he finds N_h to be a function of the friction angle ϕ only. Actually, however, the rupture figure varies with both the inclination of the load and with the ratio of q to B, and the same must therefore apply to N_h. For $\phi=30$ degrees and in the limiting case of the greatest possible inclination ($P_h=P_v \tan \phi_e$) and $\gamma=0$, the correct value of N_h is 9·8, whereas Janbu finds 2·7. As I have shown in a paper listed in N. Janbu's references, the effect of the inclination is much more accurately taken into account by the formulae cited in my General Report to Division 3a.

All the problems dealt with in N. Janbu's paper are concerned with what I call 'pure zone rupture', and this is no coincidence. The method of slices cannot be used in earth pressure or bearing capacity problems involving line ruptures for given movements of the structure, because in that case both the magnitude and the location of the pressure resultant are unknown. Therefore, N. Janbu's method is not generally applicable to earth pressure and bearing capacity problems.

It has another disadvantage which may not be apparent at first sight. With the generalizations made by A. W. Bishop and N. Janbu the slice method can now be applied to almost any shape of the rupture figure. However, the method as such does not indicate at all whether the critical rupture figure is kinematically possible, and the method does not even always ensure that it is statically possible. Defects in either of these respects may lead to very erroneous results.

Summing up my remarks, I consider the proposed method an approximate but not very reliable procedure of dealing with some earth pressure and bearing capacity problems. The formulae and graphs given by N. Janbu are certainly much less accurate than the results already obtained by the best of the previously existing methods. For variable soil constants and pore water pressures, however, N. Janbu's method should be valuable, because the other methods fail in these cases.

A. R. Jumikis (U.S.A.)

I wish to discuss the application of N. Janbu's generalized procedure to bearing capacity calculations (5/4). The bearing capacity calculations are specialized and presented for an infinitely long strip foundation, loaded with a concentrated load

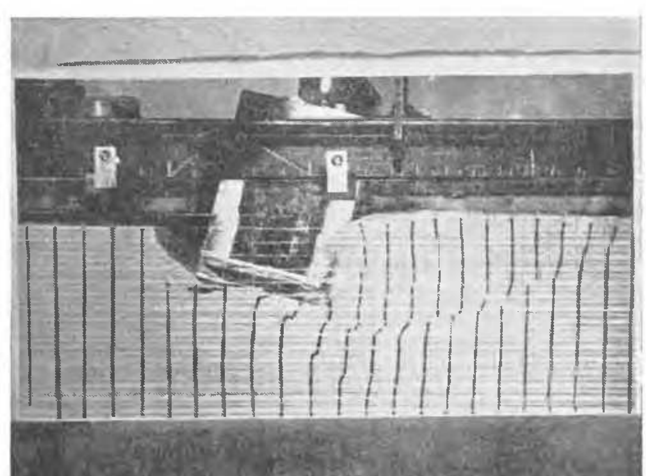


Fig. 20

applied centrally relative to the width of the footing, and directed obliquely at an angle of $\alpha=\arctan(P_h/P_v)$ with the vertical. The soil is a dry ϕ soil (cohesion$=0$, pore water pressure $u=0$). It is also understood that, when at a certain

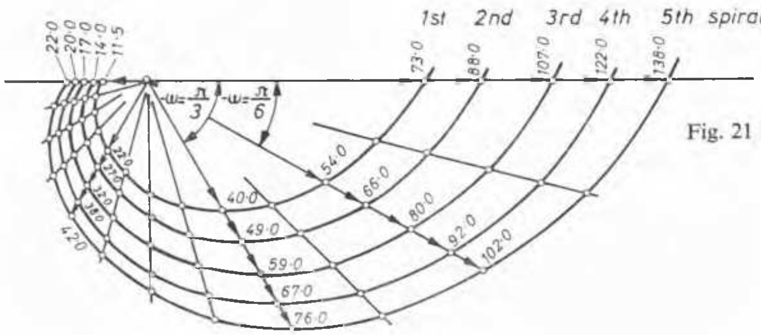


Fig. 21 Similarity of spirals. 1, Application of horizontal load, $h = 10$ cm $= 4$ in. above the base of the foundation model; 2, measurements in mm

Similarité des spirales. 1, Application d'une charge horizontale, $h = 10$ cm $= 4$ in. au dessus de la base du modèle; 2, mesures en mm

magnitude of the oblique load, $\bar{R} = (\bar{P}_h^2 + \bar{P}_v^2)^{\frac{1}{2}}$, the shear strength of the soil is exhausted, a lateral expulsion of a soil wedge from underneath the footing takes place. This manifests the ultimate failure in shear of the soil. Referring to Figs. 5 and 7 in N. Janbu's paper (5/4), the rupture surface in the soil is a compound cylindrical surface formed in part by a curve and in part by straight lines. The middle part of the rupture surface is a logarithmic spiral. The straight lines are tangents to the spiral at both its ends.

The results of N. Janbu's study are given for a strip of a soil mass and foundation the thickness of which is one unit of length perpendicular to the drawing plane.

Because stability calculations depend, among other things, upon the size and shape of the rupture surface, experimental

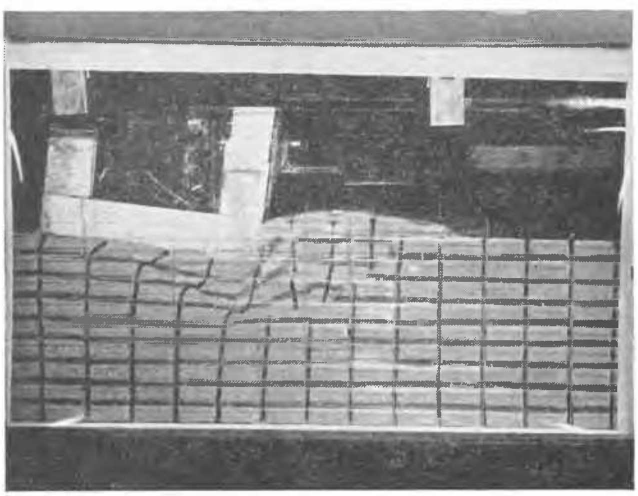


Fig. 22

research and observations from field performance of structures are valuable in order to compare theory with reality.

I have been engaged for several years in studying the shapes of rupture surfaces in dry sand brought about by oblique loads (Jumikis, 1956), and this research has revealed that the shape of the rupture or shearing surface in dry sand caused by centrally and obliquely loaded model foundations is a *cylindrical* one, of a logarithmic spiral shape. The general polar equation of the curve forming the rupture surface is

$$r = C.k.R^a.e^{-\omega.\tan\phi}$$

where

C, k and a are experimental parameters involving size and shape of spiral

R = magnitude of the oblique load

e = base of natural logarithms

ω = amplitude of spiral

ϕ = angle of internal friction of soil

r = any radius-vector of spiral for a certain amplitude.

The factor $\tan\phi$ in the exponent of the equation indicates marked differences in size as well as in shape of the spiral for various angles of internal friction of sand, ϕ.

With regard to ultimate failure, it is pertinent to note that the sand wedge, upon exhaustion of its shear strength, forms suddenly: a sharply cut spiral can be observed as in Fig. 20.

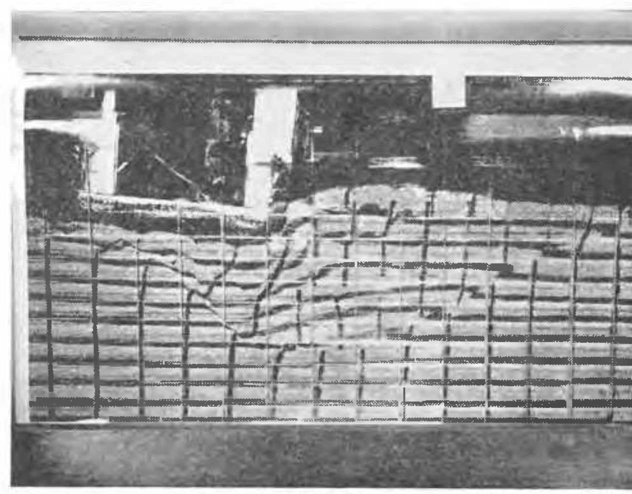


Fig. 23

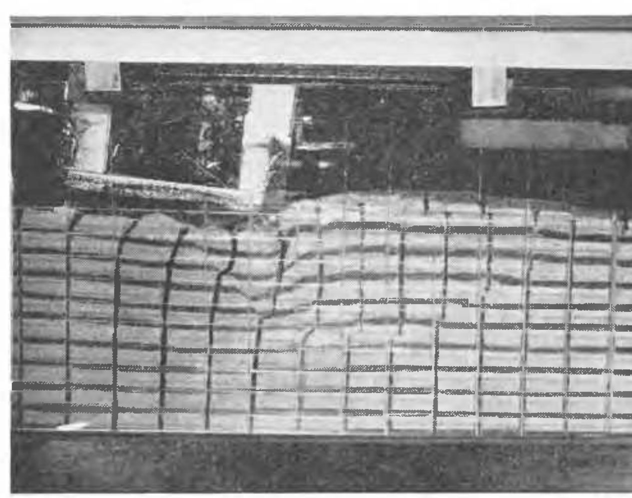


Fig. 24

This illustration shows that the assumed rupture surface illustrated by N. Janbu (in his Figs. 5 and 7) is not quite in agreement with the one experimentally observed. However, for practical purposes, the tangent-spiral-tangent curve is probably satisfactory. I also found that with various loads, the purely spiralled rupture surfaces are similar, as illustrated in Fig. 21: this fact corresponds with N. Janbu's idea as illustrated in his Fig. 8.

At failure, under oblique loading, the foundation model rests on the sand wedge in the manner illustrated, Figs. 22, 23 and 24, and not as shown in N. Janbu's Fig. 5: his Fig. 8 would correspond more nearly with the experiment. The mode of failure as in N. Janbu's Figs. 5 and 7 is confirmed by my experimental Fig. 25, for vertical eccentric loads. My experiments also confirm N. Janbu's statement that the actual critical slip surface for oblique loads is not as deep-seated as that used for the formula derivations, which are for vertical loads; see PRANDTL (1921).

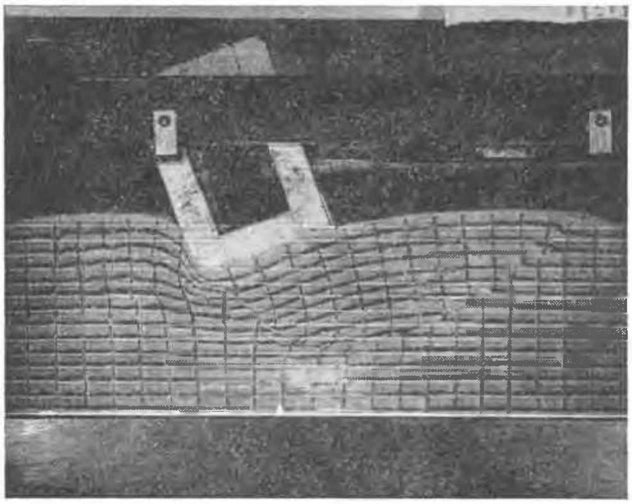


Fig. 25

N. Janbu does not mention in his paper how or in what manner the oblique load is applied to the foundation. It is apparent that an oblique load can be applied by applying both its vertical and horizontal components in proportion simultaneously; or the vertical component of the oblique load can be applied first and kept constant during the test, and then the horizontal component can gradually be applied until failure occurs. This latter method corresponds in most cases approximately to that encountered actually in engineering practice. First the weight of the structure increases as the structure goes up, and then the horizontal loads due to backfilling, hydrostatic pressure, wind loads, braking action and other loads *gradually* begin to apply. Upon the application of a horizontal component the foundation model displaces horizontally and continues to settle: during this time the soil suffers a distinct plastic deformation under the heavier loaded edge of the footing before it ruptures. Upon further increment of horizontal load the foundation model tilts, creating a probable trapezoidal pressure distribution under its footing; in this respect, probably SCHULTZE's (1952) key figure would be more appropriate than N. Janbu's Fig. 5. Because of the clearly pronounced rotation phenomenon as observed in the experiments, at impending failure, when active and reactive moments are in equilibrium, the moment method in stability calculations would seem to be more natural and much simpler. The whole system soil-foundation-load rotates as an entity.

The pressure distribution diagrams in N. Janbu's Fig. 8 do not show any influence or effect of the horizontal component of the oblique load. This point might perhaps be clarified by the author.

With oblique loads the spiral is usually somewhat shifted in the direction of the horizontal load component, as compared with N. Janbu's Fig. 5; also, the location of the pole of the spiral in such loaded systems is not at the corner edge of the base of the footing, but is somewhat higher (Fig. 26).

I believe that much thought is still to be given to N. Janbu's method of dividing the two terms in Coulomb's shear strength equation by the same factor F as in his equation 2. Unconfined compression tests of soils have shown that the shear strength of the soil remains at a constant maximum within a narrow interval of moisture content. After that maximum the shear strength of the soil drops very steeply with increase in moisture content. Besides, the frictional part in Coulomb's equation is less sensitive with changes in moisture content: it is the cohesive part which is very sensitive. In applying the method as in his equation 2, may it be that N. Janbu is a trifle too conservative? Another question in this connection might arise: if the method as in equation 2 is used, what kind of spiral should be used in stability calculations (when dividing τ_f by F)? The size, length and area of spirals, because they contain the exponential function $e^{-\omega \tan \phi}$, vary immensely as can be seen from the figures given in a table by BARBER (1956).

The application of a logarithmic spiral rupture surface to stability and bearing capacity calculations has a great advantage

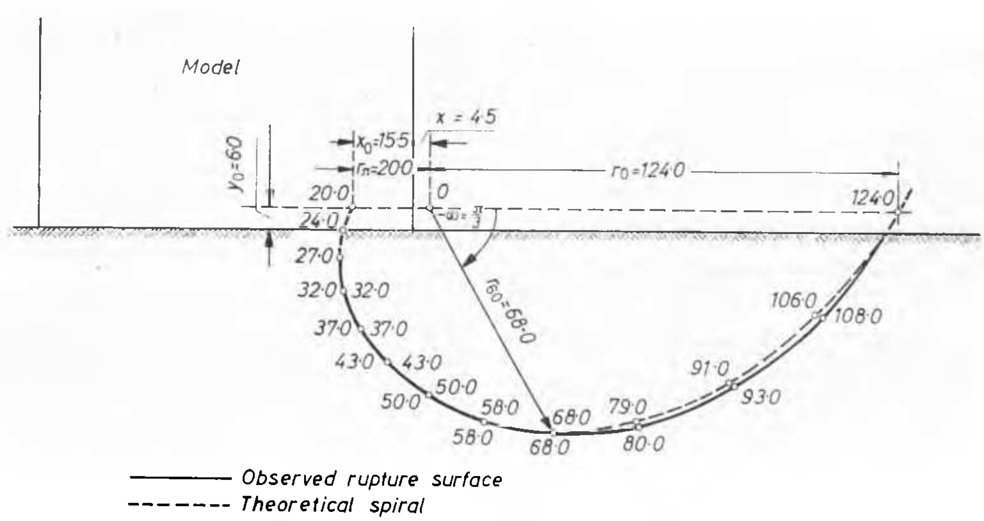
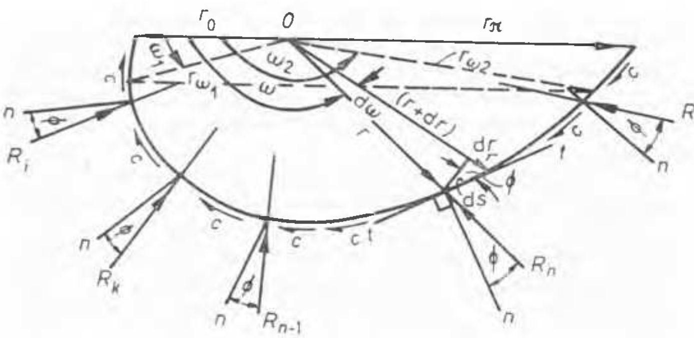


Fig. 26 Comparison of theoretical and observed spirals. Oblique loading. Contact pressure = 1·0 t/sq. ft.; $h=0$. All measurements in mm

Comparaison des spirales théoriques et mesurées. Chargement en oblique. Pression de contact = 1·0 t/sq. ft.; $h=0$. Toutes les mesures sont en mm

Angle of internal friction $\phi°$	*Spiral area* for $r_0=1$	*Moment of sector of logarithmic spiral (= moment of soil mass) for* $r_0=1$
20	6·06	5·29
25	9·49	12·91
30	15·84	33·39
35	28·65	95·13
40	57·74	311·10

over the assumed tangent-spiral-tangent, or even circular surfaces, since all radii-vectors pass through the pole of the spiral at a constant angle of ϕ to the surface, see Fig. 27. In such



ϕ = Angle of internal friction. R_n = reaction.
Cohesion: c, in pounds per unit area;
Cohesion: (c) (ds) (1) (pounds per differential area)
Moment: $dM_c = c \cdot ds \cdot \cos\phi \cdot r = c \cdot r^2 \cdot d\omega$

$$M_c = c\int_{\omega_1}^{\omega_2} r^2 d\omega = \frac{c}{2 \cdot \tan\phi} \cdot (r_{\omega_2}{}^2 - r_{\omega_1}{}^2)$$

When $\omega_1 = 0$ and $\omega_2 = \pi$

$$M_c = \frac{c}{2 \cdot \tan\phi} \cdot (r_\pi{}^2 - r_0{}^2)$$

Fig. 27 Moment of cohesion
Moment de cohésion

a case all soil reaction moments are automatically excluded from stability calculations. We do not know the magnitudes and the distribution of the soil reactions underneath the tangents of the compound rupture curve tangent-spiral-tangent and we can only make assumptions. In the spiral method, less assumptions need to be made than in that presented by N. Janbu.

I believe that my research has substantiated certain features of N. Janbu's paper.

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BENT HANSEN (Denmark)

I should like to congratulate G. SCHNEEBELI on his Paper 5/8. Anyone who has tried to determine experimentally two-dimensional figures of rupture by means of small models of sand between parallel walls of glass will certainly agree that the method proposed in this paper gives a much more clear picture of what happens and, moreover, makes it possible to get numerical values even from small models.

The mechanical analogy by which the types of rupture are studied has been applied in Paper 5/9 to the study of double sheet pile walls on a rock bottom. It shows good agreement with the proposed calculation method which is based on circular slip lines calculated by means of Kötter's equations. Actually, the method is very similar to that proposed by J. Brinch Hansen and yields approximately the same results. It is needless to say, however, that it is much easier to insert in the very simple formula given in the paper than to vary two parameters, trying to get an agreement in three rather complex equilibrium equations.

I should like to point out, however, that by the model studies it is only shown that the lowest rupture line is an arc of a circle. In fact, the proposed zone rupture, in which a whole family of rupture lines consists of circle arcs, is neither statically nor kinematically possible. This can be seen if one constructs the second family of rupture lines, cutting the circle arcs under an angle of 90°–ϕ. In the resulting net of rupture lines the stresses in the mesh points cannot be made to agree with Kötter's equations applied to both directions.

Probably the actual rupture consists of a single rupture line separating two rigid bodies which only undergo elastic deformations. The earth pressures in the sheet pile walls cannot, therefore, be found from the theory of plasticity, but they can, of course, be measured experimentally. Unfortunately, the line rupture itself presents a difficult problem. We have here the same ambiguity as in the shear box test as to whether a line rupture is a stress characteristic or a strain characteristic; that is to say, whether it is a line upon which the stresses fulfil the rupture condition, or whether it is a line connecting soil elements in a state of rupture but along which the deformation is zero. The method of J. Brinch Hansen and that proposed in Paper 5/9 both assume that the former is the case.

A. LAZARD (France)

Monsieur le président, mesdames, messieurs, je voudrais faire quelques remarques sur le papier de N. JANBU (5/4); mais je serai moins critique que les autres orateurs.

N. Janbu se fixe une position admissible de la ligne de passage des forces internes et développe un calcul par approximations successives.

A condition de ne considérer F que comme une inconnue auxiliaire, variant d'une valeur initiale F_o pour les forces internes nules à une valeur finale stable $F_{\lim}$, le procédé permet d'obtenir rapidement un couple de valeurs C_e et ϕ_e donnant l'équilibre limite le long de la surface de glissement étudiée, $C_e = C/F_{\lim}$ et $\tan\phi_e = (\tan\phi)/F_{\lim}$.

En regrettant que les exemples donnés ne concernent que des terres à frottement, je voudrais présenter les trois observations suivantes:

(1) L'intérêt du procédé consiste dans la rapide convergence des valeurs F.

Dans les exemples cités, la différence entre F_o et $F_{\lim}$ est faible. On pourrait être tenté de se contenter de calculer F_o. Mais il est certainement des cas où la différence est plus grande. D'autre part, $F_{\lim}$ semble toujours supérieur à F_o. Il y a donc intérêt à pousser le calcul jusqu'au bout.

(2) N. Janbu donne dans les graphiques inférieurs de ses figures les variations des forces internes T et E et nous devons l'en féliciter.

Néanmoins, je voudrais lui demander de les compléter: (1) par les variations des forces extérieures N et S; (2) par la position des résultantes générales de ces forces; (3) par le polygone de toutes les forces résultantes.

Je suggère même, qu'en conclusion du présent congrès, il soit dorénavant exigé de tout auteur la figuration des variations des forces telles que T, E, N, S, la position des résultantes générales, et le polygone des forces finales. Nous pourrons ainsi comparer entre elles les diverses solutions proposées et juger de la validité des hypothèses faites (voir rapport général de F. Walker pour la division 6).

A titre d'exemple, j'ai donné dans mon article *de Travaux* de septembre 1955, en application de la construction dite 'cercle des 8 points', des figures (17 page 715 et 31, page 718) où, volontairement, la position des forces intérieures était inadmissible. J'avais ainsi espéré soulever une controverse au sujet de l'hypothèse de base (proposée indépendamment par Taylor). Je n'ai reçu aucune observation, soit que mon papier n'ait pas été jugé intéressant, soit que mes contradicteurs aient été trop polis pour me faire une observation.

(3) Il ressort des exemples donnés par N. Janbu que la détermination de la position exacte des surfaces de glissement n'a pas une grande importance: F_{lim} présente un minimum très aplati (graphiques supérieurs des figures).

Cette conclusion est fort intéressante, mais elle mériterait d'être soigneusement confirmée pour des surfaces de glissement présentant des portions concaves et convexes.

De toute façon, elle va permettre de simplifier le calcul de la marge de sécurité par les procédés probabilistes auxquels j'ai fait allusion dans mon intervention de jeudi matin (4ème séance 3a).

G. P. TSCHEBOTARIOFF (U.S.A.)

I wish to thank the General Reporter for his attention to Johnson's tests reported by JOHNSON and myself (1953). Some additional new points brought out by these tests should however be emphasized in connection with the suggested discussion of a revision upwards of conventional values used for earth resistance computations.

Of primary importance is the manner in which the wall friction is mobilized.

(a) The highest values, up to $K_{p(\delta)}=12{\cdot}5$, were obtained during our test 15 when a progressively increasing downward thrust was exerted on the wall simultaneously with the application of the horizontal pressure. I believe this high value was produced because under such conditions—similar to those which develop in front of the embedded lower portion of an anchored sheet pile bulkhead—both ϕ' and δ reach their maximum values simultaneously.

(b) For conditions otherwise identical to test 15 and for the same value of $\delta=30$ degrees only a $K_{p(\delta)}=8{\cdot}8$ was obtained during our test 8 when the wall was merely prevented from moving upwards, as would be the case of a sufficiently heavy anchor block. My explanation is that since in such cases wall friction is developed as a result of expansion accompanying an upward displacement of the sand mass in front of the wall, the maximum value of δ is reached only after the peak resistance of the dense sand is passed, so that actually a reduced value of ϕ' is in effect.

Also of importance are the relative properties of deeper lying soil layers—in the General Report tests 7, 12 and 13 shown in Fig. 2 did not have the stationary block shown below the wall on Fig. 1. Thus, in contrast to tests 8, 9 and 15, the wall in these other three tests had no sand layer between its lower edge and the sectional bottom of the testing tank. As a result:

(a) Test 12, with dense sand and other conditions identical to those of test 8, reached almost the same value of $K_{p(\delta)}=7{\cdot}6$ in spite of a much smaller $\delta=15$ degrees. Presumably the close proximity of a lower rigid horizontal boundary had a compensating effect for the low wall friction.

(b) The reverse was true when a laterally yielding lower horizontal boundary was substituted in test 13. A value of only $K_{p(\delta)}=5{\cdot}6$ was obtained in spite of a higher $\delta=21$ degrees and of dense sand.

(c) A rigid lower boundary in test 7 but with loose sand above it produced results similar to test 13, namely $K_{p(\delta)}=6{\cdot}5$ with $\delta=19$ degrees.

It should be borne in mind that the $K_{p(\delta)}$ values reported by Johnson are obtained from the division of the measured horizontal component of the total passive lateral earth pressure by the total thrust which would be exerted by a frictionless fluid of the same density as the sand tested.

The General Reporter proposed as a third topic for discussion that we consider whether we should, to a certain extent, go back to the concepts of the eighteenth and nineteenth centuries by omitting cohesion from our stability computations of clay soils and using only an angle of internal friction instead.

I wish partially to support this suggestion of the General Reporter, namely in-so-far as retaining structures are concerned.

Could it be that the organizers of this conference have also accepted this suggestion since they selected an emblem for us which looks like a letter ϕ, and a capital ϕ at that?

Some of the younger members of this conference battled in recent years to get across the opposite $\phi=0$ concept to the 'old-timers' in their organizations, that is to engineers still adhering to concepts of pre-soil mechanics days. These young men may now be distressed at the thought that on their return from this conference they may hear from their elderly opponents: 'Didn't we always say so?' This should not worry them or us.

The General Reporter has correctly pointed out that the ϕ values of the nineteenth century are not the ϕ values of the present time. I also agree with him that although in that distant past ϕ was believed to be something very definite, the main difficulty now is the correct choice of a type of parameter ϕ from the many available, all meaning different things.

Although I am a firm believer in the need of some birth control where new symbols are concerned, I am afraid that we will have to introduce one more ϕ with the subscript *ce* (ϕ_{ce}), a term first suggested in 1955 by WATKINS *et al.* (1956) in order to define numerically our cell test results which corresponded to my 'consolidated equilibrium' concepts*.

The General Reporter's question as to whether for calculation of long-term stability one should go so far as to take systematically $c'=0$ is closely related to questions raised in P. W. ROWE's paper 1b/12 on the same topic.

Rowe correctly points out that his $c_e=0$ hypothesis: 'is in agreement with Tschebotarioff's neutral earth pressure ratio design method'. (This method was proposed for application to the design of retaining structures and of tunnels in clay (TSCHEBOTARIOFF, 1951, 1953).)

To study clays further in respect to this method under conditions of progressive consolidation and of 'consolidated equilibrium' a large triaxial 'cell' type device (Fig. 28) was built at Princeton University in 1953.

This research, as well as Johnson's passive earth pressure tests referred to earlier in this discussion, was and is sponsored by the Geophysics Branch of the Office of Naval Research, United States Navy.

It can be seen from Figs. 28 and 29 that this triaxial 'cell' device eliminates most of the criticisms made against the earlier, flimsier types of cell devices. It is very massive and is used in a constant temperature room. The 8 in. (that is 20 cm) diameter and 24 in. (that is 60 cm) high soil samples—disturbed or undisturbed—are enclosed by a thin rubber membrane and are given lateral support by Ucon oil which fills the space between

* Watkins originally used the symbol ϕ_r. We decided to change it to ϕ_{ce} in order to avoid conflict with other authors who used the subscript *r* previously (GIBSON, 1953) for a different purpose.

the membrane and the massive steel outer chamber. Five glass openings in that chamber, the upper two of which can be seen on Fig. 28, permit readings through the transparent oil on the sliding scales of five thin bronze bands which encircle the rubber membrane at these elevations. The volume changes and the lateral expansion of the soil specimen can thus be determined and checked against the measured amount of water expelled when drainage of the specimen is permitted and against pore pressures which can be measured in disturbed samples at four different elevations along the specimens' vertical axis. By opening a valve, supporting oil can be released in measured

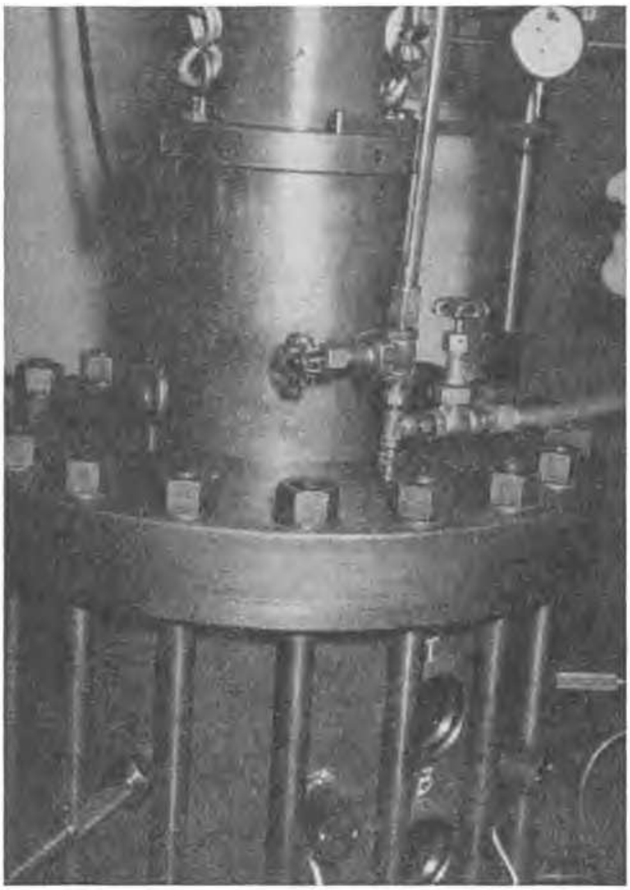

Fig. 28 Close-up photograph of massive triaxial 'cell' device for 8 in. diameter soil samples at Princeton University

Photo de la grande cellule triaxiale de l'Université de Princeton pour échantillons de 20·3 cm de diamètre

quantities. Controlled lateral expansion of the specimen can be induced thereby.

The device is so massive that a hand-operated crane has to be used to take it apart (Fig. 29). The lateral pressure σ_3 in the supporting oil is measured by two Bourdon-type gauges as a function of the vertical pressure σ_1 which is measured by two electrical resistivity Carlson pressure cells, one at the top and the other at the bottom of the soil specimen. This is purely a research tool—not one for routine testing use—since one test takes several months to perform.

In his 1955 *M.S. Thesis* (incorporated in WATKINS *et al.* 1956), J. Watkins established that the results of our tests on remoulded 8 in. diameter clay samples in the massive triaxial 'cell' device, when plotted for 'consolidated equilibrium' on a Mohr circle, had a straight line envelope which passed through the origin, i.e. indicated $c_{ce}=0$. This envelope formed with the horizontal an angle of mobilized effective shearing resistance ϕ_{ce} which decreased with an increasing PI value of the clay. Thus, for a silty clay with PI=7 per cent, Watkins found $\phi_{ce}=25$ degrees ($K_{ce}=0{\cdot}40$). For a clay with PI=62 per cent, he found $\phi_{ce}=17$ degrees ($K_{ce}=0{\cdot}55$). Over-consolidation increased ϕ_{ce} and decreased K_{ce}, but only slightly. The first clay had an activity (based on 5μ fraction) of 0·47 and the second of 0·93.

Parallel quick (Q) and consolidated quick (Q_c) tests carried to failure were performed on the same soils in conventional triaxial devices. The K values shown in Fig. 30 were computed from the classical equation using both cohesion (c_{cu}) and friction (ϕ_{cu}) from consolidated quick and from quick (ϕ_u) triaxial tests. The K values are plotted as ordinates against a depth of excavation or height of wall H which is obtained by taking the vertical pressure σ_1 at each stage of the test and dividing it by a unit weight γ of the soil of 100 lb./cu. ft. It can be seen that negative, that is impossible, K values are given by this conventional procedure for wall heights H smaller than 20 ft. On the other hand, our cell tests gave a constant K_{ce} value of 0·55 (i.e. $\phi_{ce}=17$ degrees) as shown in Fig. 30 for

Fig. 29 General view of the Princeton triaxial "cell' device as it is dismantled for a new test

Vue genérale de la cellule triaxiale de Princeton, démontée pour le commencement d'un nouvel essai

a remoulded clay with PI=62 per cent. Pre-consolidation decreased this value to $K=0{\cdot}42$, but at low vertical pressures only.

The ϕ_{ce} values obtained by us agree fairly closely with the ϕ_{cu} values of the limited number of soils so far tested, if we ignore the corresponding c_{cu} values (TSCHEBOTARIOFF *et al.*, 1956; SCHMID, YORK and RAYMOND (1957) in next item of discussion). Since the ϕ_{cu} values are smaller than the ϕ_d values, it appears possible that the ϕ_{cu} and hence also the 'consolidated equilibrium' ϕ_{ce} values would also be smaller than the ϕ' values. Such a finding would support the views expressed by P. W. ROWE's hypothesis in Paper 1b/12. However, further experimental investigations and comparisons are necessary before final conclusions can be reached.

Our 'cell' tests are being continued at Princeton by my colleague W. E. Schmid, who will outline to you some further results obtained.

I will add some remarks concerning over-consolidated stiff-fissured clays. A. W. Skempton's and D. J. Henkel's data referred to by the General Reporter were obtained from field observations which extended over a number of years. The effective ϕ' values obtained by them approximately equalled 20 degrees, which figure, applied to earth-retaining structures, gives $K=0{\cdot}49$—a value which falls within the range of our 'consolidated equilibrium' values, but, as expected, is lower than K_{ce} values for a clay of this high plasticity. On the other hand, E. DIBIAGIO and L. BJERRUM (5/2) obtain an effective

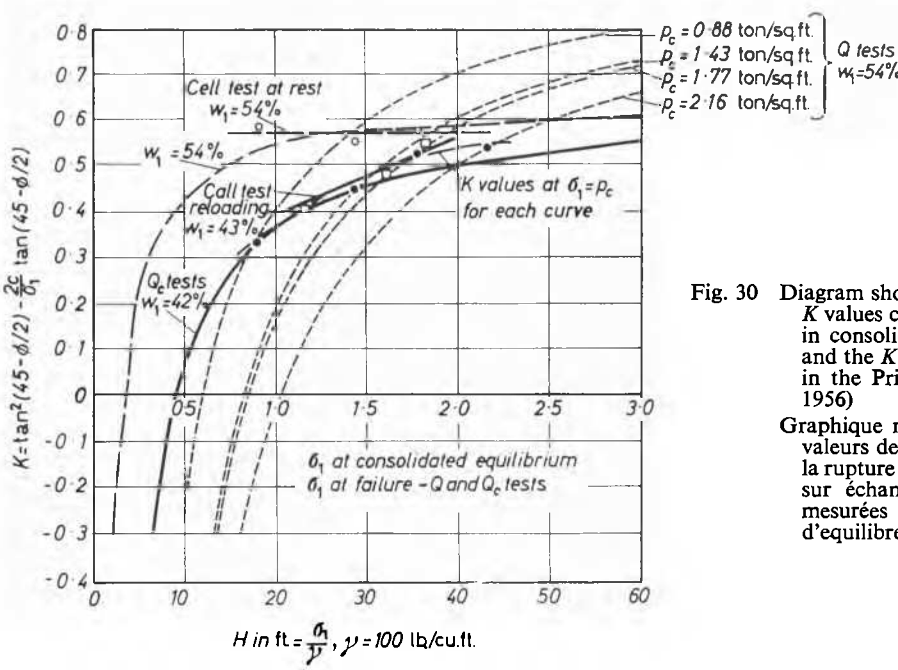

Fig. 30 Diagram showing that there is no relationship between the K values computed from the ϕ and the c values at failure in consolidated quick (Q_c) tests and in quick (Q) tests and the K values measured at 'consolidated equilibrium' in the Princeton triaxial 'cell' device (Watkins *et al.*, 1956)

Graphique montrant qu'il n'y a pas de rapport entre les valeurs de K calculées à partir de Q et les valeurs de C à la rupture dans essais rapides (Q) et dans les essais rapides sur échantillons consolidés (Q_c) et les valeurs de K mesurées dans la cellule de Princeton pour l'état d'equilibre par consolidation

$\phi' = 30$ degrees which gives $K = 0{\cdot}33$ well below the range associated so far with 'consolidated equilibrium' pressures of naturally deposited clays. It should however be noted that their relevant observations on stiff-fissured clays extended over a period of three months only prior to the start of winter frost which introduced an additional complicating factor. It appears possible that had observations been made over a number of years on this same clay they would have revealed a further softening of the clay in the fissures and would have shown for the summer months a further decrease of ϕ' and a corresponding increase of K up to usual 'consolidated equilibrium' values.

My conclusion is that there are many data which support the neutral earth pressure ratio design method, and hence the equivalent method of ignoring cohesion and using an angle of internal shearing resistance at 'consolidated equilibrium' for the design of retaining structures and tunnels in clay.

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W. E. Schmid (U.S.A.)

In addition to the tests by J. Watkins referred to in G. P. Tschebotarioff's comments, we have so far conducted triaxial 'cell' tests on the following cohesive soils in Princeton.

(a) An undisturbed saturated *soft* Chicago clay with a PI of 10, an activity of 0·54 and an unconfined compressive strength of 0·4 ton/sq. ft. (Tschebotarioff *et al.*, 1956).

(b) An undisturbed, saturated *stiff* Chicago clay with a PI of 16, an activity of 0·73 and an unconfined compressive strength of 1·48 ton/sq. ft. (Thesis by D. L. York incorporated in Schmid *et al.*, 1957.)

(c) A partially saturated Princeton Red clay with a PI of 7 and an activity of 0·47 (based on 5 μ fraction) compacted at three different moisture contents. (Thesis by G. P. Raymond incorporated in Schmid, *et al.*, 1957.)

All soils were tested under various stages of loading and reloading and under various conditions of drainage and lateral support. Of particular interest to us were the lateral earth pressures developed 'at rest' since they are most important from a design point of view.

The lateral earth pressure at rest E_o is frequently defined (Bishop and Henkel, 1957) as the pressure developed at zero lateral strain or zero wall movement. However, since cohesive soils are visco-plastic materials and the strain history of such visco-plastic materials is of little consequence for their final state of stress, I propose to define a lateral earth pressure at consolidated equilibrium as the pressure developed when the time-rate of strain is zero.

$$\frac{\partial \epsilon ij}{\partial t} = 0; \quad \rightarrow E = E_{ce} \quad \ldots . (1)$$

where ϵij is the strain at point i in an arbitrary direction j.

Since we consider ϵij as the total strain this definition represents the postulation for consolidated equilibrium, i.e. this lateral pressure contains no component from the excess pore water pressure. However, it allows for some restrained lateral deformation.

Summary of results—The results offered below refer only to normally consolidated fine grained soils since such only have been tested so far.

(1) All our results show that in the consolidated equilibrium state a cohesion parameter is not mobilized to reduce (or to increase in the passive case) the lateral earth pressures.

The coefficients for the lateral earth pressures at consolidated equilibrium are given by

$$K_{ce} = \tan^2\left(45 \mp \frac{\phi_{ce}}{2}\right) \quad \ldots . (2)$$

where ϕ_{ce} is the angle of shearing resistance mobilized at zero rate of strain. At the moment no conclusive relationship has been found to exist between ϕ_{ce} and the angles of friction developed in the conventional shear strength tests.

(2) A temporary loss of lateral support of a soil mass will cause an instantaneous reduction in lateral pressure but by creep and stress readjustment the earth pressure will soon again build up to its original consolidated equilibrium values.

We must conclude from this that for fine grained normally consolidated soils, the consolidated equilibrium state of lateral earth pressure is the stable equilibrium state.

This will be very important for the design of earth retaining structures as well as for anchorage structures in cohesive soils.

It should also be noted that just as in the 'limit' state of stress, where failure along a rupture plane is impending, two cases are possible, namely the active and the passive case. This also holds for the consolidated equilibrium case.

If the maximum shear stress in the soil mass is positive, i.e. it has a component acting on the free body in the same direction as the lateral earth pressure E, it will be the *active* consolidated

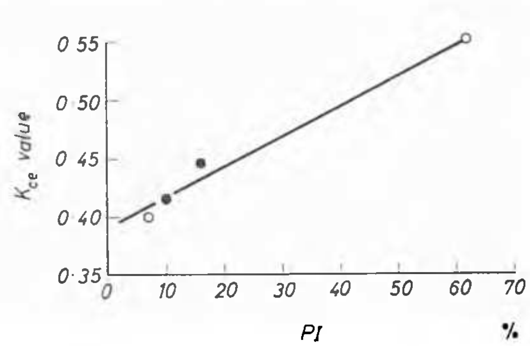

Fig. 31 Relationship between the coefficient of lateral earth pressure at consolidated equilibrium K_{ce} and the PI; full circles, undisturbed samples; open circles, remoulded samples

Rapport entre le coefficient de pression latérale du sol à l'état d'équilibre consolidé K_{ce} et l'index de plasticité; points noirs, échantillons intacts, points blancs, échantillons remaniés

equilibrium pressure. If this component is in the opposite direction, it will be the *passive* consolidated equilibrium pressure.

(3) The possibilities to relate the lateral earth pressure coefficient at consolidated equilibrium for saturated soils to other soil properties which can be more readily determined have been investigated (TSCHEBOTARIOFF *et al.*, 1956; SCHMID *et al.*, 1957).

The results look particularly encouraging for the relationships between the PI and the active K_{ce}. A rather tentative empirical relationship is given by

$$K_{ce} = 0{\cdot}38 + 0{\cdot}274 \times \text{PI} \qquad \text{(PI in per cent)} \quad \ldots\ldots \quad (3)$$

(see Fig. 31).

It must be emphasized that many more tests will be necessary, particularly in the PI range between 20 and 50, before any such empirical relationship can be confirmed.

(4) The tests on *partially* saturated compacted clay suggest that the angle ϕ varies with the moulding water content along a curve similar to the plot of dry density *versus* water content. It was definitely established that on the *dry* side of the optimum water content ϕ_{ce} increases with increasing water content. However, the maxima for both curves are somewhat displaced in relationship to each other (the dry density reached a maximum at a water content of 15 per cent whereas ϕ_{ce} showed a maximum at $w = 17{\cdot}1$ per cent).

We are not sure yet whether this shift is a physically significant one or whether it is the result of a change of the optimum water content for different compaction conditions. The reason being that the density–water content curve was determined in a Standard Proctor test whereas the 'cell' test specimen had to be compacted under different conditions although with a corresponding compaction effort.

(5) With regard to the General Reporter's question of using the $c' = 0$ concept for stability calculations, I would like to suggest the following:

(a) Experimental evidence as well as field measurements have so far established that cohesion is not mobilized in the consolidated equilibrium state. Therefore, for the calculation of long-term stability the neutral earth pressure ratio design method as suggested by Tschebotarioff or, in short, a method of using $c_{ce} = 0$; $\phi_{ce} \neq 0$ should be used.

(b) This long-time stability design should be checked against instantaneous failure. Here the cohesion comes definitely into play.

By this procedure, retaining or foundation structures will be made safe against both types of failure, namely, failures by instantaneous rupture and failures originated by successive deformations.

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A. W. BISHOP (U.K.)

A number of points have been raised during this session on which I am tempted to comment, but I will limit myself to a discussion of the coefficient of earth pressure at rest, K_o.

At Imperial College we have used for some years two different methods of measuring the coefficient of earth pressure at rest, both of them adaptations of the triaxial testing technique. The first has been used since 1950, and is a method by which the volume of water expelled from a saturated cylindrical sample during axial compression is used as a control for indicating the change in cell pressure necessary to maintain a constant diameter throughout the test. The results have been challenged on one or two occasions, and we have more recently developed a more accurate method of controlling the diameter of the sample, using a sensitive lateral strain indicator. During the test the lateral pressure is varied so that the diameter remains constant during the loading and unloading stages. The test can be carried out under drained conditions or, in the case of partly saturated soils, under undrained conditions with the measurement of pore pressure. The main results of the earlier tests have been substantiated by this later technique.

Some typical results from these tests may be quoted as examples. For saturated loose sand the value of K_o was found to be 0·46, the corresponding angle of friction ϕ' in terms of effective stress being 34 degrees. For saturated dense sand the value of K_o was 0·36, the corresponding value of ϕ' being 39 degrees. In each case, on first loading, the lateral stress increased linearly with vertical stress, and the value of K_o remained sensibly constant throughout the full stress range. If reloading cycles were carried out the ratio of lateral to vertical stress showed the influence of stress history in a manner somewhat similar to that shown by the volume changes in the consolidation test.

For a remoulded clay (normally consolidated) having a PI of 25 and an activity of 0·6 a value of K_o equal to 0·7 was obtained, the corresponding angle of shearing resistance measured in a drained test being 23 degrees. An undisturbed alluvial clay (also normally consolidated) gave a value of K_o equal to 0·43, the angle of shearing resistance expressed in terms of effective stress being 33 degrees. The PI in this case was 10 and the activity was about 0·9.

I think it is rather misleading to relate the value of K_o or of any other similar parameter to the PI alone. It is also important to know whether the samples are disturbed or remoulded and what the activity of the clay fraction is, as this can have an important influence.

In all the tests referred to above similar rates of testing were used to obtain the value of K_o on the one hand and the value of the angle of shearing resistance on the other. The effect of creep on the value of both parameters is, of course, of great interest in relation to the long-term stability or earth pressure on structures. This is a much more difficult phenomenon to measure in the laboratory, but in our apparatus it is quite easy to study the early stages of creep in which the major part of the strain or volume change occurs. It is interesting to note that with sugar, which provides a convenient material with very marked creep properties, a large volume change occurred under constant vertical stress during the first few hours of the test, yet the value of K_o remained sensibly constant. Sand showed the same tendency. This is a rather surprising result, and it will be of great interest to find out by a more elaborate test whether this occurs on a really long-term basis.

With clays we have not the same number of creep data yet. Most of our tests on saturated or partly saturated clays have been carried out on the basis of one- or two-day tests, but it is interesting to conjecture whether the K_o value will show the same tendency to remain constant during creep. It is known, of course, from other tests at Imperial College, and from the work of D. W. Taylor and A. Casagrande, that the value of the angle of shearing resistance in terms of effective stress can drop 10 per cent or more on a very long-term basis. If K_o remains constant, it is possible that the coefficient of active earth pressure might, on a really long-term basis, tend to approach the coefficient of earth pressure at rest.

G. P. Tschebotarioff and W. E. Schmid stated that in their cell test apparatus the lateral strain apparently exercises little influence on the consolidated equilibrium earth pressure in clays. This might be taken to indicate, in more familiar terms, that the lateral strain in passing from the at rest condition to the active condition had no influence on the stress ratio. In sand there are many tests which show clearly that this is not so. If it is the correct interpretation of the test results in the case of clays, it would be interesting to know at what point the discontinuity in behaviour occurs.

An alternative explanation of the results may be, however, that their cell tests do not start from the condition of exactly zero lateral strain. In any form of cell test, some lateral strain, albeit a small amount, occurs and this may account for the slightly anomalous behaviour reported by G. P. Tschebotarioff.

G. P. Tschebotarioff also found difficulty in correlating the values he obtained with the results of conventional strength tests. The tests he cited were consolidated undrained (or consolidated-quick) tests and undrained (quick) tests, in neither of which were pore pressures measured. The shear strength parameters were therefore not obtained in terms of effective stress. How a correlation could be expected between strength parameters expressed in terms of total stress and a coefficient of earth pressure expressed in terms of effective stress is not apparent.

In conclusion I wish to emphasize the importance of avoiding confusion in terminology. The usually accepted definition of the *coefficient of earth pressure at rest* refers to the state of stress under the special condition of *zero lateral strain*. On the other hand the coefficient of lateral earth pressure at *consolidated equilibrium* is defined by W. E. Schmid with reference to the state of stress when the *lateral strain rate with respect to time is zero*. The definition allows 'some restrained lateral deformation', apparently of an arbitrary amount.

There seems little justification from experimental evidence, other than the Princeton tests on clay, for the expectation that these two definitions should lead to similar results. In sands they clearly will not do so. If this new concept of a consolidated equilibrium coefficient is to be used, it should be made very clear by the symbol used to denote it that it represents neither an at rest condition, where lateral strain is zero, nor an active condition, where freedom for lateral strain is permitted and the shear strength is fully mobilized. It appears to be, in general, a property influenced by deformation, and should be qualified in some way to indicate what deformation is being considered.

L. BJERRUM (Norway)

In his General Report, J. Kérisel has proposed for discussion the question: Which value of the shear strength should be introduced in a computation of earth pressures in saturated clays, taking into account possible changes in shear strength with time?

In a discussion of this question we have obviously to distinguish between two types of problems. To the first type belong those problems which occur during the construction period, before there has been any appreciable dissipation of the pore pressures resulting from the change in loading. Typical of this type of problems are the contractors' questions about what strut loads he can expect in braced excavations or what slopes he has to use in temporary cuttings.

If the clay encountered is saturated and intact—and hereby is understood that the clay is not fissured—this type of problems can be analysed by the $\phi=0$ method. This means that the undrained shear strength, from unconfined compression tests or from vane tests, can be directly inserted in the stability or the earth-pressure computations. This is a theoretically logical approach and its reliability has been confirmed by a great number of field observations.

The second type of problems are such as occur after the construction period when the pore pressures created by the construction operations slowly change until finally an equilibrium ground water condition is established.

In many cases during this period we will have a steady increase in shear strength, as, for instance, is observed under footings and fillings. In such cases we therefore only need to consider the end of construction conditions, as all later changes will only increase the safety factor.

In several cases, however, we have to consider a reduction in shear strength with time. This may, for instance, be the case if a slope or a trench is excavated or if we dredge in front of a sheet pile wall. In such cases the unloading will result in a drop in pore pressure followed by a swelling, and a consequent reduction in strength of the clay. If, moreover, the clay is over-consolidated, we have in addition to consider the softening resulting from increase in shear stresses. This is due to the fact, which is not always sufficiently appreciated, that if a stiff dilatant clay is subjected to shear under undrained conditions, this will be accompanied by a drop in pore pressures. In and around excavations and trenches in over-consolidated clays there may thus be a substantial drop in pore pressure, partly due to direct unloading and partly due to increased shear stresses.

With time the pore pressures will, of course, increase, and the shear strength consequently decrease. This reduction in strength with time may result in delayed failures or increase in earth pressures.

For intact clays we assume today that during this process the shear strength is controlled by the full value of the effective shear strength parameters c and ϕ as found, for instance, by drained triaxial tests. There are only few field data which can support this belief, but recently we investigated a slide in Norway which occurred some years after the excavation of a cutting in an over-consolidated intact clay. In this case the c and ϕ as found from drained tests showed a good agreement with the observed strength, indicating that the reduction in strength with time was only due to a change in piezometric levels and not due to a reduction in shear strength parameters.

Now, if the clay has a fissured structure, this will influence the reduction in strength with time in two different ways. In the first place the fissures will for even very small lateral strain greatly increase the permeability of the clay. In the test trench described it was most surprising to observe that the pore pressures adopted the equilibrium values corresponding to the new ground water conditions one to two weeks after the excavation. This means that in fissured clays we necessarily have to consider a very rapid reduction in strength due to a rapid dissipation of negative pore pressures set up during the construction. This reduction can only be taken into calculation by a c, ϕ analysis in terms of effective stresses, and this method must therefore be recommended also for the problems during the construction period in such cases where fissured clay has an opportunity to expand laterally. This was clearly illustrated in the test trench mentioned, as it proved possible to correlate the variations in strut loads with the variation in piezometric levels.

The fissures also have a second effect on the strength. With time the strength decreases more than can be explained by the change in pore pressures. We do not know the explanation, but it is very likely a mechanical effect of the lateral opening of the fissures. Skempton and Henkel's interesting studies of the slides in Jackfield and in London indicate that this effect can be taken into account in an empirical way by assuming that c will decrease with time. Incidentally, this finding was confirmed by an analysis of the strut loads measured in the test trench.

Until further data are available, it is recommended that earth pressures from stiff fissured clays should be computed on the basis of effective stresses and that a reduction of c with time should be taken into account.

D. H. Trollope (Australia)

The General Reporter has drawn our attention to the accumulating evidence in support of the $c'=0$ hypothesis where questions of long-term stability are concerned and analysis in terms of effective stresses is entailed.

If it is recognized that moisture tension is an essential component of the effective stress history of a soil, it is encouraging to find in many cases that a logical interpretation of long-term behaviour can be developed when all factors are considered. Indeed, as the General Reporter remarks, it may be found possible to express the shear resistance of all soils in terms of the appropriate effective stresses and the relevant friction parameter. If this be so it is a tribute to the vagaries of this natural material that it has taken about 300 years for us to fully appreciate the difference between applied and effective stresses in this context. At this stage it is perhaps important for us to focus some attention on the role of negative moisture stresses in the general framework of the effective stress concept.

In a paper to this conference, G. D. Aitchison (1b/1) has offered substantial evidence that the effective stress law first proposed by K. Terzaghi and subsequently shown by him and others to be valid for saturated soils (viz: $\sigma'=\sigma-u$) may be extrapolated with high negative values of u for clay soils having the necessary small pore dimensions. So that, at least as a first approximation, we may assume the shear strength of clay soils to be primarily dependent on moisture tension within the range where the soil is subject to a moisture deficit. Furthermore it is implicit in an understanding of the behaviour of soils in the unsaturated range that a condition of moisture deficit can be developed from a saturated soil merely by a reduction in externally applied stresses. This point can be illustrated from a study of the conditions obtaining in a slope excavated in clay soil.

For such a slope it can be shown that the reduction in overburden pressure owing to excavation must be accompanied by an equal reduction in pore pressure if a condition of no volume change with no change in effective stress is to be maintained. Furthermore, if the reduction in overburden pressure is numerically greater than the previously existing pore pressure then a negative stress will be induced in the soil water: thus a moisture deficit is established. This moisture deficit will be relieved if free water is available, but in order to allow the intake of free water the void ratio of the soil must increase (the swelling effect). Under these circumstances the soil will ultimately reach a new equilibrium condition consistent with the new hydraulic gradients set up by excavation of the slope. The rate at which this equilibrium is reached depends primarily on the magnitude of the moisture deficit induced, the permeability of the soil and the availability of free water. Thus considerable time may be entailed in establishing the new condition of moisture equilibrium.

With the change in moisture status of the soil will go the volume change necessary to accommodate the increased moisture content and hence a reduction in shear strength will be achieved. Provided therefore that free water is continuously or periodically available the mechanism whereby the shear strength of a clay soil forming an excavated slope can reduce with time is manifest. It will be seen that the reduction in strength can be attributed to two factors, the reduction in pore pressure and the increase in void ratio. The latter factor can only be developed if there is no external restraint on the soil mass, but as the surface of the slope is unstressed this requirement is generally met.

In the case of the sheeted trench described by E. DiBiagio and L. Bjerrum (5/2) however conditions differ from those of the excavated slope. The sheeting supporting the trench provides some degree of external restraint to the soil mass.

For example, if no volume change is to occur in a swelling soil, an increase of 1 lb./sq. in. in the pore pressure (or a reduction of the same amount in moisture tension) must be resisted by an equal external restraint. It can be shown that the magnitude of the maximum strut loads measured on the experimental trench by the authors up to the end of November 1956 can be accounted for if it is assumed that the struts and sheeting are perfectly rigid and the pressures developed are equal to the change in moisture tension of the soil behind the sheeting. To achieve the measured forces it has been calculated that the distance between any point on the vertical face and the nearest point on the free water surface would have to change by an amount of the order of 1·5 m (5 ft.). Examination of the recorded movements of the water table will show that this requirement is met where the measured strut loads are greatest—at about the mid-height of the trench.

The foregoing analysis assumes that at every point in the soil mass an all-round external restraint of magnitude equal to the change in pore water stress can be developed. However this cannot be so as there is no restraint on vertical movement of the horizontal surface of the soil. The analysis does at least point out however that strut loads of the required order can be developed as a result of changes in soil moisture tension. It is clear that the actual strut loads will be a function of both swelling pressures and pressures developed due to shear strain within the supported soil; therefore before accepting the authors' conclusions that the $c'=0$ analysis is applicable to the sheeted trench case in general, it appears desirable to attempt to resolve the relative contribution of these two factors. It would be of considerable value if the authors could extend their investigations to include the effects of allowing the struts to yield until a minimum load on each strut was recorded.

N. Janbu (Norway)

Two or three minutes is too short a time to give any answers to the questions raised on Paper 5/4. However, I should like to say that in earth pressure determinations from a theoretical point of view we should distinguish, as has been pointed out by our President, between two sets of assumptions: (1) regarding shear strength; and (2) regarding the mathematical approach. No doubt all of us today realize that our problems lie in shear strength. The uncertainties in the shear strength values which we introduce into our analyses may in certain unfavourable conditions mean that our analyses may be afflicted with an error of magnitude of sometimes 50 or 100 per cent. Therefore, I cannot give any definite comparison between the deviations regarding the purely mathematical approach.

I should like to mention one point, however. I wholeheartedly believe that since shear strength investigations are our trouble-maker, the factor of safety should be placed on that, and, therefore, when comparing errors to numerical or mathematical approaches, we should compare the errors on that basis, that is, on the equivalent differences in tan ϕ. For instance, in the N_γ value, which is unknown to all of us, what is rigorous is between 15 and 23, corresponding to δ differences in tan ϕ of some 8 or possibly 10 per cent.

I believe that in order to increase our knowledge of earth pressure determinations in a general way we have three fields at which we should look with considerable interest: (1) a continued effort in shear strength determinations; (2) study of the actual behaviour of earth supporting structures to find out how these structures behave in the field; and (3) theoretical developments.

Rapporteur Général

Monsieur le président, mesdames, messieurs. Ainsi que M. le président vient de le dire, je ne peux pas avoir l'ambition de résumer de façon précise notre discussion de ce matin. Je pense toutefois, qu'il n'est pas inutile d'essayer de rapprocher certains points de vue et de localiser certaines contradictions. Je vais donc m'efforcer de le faire et je vous demande toute votre indulgence.

Au sujet des apports expérimentaux, la communication faite au début de cette séance par J. L. Serafim, du Portugal, dans laquelle il nous a montré les très hautes pressions qui se développent au bord des tunnels qui ne sont pas circulaires est des plus intéressantes. Je voudrais sur ce point dire que, même pour un métal percé d'un trou circulaire et soumis à une compression homogène, il se développe au bord du trou, ainsi que nous le savons, des efforts de deux à troix fois plus grands que l'effort moyen. Il en est de même, bien entendu dans les sols, et les efforts sont encore beaucoup plus grands, si ce trou n'est pas circulaire, par example s'il est demi-circulaire ou, en tout cas, tronqué.

M. Lazarević nous a montré l'importance, non seulement de l'anisotropie mais encore de l'anisothermie. Nous le remercions de sa communication.

F. J. M. de Reeper, des Pays-Bas, s'étonne que les déplacements verticaux et horizontaux dans les expériences relatées par K. S. Lane pour ses tunnels soient inégaux; je crois qu'il est nécessaire à cet égard de rappeler que les expériences de notre président, K. Terzaghi, sur la poussée, celles de Johnson sur la butée, montrant que pour mobiliser des efforts égaux en butée, et en poussée il faut des déplacements inégaux et réciproquement. Je pense aussi que du point de vue des tunnels des apports intéressants nous ont été fournis par T. R. M. Wakeling et par W. H. Ward. Ils montrent qu'autour des tunnels, la souplesse du revêtement n'est pas le seul facteur important mais qu'également le fluage intervient également d'une façon notable. C'est les cas également pour l'injection faite autour de la paroi du tunnel. Il me semble aussi que l'accent n'ait pas été mis suffisamment sur un troisième facteur qui est le dispositif de drainage à la paroi, c'est-à-dire, la possibilité pour la pression interstitielle de disparaitre.

En ce qui concerne les contributions de théorie générale, nous avons vu que N. Janbu a été extrêmement demandé aujourd'hui au cours de cette discussion, loué par les uns, blâmé par les autres, comme disait Beaumarchais. Il est bien certain que l'espérance d'Alexandre Collin, exprimée dans un livre qui a été traduit récemment au Canada, où il relate de longues observations sur la forme des surfaces de glissement, à savoir que dans un temps relativement court on saurait déterminer d'une façon théorique et même pratique la forme des surfaces de glissement, a été décue. Nous devons donc choisir arbitrairement des lignes de glissement puisque le problème, dans l'état de nos connaissances est indéterminé. Là, je rejoins, évidemment, dans le choix à conseiller pour les surfaces de glissement, toutes les recommandations rappelées aujourd'hui par J. Brinch Hansen et celles présentées par G. de Josselin de Jong dans une séance précédente, à savoir qu'il faut que ces surfaces de glissement soient compatibles cinématiquement et que les conditions limites soient satisfaites. J'ajouterai d'ailleurs, d'accord avec Bent Hansen, que lorsqu'on a affaire, non pas à une zone plastique, mais à une ligne de glissement qui sépare deux zones élastiques, l'interprétation de cette ligne est particuliérement difficile. Nous observerons aussi combien est utile le recours à des expériences telles que celles présentées par notre collègue A. R. Jumikis pour le choix de la ligne de glissement.

Pour en arriver aux trois points dont je proposais la discussion dans mon rapport général, je pense que l'on peut dire que, sur le premier point, le choix du facteur de sécurité nous pouvons être d'accord avec S. Hueckel, de Pologne, sur le fait qu'à chaque fois c'est un cas d'espèce. C'est un cas d'espèce, par example, pour les plaques enterrées dans un sol, puisqu'il dépend alors du déplacement que l'on peut légitimement admettre. Bien d'accord avec le W. E. Schmid pour calculer le coëfficient de sécurité, non pas seulement à court terme, mais simultanément à long terme et dans les deux circonstances indiquées.

Là où il est la plus difficile de conclure, et ceci ne vous étonnera pas, c'est sur les points 2 et 3 de mon rapport général.

Est-il correct, est-il possible de prendre à long terme la cohésion égale à 0? Tous les orateurs qui se sont exprimés à cette tribune ne sont pas d'accord sur la position à prendre. Un certain nombre ont été d'accord sur la possibilité et même l'opportunité de prendre à long terme C égale à 0, mais il semble bien que la définition de ϕ_r proposée par l'école américaine de Princeton, fasse l'objet de discussions.

Définir ϕ_r par l'expression $K=\tan^2(\pi/4)-(\phi_r/2)$ revient à mesurer K au triaxial et A. W. Bishop a montré qu'un certain nombre de paramètres interviennent soit avec une déformation latérale nulle, soit avec une vitesse de déformation nulle. Nous sommes obligés aussi de noter que la notation ϕ_r a déjà été employée antérieurement par le A. W. Bishop dans une acception différente.

D'autre part, en dehors d'un équilibre plastique est-il bien logique d'écrire une formule telle que celle ci dessus?

Nous en arrivons, par conséquent, à dire que c'est sur les points 2 et 3 qu'il y a le plus de flou dans nos discussions. Notre collègue, le L. Bjerrum, nous dit qu'à court terme incontestablement, il faut prendre comme caractéristique $\phi_u = 0$ et $C_u \neq 0$. A long terme, il est partisan d'une diminution très substantielle dans la prise en considération de la cohésion et il conseille de prendre ϕ effectif, c'est-à-dire le ϕ mesuré au triaxial, en tenant compte de la pression interstitielle et en décalant le cercle de Mohr. Cette proposition a tout son intérêt, je le crois, mais je pense que suivant le cas d'expèce, il faut réintroduire la pression interstitielle dans la mesure où l'ouvrage lui permettra ou non de se développer, car il est bien certain qu'aussi bien dans les tunnels, si on pratique des trous de drainage, que dans les fouilles ouvertes où la pression interstitielle peut diminuer en raison de l'absence de revêtement, cette pression interstitielle peut évoluer très largement.

J'en arrive ainsi à ma conclusion définitive. Beaucoup d'orateurs ont indiqué à cette tribune que la seule façon pratique d'aborder le problème était de simuler au triaxial ou en laboratoire, les conditions qui se développent *in situ*.

J'ai eu la curiosité de rechercher dans les dictionnaires anglais et français, la signification des verbes 'simuler' et 'to simulate' et j'ai constaté que dans les deux langues elle présentait une certaine ambiguité.

Dans l'Oxford dictionary, la signification est là suivante: 'donner faussement l'apparence de ... avec l'intention de tromper'. C'est sa première signification. Heureusement, elle est suivie d'une deuxième: 'imiter quelque chose en apparence'. Cette acception est un peu moins défavorable et je souhaiterais qu'elle soit adoptée.

The Chairman

I now ask our President to close the discussion by saying a few words in his own inimitable way.

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

We are all indebted to J. Kérisel for his brilliant summary of what we have learned in this session and to those who gave the items of discussion which provided him with ammunition.

The topic of this session was unique inasmuch as its theoretical foundations were laid 180 years ago by the classical paper on earth pressure which was published by Coulomb in 1776, and it has given us a unique opportunity to realize what has happened since that time.

In 1881, about 100 years after Coulomb had published his paper, one of the most prominent engineers of his generation, Benjamin Baker, read, in this very building, a paper on the actual lateral pressure of earth work in which he discouraged emphatically the application of theoretical procedures of any kind to the problems of earth work engineering, and the numerous arguments which he presented in his paper were convincing indeed. Eighty years have elapsed since he read his paper, and Coulomb's theory is today as valid as it was in 1776, but the arguments on which Benjamin Baker's warnings were based became void because the failures he described were due not to defects of Coulomb's theory but to the complete lack of knowledge of the limits of the validity of the theory. These conditions have radically changed since Baker's time, because we have acquired the useful habit first of all of examining very carefully the assumptions on which our reasoning is based, and secondly of checking our conclusions against the results of observations in the field.

The general wedge theory illustrated by the paper presented by E. DiBiagio and L. Bjerrum (5/2) is a very good illustration of this fact. According to the general wedge theory, the distribution of the lateral pressure on the timbering in open cuts is not triangular but roughly parabolic. When I published this theory about 20 years ago I was keenly aware of the fact that it can only be approximately correct, because there was no doubt in my mind that the details of the pressure distribution depend to a large extent on the method of construction. Since that time I have missed no opportunity to encourage the measurement of the pressure exerted by the earth on the timbering in open cuts, and my efforts were not wasted because we now know very well the uncertainties which are associated with the application of the theory to the design of the timbering. As a consequence, we are already in a position to compensate for these inevitable errors and uncertainties by appropriate selection of the factor of safety.

All these statements also apply to the theories pertaining to the pressure of earth or of rock on the lining of tunnels. Therefore the case records presented by contributors to the discussion are vital contributions to our knowledge in the realm of soft ground tunnelling. However, the value of these records would be still further increased if their authors would add to the records prior to their publication a detailed description of the materials to which their records apply, because I had the impression that in some of the records these descriptions were rather sketchy, or entirely absent.

Another factor which has a great influence on the pressure of earth on tunnel linings is the method of construction. The following incident may illustrate this fact. A few years ago we had to construct several thousand feet of drainage tunnel through perfectly cohesionless sand. The excavation of the tunnel formed part of a contract awarded to a firm of very good reputation, but the performance of the contractor in the drainage tunnel was disappointing in every respect. During the first few weeks the average progress was 6 in. per day and the slope located above the tunnel entrance became dotted with sink holes, because most of the excavated material had entered the tunnel through the roof. On account of these conditions it became obvious that the tunnel would be far more expensive than we anticipated and that the deadline for its completion could not possibly be met. The prospects for the future were gloomy indeed. One evening, after I had returned from the job in a depressed state of mind, I started to chat with the janitor of the guest house. In his private capacity he was the husband of the cook. The janitor told me in the course of conversation about his younger days when he was still a prospector in the famous Karibou district on the Upper Fraser River, and he described to me the difficulties which they had to overcome in mining through water-bearing silty sand in order to reach the gold-bearing gravel. Listening to his accounts I got the impression that he knew much more about soft ground tunnelling than the foreman on our drainage tunnel. Hence next morning I called on the general manager and proposed replacing the foreman on the tunnel job by the janitor of the guest house. After some initial hesitation and inquiries my proposal was accepted and the results were gratifying indeed. Within a short time the average daily progress increased from 6 in. to 4 ft. The janitor was visibly rejuvenated, and the tunnel was completed within the specified time and at a cost which was close to the estimated amount. There is no doubt in my mind that the earth pressure on the lining in the first section of the tunnel located below a series of sink holes was very different from the earth pressure on the section which was so successfully managed by the janitor.

The influence of factors other than the physical properties of the earth and the dimensions of the tunnel on the stresses in the tunnel support was also brought out by the following incident. Some 30 years ago, when I was connected with the construction of a rock fill dam in Algeria, a large diameter outlet tunnel was mined through a very stiff and slickensided, Eocene clay. The progress was satisfactory and the load on the tunnel support was consistently moderate. After the tunnel was excavated over about half its length, the miners walked out on account of a wage dispute and the tunnelling operations were discontinued over a period of about two weeks. During this period the load on the tunnel support increased in the proximity of the heading to such an extent that timbers with a diameter of 14 in. were completely crushed.

Although I know the influence of the method of construction and the rate of progress on the earth pressure on tunnel supports from experience, I am unable to account for it on purely theoretical grounds. Hence in the realm of tunnelling, too, theory can be misleading unless our theoretical knowledge of the subject is supplemented by the digest of the results of reliable field observations.

N. Janbu (Norway)

Since three of the contributors to the discussion in session 8 referred to Paper 5/4, an answer seems to be justified. Here, particular emphasis will be given to the accuracy of the generalized procedure of slices as compared to other well-known procedures: also the condensed study below will be concerned only with the discrepancies due to differences in the mathematical approach. These discrepancies will be given in terms of required per cent change in shear strength or safety factor (F) to obtain identical results, because it is only these differences which can be directly matched against the uncertainties in shear strength determinations, and differences in applied safety factors.

Earth pressure—For earth pressure calculations the largest relative differences between J. Brinch Hansen's method and Paper 5/4 are found for rough walls, such as shown by examples in Table 1.

Table 1

Comparisons between J. Brinch Hansen and Paper 5/4 for active and passive earth pressure (maximum differences, $r=1$)

$\frac{q}{\gamma H}$	*Active: % difference in F or* tan ϕ_e			*Passive: % difference in F or* tan ϕ_e			*Valid for:*
	$\phi_e=25°$	30°	35°	$\phi_e=25°$	30°	35°	
	3·5	3·0	2·5	4·5	4·0	3·5	Vertical walls, horizontal terrain, $c=0$, $r=1$ (rough) and $L=\infty$ (two-dimensional)
0·15	4·5	3·5	3·0	5·5	5·0	4·5	
0·30	5·0	4·0	3·5	6·5	6·0	5·5	

From Table 1 it is readily seen that the maximum difference between the two methods is of the order of magnitude of 5 to 6 per cent for all practical purposes. It is also quite correct that the values obtained from Paper 5/4 (for $r=1$) lie on the unsafe side of J. Brinch Hansen's.

It should be noticed, however, that J. Brinch Hansen's values for $r=1$ and $q>0$ must be slightly on the safe side because the principle of super-position, which is involved in his analyses, is not valid, strictly speaking. Therefore, the differences between Paper 5/4 and a more rigorous solution must be even smaller than given in Table 1 for $q>0$, probably less than 3 to 4 per cent. Moreover, for decreasing r the differences decrease rapidly down to zero for $r=0$.

Altogether, I believe that even the maximum differences observed are very small as compared to the uncertainties associated with our shear strength determinations, and the discrepancies due to different choice of safety factor, and roughness.

To indicate the effect of roughness, the following example is illustrative: If one person uses $r=1$ while the other uses $r=0{\cdot}5$ the difference in F (or tan ϕ) is roughly some 20 per cent for this reason alone.

For the conditions compared it is believed that the two methods are very nearly of equal reliability in practice. It is well known, however, that by means of J. Brinch Hansen's general earth pressure theory one can handle a great variety of earth pressure problems where other theories fail, particularly for given relative deformation or yield.

Bearing capacity—In Table 2 a relative comparison is made between the bearing capacity formulae suggested by J. Brinch Hansen (General Report, Division 3a) and those included in Paper 5/4.

Table 2

Comparisons between J. Brinch Hansen and Paper 5/4 for vertical and inclined foundation loads

$\frac{q}{\gamma B}$	*Vertical: % difference in F or* tan ϕ_e			$\frac{P_h}{P_v}$	*Inclined: % difference in F or* tan ϕ_e			*Remarks*
	$\phi_e=25°$	30°	35°		$\phi_e=25°$	30°	35°	
0·5	1·0	3·0	4·5	0	−1·5	1·0	2·0	Horizontal terrain, $c=0$, $L=\infty$ (two-dimensional). A minus sign indicates lower values by Paper 5/4 than by J. Brinch Hansen, Gen. Rep. Div. 3a
1·0	−1·5	1·0	2·0	0·1	−1·5	−0·5	0·5	
1·5	−4·0	−1·5	0	0·2	1·0	1·5	1·0	
2·0	−5·5	−3·5	−1·5	0·3	6·5	5·5	4·5	
Assumed: $q/\gamma B=D/B$				Assumed: $q/\gamma B=D/B=1$				

From Table 2 it is seen that the relative discrepancy between the two methods is usually less than ± 5 to 7 per cent. Since both formulae are only approximations, primarily intended for practical use, it is almost surprising that the differences are so small.

There is one exception, however, namely for high values of P_h/P_v close to tan ϕ_e, in which case the relative difference may become considerable: but neither of the two approximations is very accurate for this limiting condition. However, this circumstance is of little practical interest because in actual design (for instance of gravity retaining walls) the maximum inclination of the foundation load will rarely exceed $\phi_e/2$, corresponding to a maximum P_hP_v of roughly 0·3 to 0·4.

Again, I think one can conclude that the two methods are very nearly of equal reliability for the conditions most frequently encountered in practice.

To obtain more accurate information of the bearing capacity for large inclinations of the load more theoretical and experimental work is still needed. In this connection it will be of importance to study in greater detail the interesting model tests reported by A. R. Jumikis in his discussion. A review of previous tests will also be necessary.

Slope stability—The procedure outlined in Paper 5/4 was originally developed for the purpose of exploring the stability conditions for irregular soil and slope profiles, because it is mainly for these conditions that the slip circle method may lead to safety factors on the unsafe side, as already verified by several examples from practice.

Furthermore, for an assumed slip surface and chosen line of thrust all internal forces and stresses along the slip surface may be calculated for F=constant irrespective of the stratification and variations in c and ϕ. Numerical examples have already been worked out, but not yet published.

A more detailed publication concerning the application of the method described in Paper 5/4 is being prepared, and when available it will also include some numerical examples of the kind asked for by A. Lazard.

B. Kujundžić (Yugoslavia)

A propos des discussions des V. Mencl et F. J. M. de Reeper sur les lignes des déformations radiales d'une galérie circulaire creusée dans un massif rocheux et sollicitée par une charge hydrostatique interne, lesquelles ont été présentées dans mon rapport 5/5, je voudrais bien donner quelques explications complémentaires.

Au laboratoire de l'Institut Hydrotechnique à Belgrade nous avons effectué récemment des essais sur modèle représentant, d'une manière convenable, un milieu orthotrope. L'ouverture circulaire creusée dans le modèle a été soumise à une charge radiale interne et les déformations ont été mesurées. La ligne des déformations radiales obtenue a été de forme de lemniscate avec quatre points d'inflexion.

Dans notre rapport nous avons constaté que, à part de stratification et schistosité, l'état de tension dans le massif rocheux après l'excavation de la galérie, influence aussi la forme de la ligne des déformations radiales. Essentiellement, il existe deux influences: les caractéristiques mécaniques des massif rocheux aussi bien que son état de tension.

Des recherches systématiques sur ce problème sont en courts.

C. Lotti (Italy)

B. Kujundžić dans sa communication 5/5 rapporte les résultats des mesures des variations de longueur des diamètres, sous l'action des pressions intérieures, conduites par lui sur cinq tunnels: les diagrammes obtenus démontrent l'anisotropie de l'amas rocheux qui, selon B. Kujundžić, est dû à la stratification, à la fissuration, à la scistosité et à toutes les alterations fisique du rocher; il arrive à la conclusion que la théories de Lamé, valable pour les milieux isotropes, doit être adaptée à l'anisotropie réelle.

V. Mencl, dans la discussion, observe que les différences des déformations sont dûes à l'état de contrainte determîné dans le rocher autour de l'escavation, de traction à la clé et de compression à la retombée. Il est clair que le materiel dans cet état des contraints a des différents réactions aux efforts suivants.

Je ne veux pas entrer en discussion sur les causes qui déterminent les differentes distributions des deformations dans un tunnel, et j'observe seulement que probablement toutes les deux raisons, données par B. Kujundžić et V. Mencl, sont croyables; tandis que je ne peux pas être d'accord avec B. Kujundžić, avec sa conclusion d'adapter la théorie du tube de Lamé à l'anisotropie réelle.

Il est mieux à mon avis, comme d'ailleurs fait allusion l'auteur même, de reduire l'anisotropie par des injections de ciment. Il est evident que les injections de ciment, particulièrement si elles sont concentrées dans un anneau de rocher de pas grand epaisseur autour du rêvetement, peuvent ameliorer l'état fisique du rocher en lui donnant l'isotropie que nous cherchons.

Des essais récents, faits en petits tunnels pour la détermination du module élastique du rocher de fondation d'un barrage

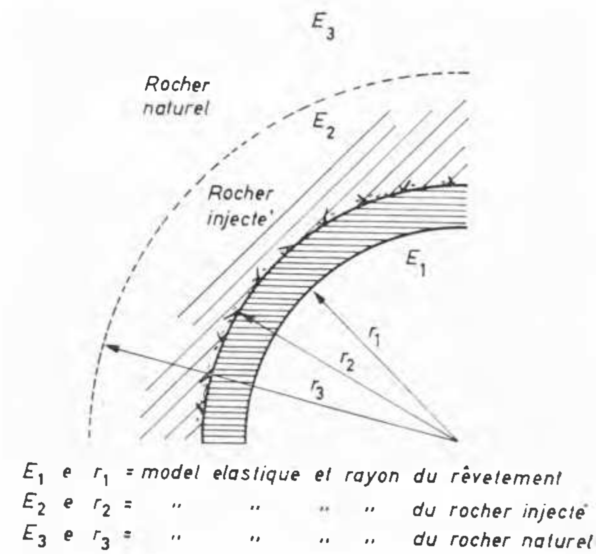

Fig. 32

ont démontré que les injections de ciment ne peuvent pas seulement améliorer mais rendre aussi très uniforme le module elastique. D'autres essais systematiques, et plus en grand sont actuellement en cours et nous espérons de donner des résultats bientôt.

Il s'agit par consequence, au lieu d'introduire le facteur anisotropie, qui varie de place en place et que l'on devrait déterminer avec des mesures continuatives pour toute la longueur du tunnel, de faire l'application des formules de Lamé au rêvetement et à un anneau de rocher autour du rêvetement, anneau qui sera amelioré par les injections et qui aura un module élastique très uniforme: au dehors de cet anneau, les efforts sont si réduites que l'anisotropie de l'amas rocheux n'a aucune influence sur la distribution des contraints dans le rêvetement.