Session 7 – Division 5

Earth and Rock Pressures

Poussée des terres et des roches

Chairman/Président: W. C. VAN MIERLO (Netherlands)

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J. BRINCH HANSEN (Denmark)
G. STEFANOFF (Bulgaria)
J. VERDEYEN (Belgium)
W. H. WARD (Great Britain)

Chairman: W. C. VAN MIERLO (Netherlands)

The chairman for this session was to have been Dr. Sokolosky of the U.S.S.R., but because he is not attending this conference the Organizing Committee invited me to take over the chairmanship, and I do so with great pleasure.

Before starting the discussions we will have a special lecture on permafrost in the U.S.S.R. as a foundation for structures." This lecture will be presented by Dr. Tsytovich, Professor of the Technical High School of Moscow and Director of the Institution of Permafrost of the Academy of Sciences. He is also a Corresponding Member of the Academy of Sciences, U.S.S.R. Professor Tsytovich.

(*Professor Tsytovich's special lecture appears on pp. 155–67.*)

CHAIRMAN VAN MIERLO

Professor Tsytovich, thank you very much for your remarkable lecture. Not only have you told us the fundamentals of permafrost, you have also given us fine examples of permafrost engineering. It indicates the high scientific level of the studies in your Institute. We hope you will continue, and that you will have great success with the study of permafrost in the future. I thank you on behalf of this audience for a fine lecture.

Gentlemen, because of the time, I suggest we continue immediately with the Session. The members of the panel are Professor Stefanoff from Bulgaria, Professor Brinch Hansen of Denmark, Professor Verdeyen of Belgium, and Dr. Ward from Great Britain. I now invite the General Reporter, Professor Mencl, to give a summary of his General Report.

General Reporter: V. MENCL (Czechoslovakia)

In contrast with the general upward trend in the number of books and articles the Society's members have to read, or rather would like to read, the number of contributions submitted to this Division has decreased since the Paris Conference. Nevertheless, the decreased number of contributions is balanced by a considerable number of scientific writings published since 1961. Let me mention at least the largest ones: the set of contributions to the Fifteenth Canadian Soil Mechanics Conference in 1961, containing an excellent review by Rochette; the instructive chapter on retaining structures by Tschebotarioff in *Foundation Engineering* edited by *Leonards* (1962), Kézdi's book, *Erddrucktheorien* (1963) and the activity of the German commission, Arbeitsausschuss für Ufereinfassungen (Commission for Protection of Banks). Also, a great many of the contributions to the Hungarian International Soil Mechanics Conference in 1964 were directed to the problems of earth pressure. What are the general impressions received from a review of the activity on the questions of earth pressure? They can be divided into those concerning fundamental problems and those concerning particular problems.

I. FUNDAMENTAL PROBLEMS OF EARTH PRESSURE ON WALLS

Concerning the problems of a fundamental character, it may be said that those who approach the problem from the point of view of the actual behaviour of the structure and those who seek the imaginary limit state of stability work in isolation. No mention therefore, is given to Brinch Hansen's equilibrium method in Tschebotarioff's review. An exception to this general attitude is the studies of the German Commission which I shall mention later.

To make the distinction clearer, let us consider an analogical problem—soil reactions underneath a continuous footing. One approach is to calculate the pressure distribution taking the deformation of the strip or slab in its actual state (in other words in "working" conditions) as a basis. This leads, for example, to the solution of an elastic beam on elastic supports. On the other hand, when designing cross-sections, the state of ultimate strength is considered (Fig. 1a). The other approach, the limit state of stability design, maintains the principle that only those loadings which would act when the ultimate stability would be nearly reached should be considered. Therefore, the soil reactions as well as the cross-sections are calculated with the same state of behaviour of the structure (Fig. 1b). Following this rule, the stability method (in the given case the "equilibrium" method) is to be advocated.

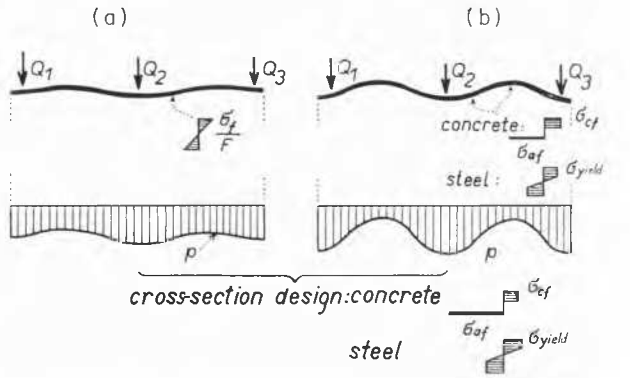

FIG. 1. Soil reactions under continuous footings.

One of the best ways to postpone a decision on a reasonable proposal is to suggest an improvement. An old Russian proverb states that the better is the enemy of the good. Nevertheless, the General Reporter's role is to comment and this Conference is held as a platform for discussion.

(1) The first criticism of the equilibrium method has already been presented by its author, Professor Brinch Hansen, namely, that it does not tell us the actual deformations. This is the same problem as that in foundations where the bearing capacity and the settlement are to be calculated separately.

As far as flexible bulkheads are concerned, if clayey soils are excluded, an instructive approach to this problem is contained indirectly in the contribution made by Thompson and Matich to the Canadian Soil Mechanics Conference in 1961 as well as in the discussion of it. Their view may be summarized by saying that large deflections have not been feared from the point of view of operation but merely as an indication of large stresses. Nevertheless, I took the liberty of submitting the question to the discussion as follows: Is stability the only criterion when designing flexible bulkheads? Does not deformation restrict the design, even when soil other than clay is concerned?

(2) The second group of comments concerns the simplicity of the method. As far as I know, no objections have been presented to the statement that the equilibrium method is clear and simple in principle. Nevertheless, comments concerning the simplicity of the practice of calculations have been made in studies connected with the activity of the German Commission. Consequently Schultze (1963), Windels (1963), and others tend to retain the modified Blum method, because of its simplicity, and to check it by comparison with results obtained with the methods of Hansen and Rowe. That is why the General Reporter, in accordance with his duties, has asked Professor Brinch Hansen to discuss the contribution made by Professor Schultze to the Hungarian Conference as well as Mr. Windels' article in *Bautechnik* (1963).

(3) The third group of comments may be directed to the problem of figures of rupture or pattern of rupture in a soil mass. It is believed that, when analysing the safety against ultimate failure, the solution is independent of the previous "history" of the structure. Yet some questions arise. Is it not the previous history which determines the failure plane in soil and rock mass? Is it not the state of outset of dilatancy, not far beyond the yield point, which predetermines the future failure plane? Is it not in the no-man's-land of strain hardening that the clue to the questions of stability is to be sought? Is not Professor Rowe's statement at the Paris Conference, "I have not got two boxes, an elastic one and a plastic one, and I do not have to put a problem in one or the other," to be remembered?

The large effect of dilatancy on earth pressure was probably foreseen by Bjerrum at the Paris Conference, and Rowe's article in the *Journal of the Soil Mechanics and Foundations Division* of the A.S.C.E. (1963) deserves close attention. An example of how the dilatancy affects results is shown in the measurements of *Arthur and Roscoe (5/1)*. In connection with dilatancy the application of the postulate of minimum potential energy is to be considered. Some questions then arise. Is it true that the position and shape of a failure line are given by the minimum of stability at the ultimate state? Is it not the maximum increment of entrophy at the moment of outset of dilatancy which governs the position and shape of the shear failure plane (Fig. 2a)? Should not the transient negative pore pressure occurring at the outset of dilatancy be considered when searching for the failure plane in clay (Fig. 2b)? Is it not the reason why the actual slip plane is not situated as deep as that corresponding to the minimum of stability, as observed by Ireland (1954), Skempton, Sevaldson, and others?

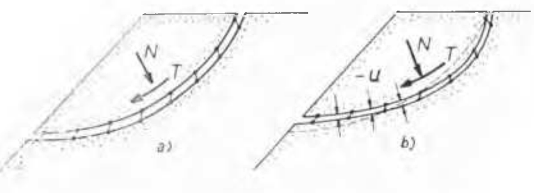

FIG. 2. Position and shape of shear failure planes.

The pattern of rupture differs in dense and in loose sands even when dealing with the problems of earth pressure (Fig. 3). The complicated interference of dilatancy with earth pressure questions is to be observed in the analyses by Rochette.

The instructive analysis of the energy laws in the theory of plasticity presented by Horne in *Progress in Solid Mechanics* (1961) should be noted as an important development.

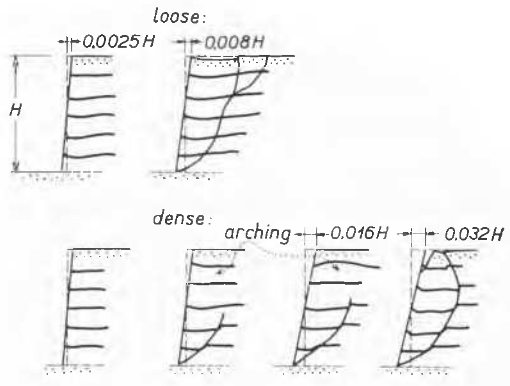

FIG. 3. Rupture patterns for loose and dense sands.

These problems, as well as those of flexibility and arching, are the reasons for the following discussion questions. What are the differences between the shear failure of a soil mass as compared with the bodies of classical theory of plasticity? What parts of the modern theory of plasticity are to be adopted by soil and rock mechanics?

The Safety Factor

A second question of general interest is the safety factor. The tendency of the modern limit state design of structures is to divide the safety factor into partial coefficients, to attach each immediately to the relevant factor of statical analysis, and to establish the instructions for how to determine the values of each. This tendency appeared in the adoption of partial coefficients by Brinch Hansen (1953), in the book by Keldysch (1953), and in the recommendations of the British committee for structural safety under the chairmanship of Professor Pugsley. In the period covered by this report, Professor Hueckel—the General Reporter to the Division in Paris—has contributed much to this question (Hueckel, 1963, 1964). I believe that there are no objections to this approach and that the time has come to reach an agreement on the function and volume of the individual factors. One may object that such a problem is general in character and therefore not exclusively within the competence of Division 5. However, as Dr. Kézdi observed (1963), the questions of earth pressure occupy a key position because they cover active as well as passive problems, and not only earth bodies are involved.

The advantage of the conceptions of the Russian and British schools is that a scheme of partial factors gives everyone the possibility of seeing through them, of analysing their value, and of making them proportional to the importance of the structure. Perhaps a special committee should investigate this question. Nevertheless, referring to the results of the Paris discussion, particularly to the deductions of Brinch Hansen, and also to the standards laid down in those countries where the principle of limit state design has been taken as a basis, two sequences of steps in static analysis are possible. One may object that it is too far-fetched to require uniformity in the calculation, but everyone who has gone through a Babel of discussions in cases of actual importance for lives and economy will probably confirm the advantage of such uniformity.

The first scheme has two alternatives: (1) To multiply the active loads by the factor of overloading, by the factor of ignorance, and by the factor of safety (in strict sense); to divide the passive loads by the factor of possible diminution; to calculate the guaranteed values of the means of strength parameters (also taking into consideration the possible long-term reduction of strength); to carry through the proper statical calculation. (2) To multiply the active loads by the factor of possible overloading; to divide the passive loads by the factor of possible diminution; to calculate the guaranteed values of the means of strength parameters (also taking into consideration the possible long-term reduction of strength); to divide the strength parameters (c and $\tan \phi$) by the factor of ignorance and by the factor of safety (in strict sense); to carry through the proper statical calculation. The two ways differ only slightly in approach. Some engineers prefer the first one because it gives a calculated line of rupture nearer to that which will actually develop if the structure fails. In both cases the results of calculation provide the required safety. The advantage of this scheme is that it can be universal for all divisions, while its disadvantage is that it contradicts established methods in "passive" problems both for foundation engineering and for the computations of passive earth pressure.

The second scheme also has two alternatives: (1) In "active" problems (active earth pressure, stability of slopes) the arrangement of the first scheme. (2) In "passive" problems (bearing capacity, passive earth pressure) another arrangement: to multiply the active loads by the factor of overloading; to divide the passive loads by the factor of possible diminution; to calculate the guaranteed values of the means of strength parameters (also taking into consideration the possible long-time reduction of strength); to carry through the proper statical calculation; to divide the results by the factor of ignorance and by the factor of safety (in the strict sense). The need for two methods in the second scheme may be criticized, but it has the advantage that at least in passive cases the factor of safety and the factor of ignorance influence the result proportionally. The economy of introducing a more elaborate, but at the same time more trustworthy, method of statical analysis can be directly examined. The second scheme also respects the methods already introduced in "passive" problems.

Stability of Rigid Walls in Clay

This is the third question of fundamental importance. It may be observed that even taking into account the detailed analysis presented by Šuklje and Vidmar (1961) and by Vidmar (1963) the problem of earth pressure in clay deserves further attention. This is necessary because the average active earth pressure after stopping the movement of a wall was found to correspond to a shear resistance of soil equalling about 66 per cent of standard shear strength. This value of 66 per cent may not be too much lower than the long-term strength of the material, but it is much higher than the resistance encountered in relaxation tests (Mencl, *et al.*, 1964).

Observations of Existing Structures

Because of the complexity of the problems connected with flexible walls the significance of measurements on existing structures was emphasized at the Paris Conference. New and interesting results have been presented by Endo (1963); in his paper the amount of the distribution of pressure on sheet pile walls during and after the excavation of a 7.9-m-deep cut is shown. When the results of measurements were compared with those based on existing rules, the measurements by Endo approach the conclusions of Tschebotarioff (1962). A set of observations was included in the contributions to the Fifteenth Canadian Soil Mechanics Conference (1961).

II. PARTICULAR PROBLEMS OF EARTH PRESSURE ON WALLS

Many articles and contributions have been presented in this field. Meyerhof (1961) discussed the very instructive results of investigations of skin friction between the wall material and different types of soils with different densities. *Verdeyen et Nuyens (5/13)* report the results of measuring the resistance in a model of the mechanism of rupture of the soil around anchor plates. It is interesting to see how the mechanism of rupture varies with the length of the anchors. If the anchors are short, the rupture zone in the soil in front of the anchor plates becomes connected with the zone of rupture behind the wall. The depth at which the anchor plate is embedded is of greater importance than the increase in the plate width.

Hueckel, et al. (5/5) studied the distribution of passive earth pressure on the surface of anchor plates. The results are an excellent supplement to the measurements of the resistance of the anchor plates presented to the conferences in London (Hueckel, 1957) and at Aachen (Hueckel, 1961).

The analytic study of the resistance of plane elements, in general, as a basis for the calculation of the resistance of the anchor plates is the object of an interesting contribution by *Biarez, et al. (5/2)*. As in the papers previously mentioned, the importance of the depth of embedment of the plate is shown, the relation of depth to plate width being taken as a criterion for the varying mechanism of rupture.

III. PROBLEMS OF SPECIAL INTEREST

Four problems are covered by the contributions.

Active pressure of sand subject to vibration. This problem, the subject of wide interest at conferences on earthquake engineering, is represented here by the contribution of *Ichihara (5/6)*. Measurements on an elaborate large-scale apparatus show that the displacement of the wall produces a decrease in the coefficient of earth pressure during vibrations of low frequencies, but that the point of application of the resultant is situated at nearly half the height of the wall and that the angle of friction on the wall is only 25 per cent of that found in static conditions.

The failure of a high crib wall. Of great interest for workers inexperienced in this type of wall (as is the General Reporter) is *Tschebotarioff's (5/12)* discovery of how susceptible these structures are to damage by settlement. In the case given, the wall was curved outwards (concave when observed from the backfill) which contributed to its failure. The decreased strength of backfill because of a small intermediate principal stress probably also contributed. Rules of practical significance are suggested.

Soil pressure on buried tubes. A goodly number of contributions to the problem of flexible conduit performance were presented to the Canadian Conference (1961). Three contributions to the present Conference show the growing interest in this field. *Habib et Luong (5/3)* analyse the danger of collapse by buckling of thin flexible tubes and evaluate the results of both theoretical analysis and intuitive experiments by introducing a formula for critical pressure. In a general picture of the interaction between tube and soil, *Luscher and Höeg (5/8)* show the three factors of mobilized resistance: the growth of lateral passive earth pressure during a small deformation, the beneficial intervention of the soil in the mechanism of buckling, and the arching of the soil. The influence of a varying system of embedment of the tube is studied by *Malishev (5/9)*. The author presents a method of calculation for practical use based on his formulas published in the *Proceedings* of the Brussels Conference (Malishev, 1958).

Effect of underground explosion is analysed by *Vesić (5/15)*, using a more or less classical approach. In this way a clear and simple method of calculating the radius of the cavity caused by the explosion as well as other factors is given. Although the author remarks that the assumed simplifications of the mechanism and the use of a statical rather than a dynamic approach may produce deviations from the actual state, the results are in agreement with observations of nuclear explosion tests.

IV. TECHNIQUE OF MEASUREMENT

Many of the contributions describe the results of tests in which new and ingenious methods of measurement were used or examined. Arthur, *et al.* (1963) introduced a method of calculation of stresses in a soil mass based on measured strains, using an X-ray technique for the determination of the strains. Recently, in another contribution, *Arthur and Roscoe (5/1)* have examined the necessity of using the X-ray technique and have concluded that the displacement at the plane of contact of the soil with a glass wall does not differ from that inside the body, if certain precautions are observed.

Although an excellent analysis and a list of earth pressure cells were published by Hamilton (1960), still other new modifications are forthcoming (Arthur and Roscoe, 1961). The use of effective earth pressure cells made possible the new findings of values for the pressure transmitted to a flexible bulkhead by a linear force at the surface of the backfill as described by *Verdeyen et Roisin (5/14)*. The results deserve attention because of a complete disagreement with the results of usual methods of calculation. The results were partly presented at the Paris Conference (Verdeyen and Roisin, 1961).

V. ROCK PRESSURE ON WALLS AND TUNNELS

The question of rock pressures, especially in the fields of the application of measurement and the invention of new techniques of measurements, has been the subject of broad and lively interest. The seven international meetings in America sponsored jointly by the Colorado School of Mines, the University of Minnesota, Pennsylvania State University, the University of Missouri, and the American Institute of Mining Engineers, as well as the International Conference on State of Stress in the Earth's Crust (Santa Monica, 1963) and the International Conference on Strata Control and Rock Mechanics (New York, 1964), have contributed greatly to this development. The activities of the National Committee of Great Britain, the Internationales Büro für Gebirgsmechanik of the German Academy of Science, and the Internationale Gesellschaft für Felsmechanik in Salzburg should also be mentioned, as should the Eighth International Congress on Large Dams (Edinburgh, 1964). If several books and many other publications are also considered, it is difficult to present a review of the activities in this field since the Paris Conference without approaching the task from a narrow point of view. In spite of controversial opinions, the General Reporter intends to show that in many fields the theories used in soil mechanics problems are very helpful in the study of rock pressure problems. In this way the tradition of the Paris Conference can be followed (see discussion in Vol. 3, p. 165).

Tunnelling in Soft Ground

There is no doubt that this problem must be studied by using the methods of investigation and analysis of soil mechanics. Three contributions belong to this category. *Ward and Thomas (5/16)* discuss the results of measurements of earth pressure on tunnel linings in the London clay. As in the preceding reports of these authors, pressure values amounting to 75 to 100 per cent of the hydrostatic overburden pressure were recorded. The great number of vibrating-wire strain gauges used to perfect the investigation deserves attention. The explanation of the elliptical deformation of the rings seems a little too elaborate. This phenomenon can be probably explained by the need to mobilize the passive earth pressure in the horizontal direction before the long-term hydrostatic conditions are re-established.

In the contribution of *Sutherland (5/11)* a statical analysis of a novel procedure is shown, namely, that of driving shafts (with a diameter of 7.8 ft) by jacking through the sand mass (8 to 23 ft in thickness) upwards from tunnels erected underneath the sea bed. The force necessary for the jacking operations was carefully investigated using the similarity with other problems of soil mechanics as well as the results of measurements on models. This force was verified by the findings at the site. As in many other cases of pushing blind pipes through dense sandy soils, the force necessary was greater than that determined by using existing theoretical formulae. The comparison of the three ways of investigation is of great interest, especially the fact that the results of model tests proved correct in a quantitative manner.

How helpful the ingenious methods of theoretical soil mechanics are in predicting the behaviour of a tunnel in soft ground is shown in an excellent way by the contribution of *Bent Hansen and Nielsen (5/4)*. This contribution is of great interest for the method of investigation used and for the development of new solutions in the theory of consolidation.

Shear Strength of Rocks

It is in the domain of shear strength that the philosophy of soil mechanics should influence ways of thinking in rock mechanics. The considerable function of dilatancy in the magnitude of shear strength where the movement occurs across strata is nearly unknown to investigators in rock mechanics (Mencl and Paseka, 1963), although in underground conditions it may contribute to the arching of rocks, to the growth of concentrated stress resulting in rock bursts, and other such phenomena. The beneficial effect of bolting is very much a result of dilatancy. The influence of the strength of blocks on the occurrence of dilatancy should be considered without losing sight of the example by Bird (1961) of small values of shear strength of gravel with weak grains. The change in the behaviour of rocks at great depths may be explained as a loss of dilatancy by analogy with the behaviour of sand at very high pressures (Vesić and Barksdale, 1963).

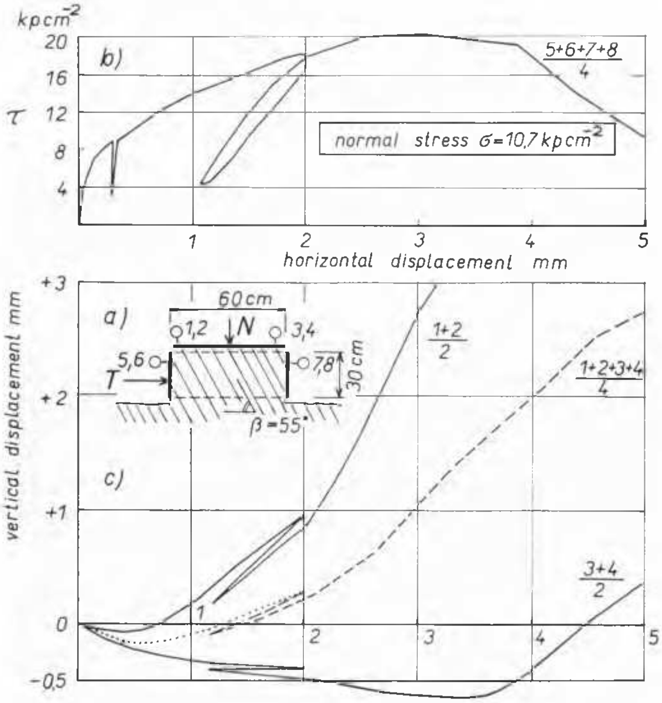

FIG. 4. Results of shear test performed on foliated gneiss.

The General Reporter thinks that it is worthwhile to observe the results of a block shear test. Fig. 4 presents a result with thinly foliated hard gneiss. The lower diagram indicates the vertical displacement of the four corners of the plate pressed by the vertical force. Is it not of the same character as a shear test on dense sand?

On the other hand, soil mechanics may profit very much from the findings of rock mechanics. In rock mechanics, for example, it has been proved that the long-term strength of rock material specimens is very much influenced by the boundary conditions of the test (Potts and Hedley, 1964; Mencl, *et al.*, 1964) and particularly that relatively high cylinders in uniaxial and triaxial tests give pessimistic results.

Theory of Models

Because of the complexity of the problems in rock mechanics the use of models is still increasing. The contribution of *Oberti and Fumagalli (5/10)* from the laboratory in Bergamo, Italy, illustrates this method of investigation in an excellent manner. Because of the laws of scale, the rock is represented in the models by a soil. However, to find a soil really equivalent in mechanical behaviour to a rock, the knowledge of the properties of both materials is necessary. In this regard the General Reporter would recall the success of his colleagues at the Mining Institute of the Czechoslovak Academy of Sciences in modelling which was attained by introducing the dilatancy as a principal factor.

Measurements in situ

As mentioned previously growing activity in the field of measurement of stress and deformation in rock and of its support at sites is helpful in the understanding of the mechanical behaviour of rock. The investigations by Potts (1964) and his colleagues in the field of mining as well as the measurements by Rabcewicz (1964) in tunnelling are successful examples.

Krsmanović and Buturović (5/7) present the results of the measurement of pressure of weak rock on a tunnel lining in a 650-day period, using 10 to 12 flat jacks in each section. The results, as well as the deformation and mode of failure of the lining, indicated in principle that the pressure existed in the abutments at first, and that the pressure in the crown increased only in the course of time. The General Reporter recalls that the same phenomenon was observed (Mencl and Mencl, 1964) whenever "classical" methods with crown-bar timbering and unreinforced concrete linings were used in weak rock. A timely grouting of the gap behind the lining in the crown is of considerable aid.

PROPOSALS FOR DISCUSSION

The following subjects are suggested for discussion.

1. What are the differences between the shear failure of earth bodies and the bodies of the common theory of plasticity? Are they worth considering? What methods of the modern theory of plasticity are helpful when taking into consideration the phenomena of dilatancy and flexibility of soil and rock bodies?
2. Is it of practical use to seek uniformity of calculations if the limit state of stability is concerned, and if so, what scheme is to be recommended?
3. Relaxation phenomena in earth pressure problems and methods of calculation of earth pressure in cohesive soils.
4. Further development of the methods of design of flexible walls.
5. The similarity in the mechanical behaviour of soils and rocks and its influence on the methods of analysis of rock pressure.

SUJETS DE DISCUSSIONS

Il est recommandé que les sujets suivants soient considérés pour fin de discussion.

1. Quelles sont les différences entre la rupture de cisaillement des matières du sol et celles de la théorie courante de plasticité? Ces différences méritent-elles d'être considérées? Quelles méthodes de la théorie moderne de plasticité sont utiles lorsqu'on tient compte des phénomènes de dilatabilité et de flexibilité des matières du sol et du roc?
2. Est-il nécessaire de viser à l'uniformité des calculs lorsqu'il est question de l'état limit de stabilité, et si tel est le cas, quelle méthode devrait être recommandée?
3. Les phénomènes de relâchement des problèmes de la poussée de terre et les méthodes de calcul de la poussée des sols cohérents.
4. L'élargissement des méthodes de conception des parois flexibles.
5. La similitude du comportement mécanique des sols et des roches et son influence sur les méthodes d'analyse de la poussée des roches.

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CHAIRMAN VAN MIERLO

Thank you Professor Mencl. You have already mentioned the different proposed subjects for discussion. In the panel are different experts on these subjects and I propose that the discussion now opens.

Panelist: G. STEFANOFF (Bulgarie)

Le premier sujet de discussion, proposé par M. le professeur Mencl, aborde une question à laquelle il a donné une considération spéciale dans son Rapport général. Cette question est liée avec les deux questions à caractère fondamental, nommées ainsi par M. Mencl. Je suis parfaitement d'accord avec son opinion, particulièrement en ce qui concerne les déformations qui sont nécessaires pour que la poussée ou la butée des terres commence à agir.

Malgré que les problèmes de la poussée des terres soient parmi les plus anciens de la mécanique des sols, ayant en vue les théories classiques de Coulomb (1773), de Rankine (1857), etc., malgré que les méthodes de calcul de la poussée ont été précisées et raffinées durant presque deux siècles, en pratique l'ingénieur continue toujours à calculer avec la poussée et la butée des terres.

Dès sa création, la science relativement jeune de la mécanique des sols posa l'idée et introduisit le calcul des fondations d'après les déformations (les tassements). Cette méthode de calcul limite—état limite de déformation (nommée "limit state design—limit state of deformation" par le Rapporteur général) fut acceptée plus tard par les autres disciplines du génie civil, mais dans le domaine de la poussée des terres, sous ce rapport la mécanique des sols, sauf quelques essais réussis, reste en arrière. Ceci est confirmé par la Conférence de Bruxelles (1958), ainsi que par les discussions à la Section 5 du 5e Congrès International (1961) et par le Rapport que nous discutons à présent.

On sait que la solution précise est très compliquée, et jusqu'à présent il n'existe pas de solution appropriée qui soit appliquée en pratique. Malgré cela, l'ingénieur n'est pas toujours tout à fait sans ressources lors de la résolution des problèmes pratiques de ce genre. Je vous prie de me permettre de présenter un tel exemple. Il s'agit du barrage Georges Dimitroff en Bulgarie où ces problèmes surgirent d'une manière très urgente. La hauteur du barrage est de 42 m, son volume—470 000 m.cu., le contenu du réservoir—140 000 000 m.cu. Le barrage a été construit sur un terrain hétérogène. Le flanc droit et le lit du fleuve se composent de granites et de granites gneissiques, le flanc gauche, de con-

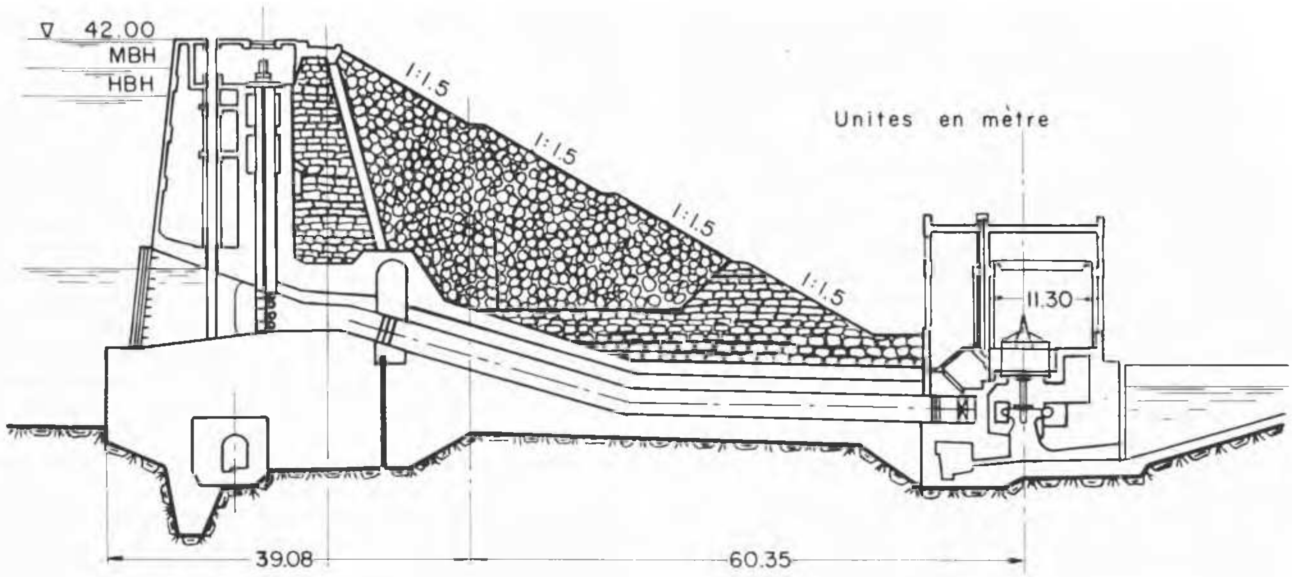

FIG. 5. Coupe transversale du barrage.

glomérats tertiaires alternant avec des grès argileux à gros grains. Les uns et les autres sont disloqués et fissurés en profondeur. A cause de cela on a choisi un type de barrage analogue à celui de Shing-Mun en Chine—combiné en plan et en section transversale. La partie principale du barrage est édifiée en maçonnerie de pierres avec mortier du côté amont et en remblais de soutènement pierreux du côté aval (fig. 5). Lors du calcul surgit le problème de l'ordre de grandeur de la poussée du remblai pierreux sur la maçonnerie (réservoir vide), ainsi que de la butée en direction inverse (réservoir plein). Les calculs éxécutés à partir de surfaces de glissement planes et courbées ont montré que lors de l'action totale de la butée la stabilité du barrage est plusieurs fois garantie. Mais comme l'interaction entre les deux corps de soutènement, c'est-à-dire le grade de mobilisation de la butée en rapport avec la déformation, n'a pas pu être établie théoriquement, on ne savait pas sur quelle part de la butée on pouvait compter. Malgré cela on a décidé qu'en présence des conditions existantes, la sécurité de l'ouvrage était suffisante. Pour assurer un contact constant entre les deux corps on a placé entre eux une semelle de sable d'une épaisseur de 1,5 m. L'exploitation parfaite du barrage pendant dix années a justifié les solutions acceptées.

Je voudrais bien remarquer que les décisions prises ne pourront pas être extrapolées aux barrages plus hauts sans études théoriques et, éventuellement, expérimentales. Avec cet exemple je voudrais démontrer que malgré qu'il manque de solutions théoriques aux questions posées, néanmoins l'ingénieur dans certains cas peut trouver une issue au problème.

Panelist: J. BRINCH HANSEN (Denmark)

The General Reporter has asked me to discuss two subjects: safety factors in limit designs, and methods for design of sheet walls. Accordingly, I shall discuss them both briefly.

SAFETY FACTORS IN LIMIT DESIGN

If I have understood the General Report correctly, Professor Mencl mentions three different ways of introducing safety factors in limit designs: (1) to apply safety factors to the loads only; (2) to apply reduced safety factors to the loads, and additional safety factors to the shear-strength parameters; (3) to treat "active" problems in the first way, but for "passive" problems to apply reduced safety factors to the loads, and other safety factors to the passive earth pressure or bearing capacity.

The third method seems—in spite of its extensive current use—rather illogical to me. Both active and passive earth pressures are functions of the strength parameters of the soil, and any uncertainty in these pressures is, consequently, best covered by applying safety factors to the strength parameters. Since active pressures can be greater than calculated, just as passive pressures can be smaller, I have never seen the logic or the necessity of applying a safety factor to the passive pressure, while leaving the active pressure unchanged.

Moreover, clear-cut active and passive pressures occur only in special cases. For a rigid wall, which rotates about a point between its top and bottom, it is actually impossible to say where active pressure stops and passive begins. The earth pressure diagram forms one continuous curve. Also, in a deep circular slip surface under a slope, the conditions at the upper end are quite similar to a case of active earth pressure, whereas at the lower end they are just as similar to a case of passive pressure. Of course, it is impossible to say where one condition stops and the other begins. Another example of a mixed case of "active" and "passive" problems is a foundation located near the top of a slope. With a variation of the foundation load the problem changes quite gradually from one condition to the other. For these reasons I consider it impossible to distinguish logically between "active" and "passive" cases, except by quite arbitrary definitions. The dilemma solves itself, however, as soon as safety factors are applied to the shear-strength parameters.

We will find a somewhat similar situation, if we analyse the distinction between "active" and "passive" loads. In a simple stability problem the "passive" loads are presumably due to those earth masses having a stabilizing effect, whereas the opposite applies to the "active" loads. However, in one problem a particular soil mass may be considered "active," when circular slip surfaces are employed, but "passive" if spiral slip surfaces are used. Moreover, in complicated plasticity problems such as the above-mentioned rigid wall, it may be almost impossible to determine which soil masses in the rupture figure are "active" and which "passive." The only simple way out of this dilemma is to apply no safety factors to earth weights or other dead loads. This is also justified by the fact that such loads can be calculated rather accurately. Live loads, on the other hand, should be provided with safety factors. However this means—if we want a generally applicable principle—that the safety factors cannot be applied to the loads alone, as in the first method, since live loads are negligible in many stability problems. The second method remains and is, in my opinion, the only one with general applicability. Safety factors must be applied both to the shear parameters and—in principle—to the loads. However, as I have explained, for simplicity no safety factors should be applied to dead loads.

This is actually the principle already employed in most modern slope stability calculations. All we have to do to achieve the desired unification is to apply the same principle to earth pressure and bearing capacity problems. This is quite easily done, and is the general principle incorporated in the new Danish Code of Practice for Foundation Engineering (1965).

DESIGN OF SHEET WALLS

The design of anchored sheet walls can be done in two different ways: either as a limit design, that is with a certain safety against ultimate failure, or as a working stress design, that is with a maximum value for the actual stresses occurring under working conditions. Rowe's method is, at present, the best available one of the second type. My only real objection to it is that his "flexibility number" is defined in such a way that it is not dimensionless, which makes an extrapolation from model tests to prototype somewhat doubtful.

My own earth pressure methods are of the first kind and have, as one of their advantages, the distinction of taking account of both the statical and kinematical conditions in the rupture figure.

The so-called classical methods, Blum's for instance, are not really of one type or the other because they operate with Coulomb's or Rankine's earth pressures, which—for an anchored sheet wall—correspond neither to actual working pressures nor to pressures in a kinematically possible state of failure. Consequently, they do not in themselves lead to acceptable designs and are, instead, provided with empirical correction factors, such as a factor 2/3 on the moments found in Blum's method.

In the *Empfehlungen* of the German Ausschuss Ufereinfassungen, Blum's revised method is primarily recommended, but it is mentioned that Rowe's or my method may be employed too, presumably in special cases. Schultze has made comparisons between the three methods and has found approximate agreement in the investigated cases. Although he admits the theoretical superiority of Rowe's and my method, he recommends the use of Blum's revised method on account of its simplicity and rapidity. However, in these days of electronic computers it can hardly be considered relevant to favour a method of doubtful theoretical basis on account of its simplicity alone. A Danish engineer has already coded my sheet wall methods for electronic calculation, so that several such calculations can now be made in a few minutes. In this way the computers allow us to carry out, cheaply and quickly, the best calculations we can devise, almost irrespective of their complexity. But this implies an obligation for us to use such methods instead of the old crude ones. The days when it was considered necessary to sacrifice quality for simplicity should definitely be over.

Panelist: J. VERDEYEN (Belgique)

Monsieur le Rapporteur général m'a demandé de faire quelques remarques concernant l'exécution et le calcul des murs flexibles. Je me propose de traiter ce problème sous deux aspects: le premier est celui du calcul et de la déformation des palplanches métalliques proprement dit, le second est celui des tassements ou déformations des massifs soutenus et de leur influence sur les constructions avoisinantes.

Le calcul d'un rideau de palplanches doit se faire en tenant compte de sa déformation qui est fonction de ses liaisons, de sa raideur propre et de la nature du sol. Ces divers éléments influencent la loi des pressions sur le rideau. En ce qui concerne les liaisons on doit distinguer: (1) le rideau libre en tête pour lequel il n'y a pas d'effets de voûte et pour lequel le calcul est classique et facile; (2) le rideau ayant un appui en tête qui détermine deux zones d'appui pour la palplanche, dans le bas on a soit un encastrement, soit un appui simple, dans le haut on a soit un appui indéformable, soit un appui plus ou moins déformable, ce qui est par exemple le cas des ancrages. Si l'appui est déformable il y a réduction des pressions en tête, mais, par contre, une augmentation des pressions sur la fiche d'où une augmentation de celle-ci. Dans chaque cas, on doit examiner la solution la plus avantageuse et prévoir, par exemple, un appui déformable qui sera moins sollicité ou prévoir un appui indéformable dont la sollicitation sera importante. Les essais faits au laboratoire de l'Université de Bruxelles ont montré, en particulier, que dans le cas d'un rideau calculé comme simplement appuyé dans le sol et dont l'appui supérieur devient déformable, pour l'une ou l'autre cause, il y a risque réel de dérobement du pied par effet de bêche.

Lorsque l'on procède à une excavation nécessitant plus d'un appui, il y a lieu de calculer les rideaux de palplanches métalliques pour les différentes phases d'exécution des travaux: d'abord la palplanche encastrée, puis la palplanche tenue en tête, puis enfin la palplanche sur appuis multiples. Les lois de répartition des pressions sont fonction de ces phases.

Lorsque l'on envisage la déformabilité proprement dite des palplanches, des essais en cours à l'Université de Bruxelles semblent montrer que la résultante des pressions augmente avec la raideur des palplanches, toutes autres choses égales d'ailleurs.

Pour ce qui est de la nature du sol, il est évident que pour les sols pulvérulents, plus l'angle de frottement est grand et plus la compacité est grande, plus petite est la résultante des pressions. Par contre, l'effet de voûte est plus marqué. Lorsqu'un sol pulvérulent est à une compacité inférieure à la compacité critique, la déformation du rideau doit être limitée. En ce qui concerne les sols cohérents, le problème est beaucoup plus compliqué et il ne semble pas possible d'énoncer des règles générales.

Le deuxième aspect de la question concerne les tassements ou déformations du massif soutenu. Lorsque l'on procède à l'exécution d'une excavation à l'abri d'un rideau de palplanches, on doit limiter les tassements de terrain voisin afin d'éviter d'influencer les constructions qui se trouvent à proximité de la fouille. Lors de la construction du tunnel de la Jonction Nord-Midi et des tunnels de la Petite Ceinture à Bruxelles, malgré les nombreuses précautions prises, certains immeubles contigus à la fouille se sont fissurés. L'importance, la plus ou moins grande concentration et la position des charges extérieures doivent être prises en considération. Elles ont une influence sur les figures de rupture.

A notre connaissance on n'a pas encore énoncé de critères permettant de limiter ces effets. Le problème reste ouvert, il serait intéressant que des recherches soient abordées dans ce sens.

Panelist: W. H. Ward (Great Britain)

I have noticed in recent years that there has been, firstly, a revival of interest in model studies of earth pressure problems, and secondly, a growing use of instrumentation on full-scale earth-retaining structures. The reason for these two activities is undoubtedly that classical ideas on earth pressure do not enable us to describe the deformations and stresses in the structures we build with sufficient accuracy, even if they do permit us to avoid failures.

I would like to make a few comments on the experimental techniques being used in models. Many model tests are set up specifically to provide quick answers to some special construction problem and typical of these is the paper by *Sutherland (5/11)* which resulted from his need for an estimate of the load required to raise a shaft upwards through sand. This type of model study is of great practical value and is of the simple classical type where there is a continuous state of failure. Only the ultimate load is sought. Several other recent papers have described studies of this nature.

Just lately, we have also seen the introduction of much more elegant model studies using new and elaborate techniques to measure the stress and strain distribution in both the structure and the earth mass at all stages of deformation, and attempts to correlate the model data with the stress-deformation characteristics obtained from shear tests. I refer in particular to the work of Roscoe and his colleagues at Cambridge and to that of Rowe at Manchester.

In basic model work of this type it is, of course, necessary to obtain a complete picture of the development of *all* the forces acting on the earth structure *and* their distribution, otherwise a complete understanding of the problem is impossible. For example, we need to measure both the normal and the tangential distribution of the forces acting on a wall. Some contributions to our meeting are unfortunately deficient in this respect; for example, in the paper by *Hueckel, et al. (5/5)* only the distribution of the normal forces has been measured.

Model earth pressure studies have been limited on the whole to cohesionless materials and to quick tests on soft clays. The more complex problems involving creep, drainage, and relaxation of clays are exceedingly difficult to handle in models, and what little data we have on this subject has been obtained mainly from full-scale measurements on structures.

The work we have been doing at the Building Research Station on the structural behaviour of tunnel linings in London clay falls in this category and the General Reporter has asked me to enlarge on this topic.

When an unlined circular hole is dug in the London clay at a great depth, a deformation of an elastic nature occurs simultaneously with the excavation. The stress concentration created is in general not sufficiently large to cause collapse, and an unlined hole will remain open for many years; there is no earth pressure problem. When a lining is inserted without force into the hole it develops a loading quite slowly by virtue of a swelling process of the clay. In my view, this has nothing whatever to do with the classical theories of earth pressure involving rupture of the ground.

I cannot, therefore, accept the General Reporter's suggestion that the elliptical squatting of the lining can be explained by the need to mobilize the passive earth pressure in the horizontal direction. One might well ask why the lining squats at all, *or* why does it not elongate in the vertical direction?

It should be mentioned perhaps that the clay has been severely unloaded by the excavation and that there is a strong but invisible drainage of water towards the hole. Thus the clay continues to swell and deform as a new state of effective stress and water pressure is established at the lining boundary. In this connection we cannot afford to overlook the anisotropic and fissured properties of the clay; they are basic to any real understanding of the loading mechanics of tunnel linings.

Chairman van Mierlo

Gentlemen, the fifth item for discussion, "The similarity in mechanical behaviour of soils and rocks and influence of soil mechanics on the methods of analysis of rock pressures," has not been discussed. It is left to the audience to comment on this item. I now suggest that we have a break of about fifteen minutes.

(There followed a brief intermission.)

Chairman van Mierlo

We now continue with the oral discussions.

A. Vesić (U.S.A.)

This is a brief comment on the differences between the shear failure of real soils and rocks, and the bodies used in the common theory of plasticity. In proposing this subject for discussion, the General Reporter has pointed out the importance of dilatancy or the volume change characteristics of the masses involved in shear. His remarks may find additional support in the evidence that we have assembled over the last ten years on the mechanism of soil failure

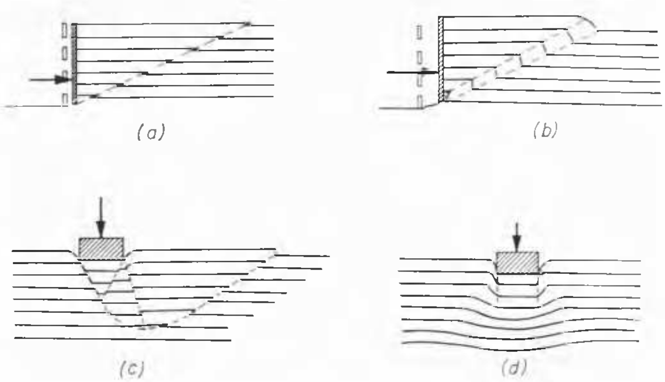

FIG. 6. Failure patterns: (a) and (c), dense sand; (b) and (d), loose sand.

behind anchor plates, or beneath vertically loaded foundations. Numerous experiments, in which the deformation patterns were recorded in different soil types under a variety of loading conditions, show that the failure patterns in such relatively incompressible soils as dense sand or saturated clay under quick loading bear a striking similarity to those of metals and of the ideal bodies of the theory of plasticity. However the analogous patterns in relatively compressible soils such as loose sands, silts, and soft clays under very slowly applied loads may be considerably different.

Sometimes, as in the case of anchor plates, the differences appear to be only in the size of the slip zone and in the relative displacement needed to produce failure. Thus, a plate pushed against a mass of dense sand may fail at a very small displacement, say 1 per cent of the plate height, producing a Coulomb type of wedge sliding over a real slip surface (Fig. 6a). The same plate pushed against a mass of loose, compressible sand may fail at displacements comparable to plate dimensions, producing a failure pattern such as shown in Fig. 6b, in which the slip surface has degenerated into a slip zone of finite dimensions. In such instances, where some geometrical similarity still exists between theoretical and actual deformation patterns, the conventional computation of failure loads still can make some sense, although the values computed are of little interest for design because of the great displacements necessary to reach failure.

In contrast to the preceding case, the failure pattern in a relatively compressible soil or rock may indeed be basically different from that occurring in an ideal solid treated by the theory of plasticity. The best example of this kind is that of the bearing capacity failure under a shallow or deep foundation (Figs. 6c and 6d). A shallow foundation on very dense sand may produce a pattern such as that shown in Fig. 6c which is obviously similar to the general shear failure pattern usually treated by the theory. However, the same shallow foundation on loose sand, any deep foundation, or any impact-loaded foundation would produce a basically different pattern (Fig. 6d), that we have named "punching failure" or *rupture par enfoncement*, (cf., de Beer and Vesić, 1958; Vesić, 1963). In such instances, obviously, conventional compuations, based on theoretical failure patterns for incompressible solids, make very little sense. Limited success in analysis can be achieved again by assuming failure patterns that bear some similarity to the observed ones (Vesić, 1963).

It should be added here that the failure patterns appear to be affected by the *relative* compressibility of the soil mass in question, that is a quantity such as our *rigidity index* (cf., Vesić, 1964) or generally a ratio of the modulus of deformation of soil to its shearing strength. Since this relative compressibility of some soils, such as sands, increases with pressure, changes in scale may also bring changes in failure patterns. Thus, a failure pattern should not be inherently associated with a certain soil type. The fact that a certain pattern was observed on small-scale models does not necessarily mean that the same pattern is to be expected if the scale is drastically increased. Here we have an entirely new area of future research in which large-scale models and full-size tests will play a predominant role.

A final comment will be made concerning the analysis of earth pressure analysis and determination of ultimate loads. In view of the magnitude of displacements needed to reach "failure" in a relatively compressible soil, it may be indeed questioned whether the "ultimate loads" so estimated are really of any practical interest. If loads corresponding to displacements at which failure occurs in relatively incompressible soils were to be determined, the use of the theory of elasticity would be more promising, since the displacement patterns observed in that range of loads definitely resemble more closely the patterns predicted by the latter theory.

It should be understood that, whatever course in analysis is taken, only a gross approximation of a palliative nature is achieved. What is in reality needed for earth pressure computations dealing with compressible soils is a new, improved plasticity theory, perhaps organized along lines similar to a non-linear, large-strain theory of elasticity.

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S. TAKAGI (U.S.A.)

I believe that a rigorous formulation of the plasticity of soils is possible. The contemporary theory of plasticity, however, was developed mainly for metals and is based on an assumption that appears to be inappropriate to soils.

The principal axes of strain rate and stress are assumed to be coincident in the contemporary theory of plasticity (Saint-Venant's postulate, see Hill, 1956, p. 38). This assumption may be used for a material that has cohesion but no friction. No volume change is caused in such a material during plastic deformation—a property appropriate to plastic metals. However, dilatancy is always caused in a material that has friction and cohesion during plastic deformation fulfilling the assumption (Drucker and Prager, 1952) —a property that is not appropriate to soils.

It has been proved (Takagi, 1962) that a plastic material obeying the Coulomb criterion of yielding during plane deformation—called $c-\phi$ material—causes either expansion or compression according to the magnitude of the angle α between the principal axes of the strain rate and of the stress. Introduction of angle α has a profound effect on the plastic deformation of $c-\phi$ material. It has been shown for plane deformation (Takagi, 1962) that (1) α is determined so that a solution may exist under given boundary conditions, and (2) the boundary conditions influence the stress-strain rate relationship which contains α. Existence of an angle α not equal to zero is a manifestation of anisotropy caused by the deformation. The theory of plastic deformation containing α is a general theory of plasticity, whose special case is the theory of plastic metals.

The mathematical difficulty of constructing the general theory of plasticity has not yet been surmounted, however.

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K. H. ROSCOE (Great Britain)

Perhaps the most remarkable discrepancy in soil mechanics today is the emphasis placed upon the dilatant characteristics of soils when discussing their shear properties and the automatic presumption that they are non-dilatant (or have an impossibly large artificial dilatation) when using these shear properties in earth pressure (and similar) calculations. Most current methods of solving earth pressure problems (apart from solutions based on the theory of elasticity) are concerned only with conditions of the soil at failure. The soil is then, at failure, presumed to have the idealized properties of constant cohesion (c) and constant internal friction (ϕ) which are fully mobilized either on sliding surfaces throughout the whole of the displaced soil mass or along the entire length of an assumed arc of failure within the soil. Furthermore, if the soil failure is caused by the load imposed by a structure such as a retaining wall it is presumed that the soil slides upon the structure and a constant angle of wall friction (δ) is developed at all points of contact. In such a problem the soil behaves passively and failure is considered to correspond to conditions when the soil exerts a peak (or maximum) force on the structure.

Having made the presumption that c, ϕ, and δ are constants, Coulomb's equation is then applied to the sliding surfaces within the soil mass. Since Coulomb's equation is merely a restatement of Amonton's (1699) law of limiting friction for contact sliding between solid bodies, this procedure is equivalent to considering that the soil on each side of an assumed sliding surface is behaving as a solid, rather than dilatant granular, body. A further consequence of the application of Coulomb's equation to a sliding surface in a soil mass is that it automatically presumes that the sliding surface is one of maximum obliquity of stress.

In direct contrast to all the above assumptions it is generally accepted from the results of laboratory shear tests that (a) all soils (except under very rare circumstances) dilate (that is, change voids ratio) when sheared, provided conditions do not prevent such a change; (b) the limiting cohesion and/or internal friction of all soils alter as their voids ratio changes; (c) the rate of dilatation (that is, change of voids ratio) attains a maximum at peak load conditions for soils which expand when sheared with full (and in many cases partial) drainage permitted. Such soils when subjected to continued strain beyond peak load conditions attain a critical state at which they cease to dilate and possess an "ultimate" strength that is less than their "peak" strength. (Some soils may proceed, with even further distortion, from the critical state to a residual state in which full particle orientation is developed in the region of a sliding surface [for further discussion, see Roscoe, 1964].)

In nature, all overconsolidated clays and almost all deposits of cohesionless soils expand during shear and hence the solid body sliding concepts of constant c and constant ϕ on any assumed sliding surface are not likely to apply to "peak" load conditions. Even at "ultimate" failure of such soils, when all dilatation has ceased, the concept of a constant ϕ being developed on a sliding surface would only be justified if all elemental areas of this surface coincided with planes of maximum obliquity of stress. In general, an engineer should be interested in working loads well below the collapse loads corresponding to peak strength conditions. At these lower stress levels all soils will in general be dilating and will behave in a manner which is far removed from the present application of Coulomb's solid body sliding concepts.

It is evident that there is a great need for reliable data at all stress levels concerning the pattern of deformation and mode of failure of soil masses as well as of the pattern of contact stress distribution between soil and structure. Two tools have been developed at Cambridge to assist in obtaining such data and the results of some preliminary tests have been described by Arthur, James, and Roscoe (1964) where earlier references will be found. The first tool is a small earth pressure cell capable of measuring the position, magnitude, and direction of the over-all force vector on its active face. The face of any structure that is in contact with the soil can be built up of these load cells and hence normal and shear contact stress distributions can be separately determined as can also the local magnitudes of the angle of wall friction. This latter quantity is denoted by δ' to distinguish it from the average over-all angle of wall friction δ on the whole of the contact face. The second tool is X-ray and radio-isotope equipment. Both can be employed to observe density changes throughout the deforming soil mass. X-rays have also been used to observe the displacements of lead shot placed at the nodes of a network in the soil sample and hence the pattern of strain of the soil mass can be obtained at any desired stage of loading of the soil.

R. G. James (1965) has recently carried out a series of model wall tests under conditions of plane strain on Leighton Buzzard sand (18 > BS sieve > 25) at various initial voids ratios ranging from 0.56 to 0.79. The wall face, OB in Fig. 7, is 13 in. high and is built up of 18 load cells arranged in three parallel rows. It is rotated towards the sand mass about the axis O. The over-all internal dimensions of the glass-sided sand container are 8 ft × 5 ft × 7½ in. and it rests on a large independent antivibration mounting. The automatic recording and tape-punching equipment is shown on the left of Fig. 7. While James' data concerning strains and over-all contact forces (as distinct from stresses) broadly confirmed the earlier findings of Arthur this series of tests was the first in which both normal and shear contact stress distribution had been observed. Unfortunately, in the time and space available, it is only possible to give the briefest of indications of the nature of these results.

FIG. 7. Laboratory earth pressure test rig and associated recording equipment, showing rotating wall OB.

Fig. 8 shows the wall OB after rotation through an angle θ when the over-all force on the wall is P acting at depth d and the over-all angle of wall friction is δ. The maximum value of P is P_m. The local angle of wall friction at any depth Z is δ'. In Fig. 9 the values of δ', as recorded by the load cells, are plotted at depths Z corresponding to the mid-height of each cell for three stages of loading as represented by $P/P_m = 0.58$, 0.83, and 0.97 respectively in a test on an initially dense sand sample ($e_0 = 0.57$). It can be seen that the pattern of distribution of δ' changes little with change of load, and that for $Z = 3$ in. the value of δ' is 50° acting upwards on the wall. As Z increases the magnitude of δ' decreases approximately linearly to below 10° acting upwards when $Z = 11\frac{1}{2}$ in. Similar behaviour was found at all initial voids ratios but the value of δ' was everywhere about 15° less for a loose sample ($e_0 = 0.79$) than the figures quoted above.

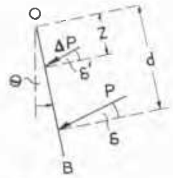

FIG. 8. Over-all (P, δ, d) and local (ΔP, δ', Z) soil contact force distribution on wall OB.

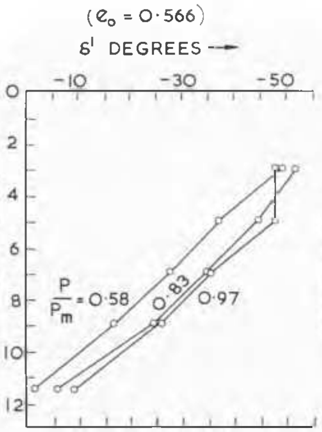

FIG. 9. Variation of local angle of wall friction δ' with depth Z on wall at three over-all load levels $P/P_m = 0.58$, 0.83, and 0.97.

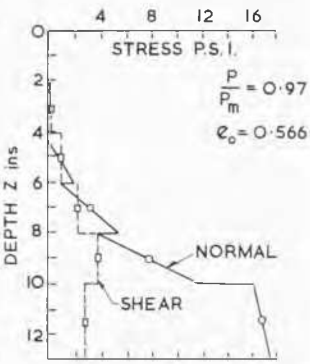

FIG. 10. Distribution of normal and shear stresses on a wall when approaching peak load conditions.

The distribution of the normal and shear stresses for $P/P_m = 0.97$ in a dense sand ($e_0 = 0.57$) is shown in Fig. 10. These data have been plotted by assuming linear stress distributions over individual load cells rather than by constructing smooth continuous curves since the former method is the least likely to distort the actual cell recordings. The general patterns of both stress distributions were very similar, but of different scales, at the lower load levels and also for all initial voids ratios. It can be seen that the greatest shear stress occurs much higher up the wall than the greatest normal stress.

In all these tests it was found that at loads corresponding to about half the peak load not only is the general pattern of contact stress distribution clearly defined but the general pattern of strain throughout the soil mass is also quite evident. Thereafter as the load was increased to the peak value both patterns changed little and the magnitudes of all quantities increased in approximately constant proportion. It was found that this pattern of strain extended over a curved wedge-shaped sector increasing in width with distance from the toe of the wall to the sand surface. Two tests were carried out under as nearly as possible identical conditions; the initial voids ratios were 0.56 and 0.57 respectively. The variations, with wall rotation θ, of P, δ, and d for the over-all contact forces are shown in Fig. 11, the dotted curves referring to one test and the solid lines to another. It is evident that from

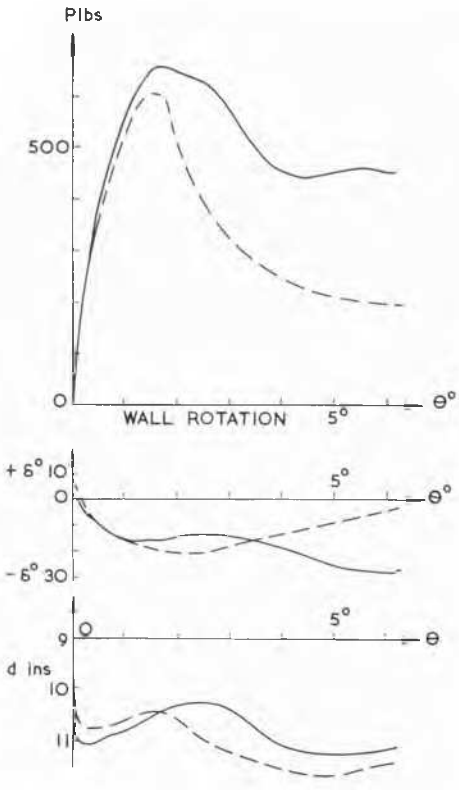

FIG. 11. Curves showing variation, with wall rotation θ, of contact force as defined by P, δ and d (see Fig. 8) for two tests on approximately identical samples of dense sand.

the start of the test up to peak load conditions the samples behaved almost identically but beyond peak load they were quite different. It is possible that up to peak load both samples work-harden under deformation and develop identical wedge-shaped sectors of strain of the type described above, but that once peak load has been exceeded parts of these wedge-shaped sectors become unstable and work-soften under further deformation. Consequently the location of the ultimate rupture surface that develops within the wedge-shaped sector is entirely a matter of chance and will probably depend upon initial lack of uniformity of the packing of the sample. The X-ray photographs of the ultimate rupture surfaces for both these tests were quite different but both lay within the wedge-shaped sector defined above. Consequently it might be inferred that while pre-peak behaviour is repeatable and might become capable of prediction post-peak behaviour is not.

It is undesirable to generalize from the results of a few tests but this brief outline has been given to indicate the need for reliable data under controlled conditions, be it from the field or the laboratory, if any rational improvements are to be made in earth pressure theories.

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U. LUSCHER (U.S.A.)

This discussion concerns the phenomenon of buckling of underground pipes. Buried-tube geometry is quite complex, but can be simplified to a circular-symmetric tube-soil situation for basic experimental and theoretical work (see Fig. 2b of the paper by *Luscher and Höeg, 5/8*). The approximate mathematical solution for buckling in three or more modes is:

critical buckling pressure: $p^* = 2\sqrt{(k_s EI/R)}$,
critical buckling mode: $n = \sqrt[4]{(k_s R/EI)}$.

In these equations k_s is the modulus of elastic soil resistance (in units of pressure), and its determination represents the major problem.

One method of determining k_s tentatively suggested by *Habib et Luong (5/3)* was to use the resistance of the soil to uniform pressure applied in the cavity. This idea had earlier been used by the writer for the symmetric thick soil ring (Luscher, 1963). The theoretical result is shown in Fig. 12,

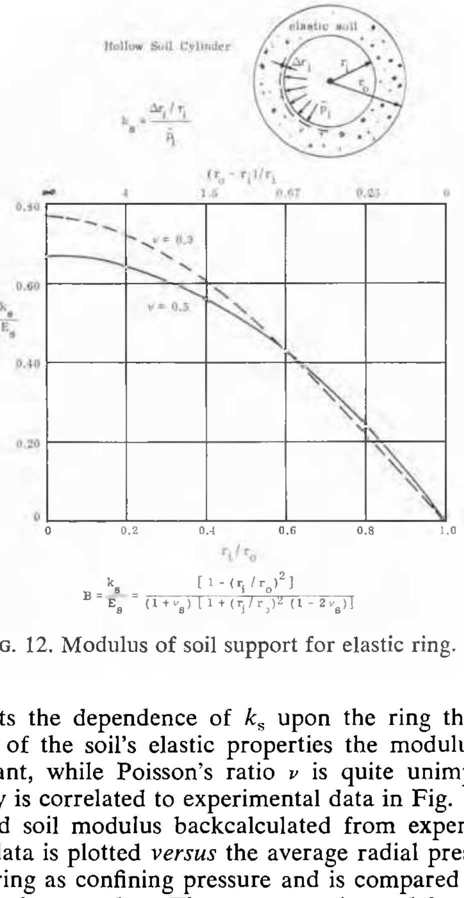

FIG. 12. Modulus of soil support for elastic ring.

which plots the dependence of k_s upon the ring thickness. Note that of the soil's elastic properties the modulus E_s is all-important, while Poisson's ratio ν is quite unimportant. The theory is correlated to experimental data in Fig. 13. The constrained soil modulus backcalculated from experimental buckling data is plotted *versus* the average radial pressure in the thick ring as confining pressure and is compared to conventional œdometer data. The agreement is good for a range of wall thicknesses and effective stresses. Hence the theory is verified.

The extrapolation of this theory to tubes of various materials, surrounded by an infinitely thick layer of the same soil as used in the laboratory tests, is shown in Fig. 3 of the writer's paper to this conference (*Luscher and Höeg, 5/8*). Buckling curves similar to column buckling curves result, also limited by the material's yield stress. Other soil rigidities, inelastic buckling, or the difference between circular-symmetric and buried-tube situations have not been considered in that figure. The critical buckling pressures are very high,

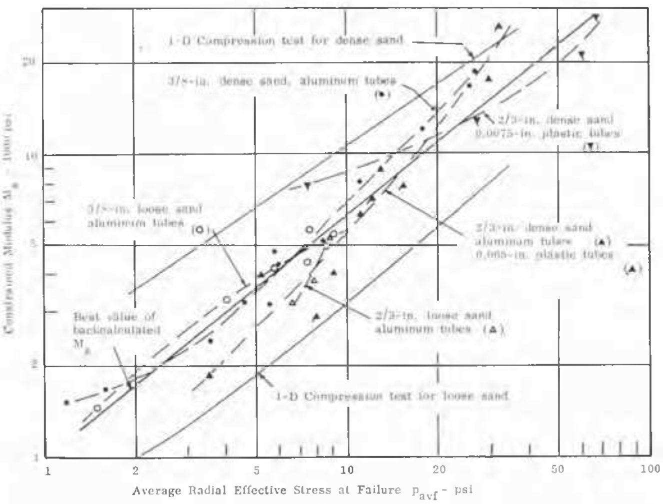

FIG. 13. Correlation of buckling pressures with theory.

orders of magnitude above those predicted on the assumption of hydrostatic buckling.

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K. H. HÖFER (Germany)

Although there are great differences in the construction tasks of foundation engineering and of mining engineering, it seems to me that there is a greater similarity between soil mechanics and rock mechanics than is commonly supposed. Students of both work in the same field in the earth's crust, although the one deals with relatively loose material, the other with rock, which is more or less solid or broken. Neither soils nor solid rocks behave elastically. However, according to the rheological models of Kelvin-Voigt or Poynting Thomson, some rocks have a visco-elastic behaviour, that is, deformation appears as an after effect and comes to a rest. There are many more weak rocks in the mined part of the earth's crust which are visco-elastic, according to the Burger or Maxwell-Modell theories or are even visco-elastic-plastic. Examples of such rocks are gypsum and marble, many shales and sandstones in the coal measure strata, and all salt rocks as rock salt, sylvinite, or carnallite. In view of this, it is not enough to know only something about the breaking strength for compression or the shear strength and the elastic moduli; one must also know the yield stress, the relaxation time, and the viscosity of the material. However this information cannot be obtained from laboratory tests alone.

In very homogeneous rocks (such as, rock salt) the breaking strength for compression is of the same order both in the laboratory and *in situ*, but the more fissured the rock is, the more the breaking strength decreases with increasing volume of the specimen, and the *in-situ* strength may be very low compared with the strength values obtained in the laboratory. Ultrasonic tests of rock salt and iron ore both in the laboratory and *in situ* showed that the dynamic elastic modulus of rock salt *in situ* was of the same value as that obtained on a specimen. On the other hand the value of the elastic modulus of iron ore *in situ* was only one-twelfth of that found in the laboratory. It can be concluded from this that the breaking strength of this material *in situ* is very much lower than the breaking strength of the specimen. In the case of rock salt, however, it is not enough to know that the strength of the specimen and the *in-situ* strength are of the same order, for it reveals that the relaxation time and the viscosity *in situ* and in the laboratory differ very much. In the laboratory we have obtained relaxation times in the order of minutes but underground at the pillars the relaxation time was in the order of hundreds of days.

To resolve all the problems involved in the field of rock mechanics both for mining purposes, and for dam foundations and tunnelling, it is necessary to work in a wide field of research, and to obtain both empirical and theoretical solutions. Therefore, underground deformation measurements are connected with laboratory test such as the investigation of the strength behaviour of the material, model tests, and mathematical works. New instruments are now common in rock mechanics research—X-ray interferencegoniometers, electronic microscopes, and last but not least, a real triaxial testing machine for testing cubic rock samples up to 200 mm with three different independent main stresses. This machine has a capacity of 600 tons in the vertical direction and twice 300 tons in the two horizontal directions.

It seems to me that methods in the two fields of soil mechanics and rock mechanics and the materials dealt with are not so different that there should not be closer connection between these two engineering branches. I am glad to hear that there are many opinions expressed by soil mechanics specialists which are very close to our rock mechanics thinking.

A. J. DA COSTA NUNES (Brazil)

We did not find, in the General Reports or in the *Proceedings* of this Congress any reference to the methods for anchoring in soils or in rocks, or any reference to the problems of calculation peculiar to these methods. However, such methods for anchoring are beginning to be developed simultaneously in several countries. We have noted, for example, that these methods are often used in Montreal for retaining walls for urban excavations. During the Excursion 11, one of the participants of the Congress told us that the processes of anchoring are now in fashion. Contrary to what happens to other fields of human activity, in our profession fashion as a rule follows the economical advantages.

Since 1957 we in Brazil have developed independently the process of injected and prestressed anchorage, utilizing calculation methods which we would be pleased to have discussed in this Congress.

The types of problems that have been solved by such anchoring methods may be summarized as follows: (*a*) stabilization of rock boulders lying on slopes of broken rock or on residual soil; (*b*) stabilization of slopes on soil or on broken rock, some supporting constructions; (*c*) stabilization of excavations in caves and in tunnels; (*d*) anchorage to the mass of soil of constructions executed on slopes; (*e*) stabilization of piles driven in soil strata lying on a very steep rocky substructure; (*f*) anchorage of constructions submitted to pulling out forces or to inclined and horizontal forces such as dikes, locks, and basements subjected to uplift, dams, and retaining walls subjected to uplift and to horizontal forces due to water and earth pressures; (*g*) strengthening of existing retaining walls which have become insufficient for some reason; (*h*) creation of reactions for load tests and for penetration tests; (*i*) temporary support of unstable slopes to provide conditions for ultimate stabilization.

Fig. 14 indicates the steps in the execution of slope stabilization. The main advantages of this method are as follows: (1) since the walls can be anchored at as many points as desired, the strains to which they are submitted are relatively small and their thickness may be very economical; (2) anchored walls can be executed from top to bottom as the excavation proceeds and can be applied over the natural ground cut. Under these conditions we avoid the need for backfilling after executing a slope and building the wall.

As a general rule, soil in its natural state has better characteristics of shearing resistance than fill, and this contributes more to the economy of the anchored wall. If there are structures close to the edge of the wall the anchored type is the only way to eliminate the need for a sub-foundation for the structure, unless the cohesion of the slope

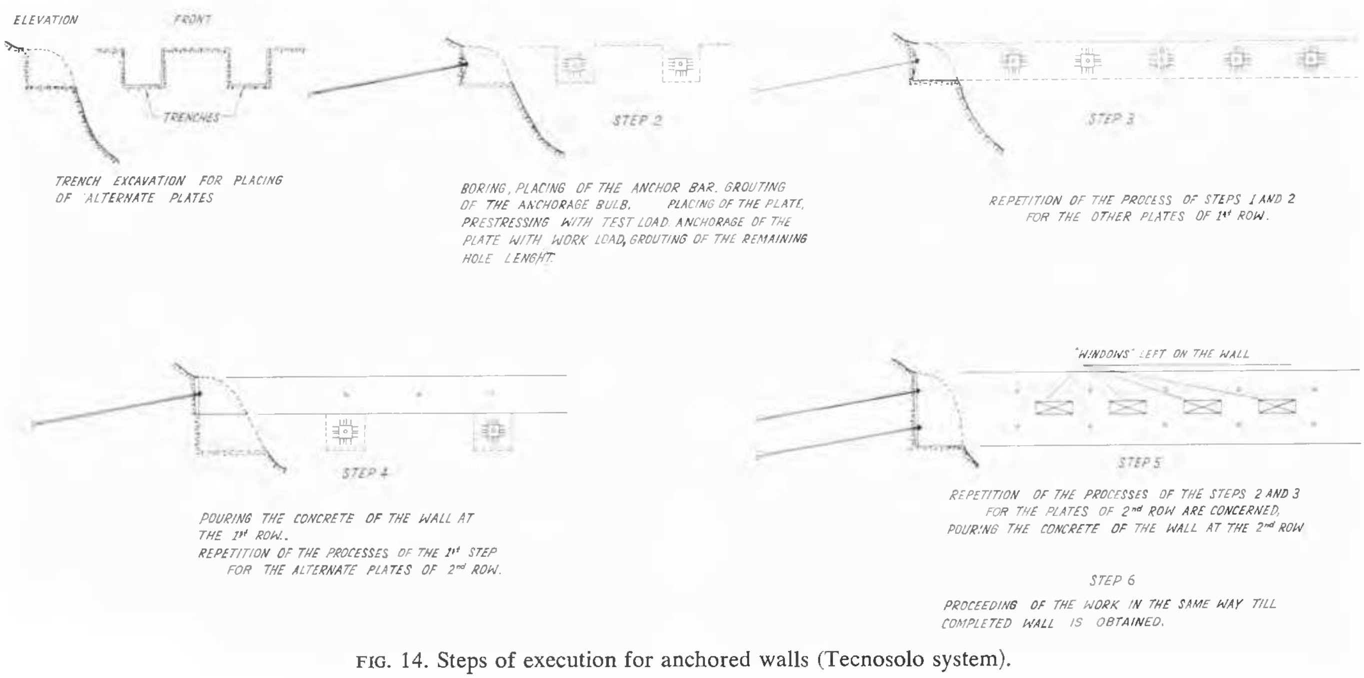

FIG. 14. Steps of execution for anchored walls (Tecnosolo system).

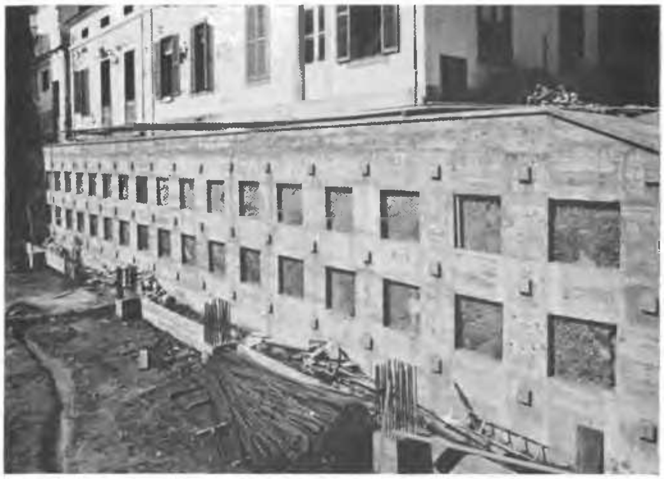

FIG. 15. Anchored wall supporting a slope above which a construction is located.

supports the construction even though the slope is cut near the construction. Fig. 15 shows an anchored wall supporting a slope above which an important construction is located. Note the openings left through the wall which saves concrete and allows drainage; these can be used every time the soil has enough cohesion to remain stable by means of the arching effect around the openings.

D. H. TROLLOPE (Australia)

(*Oral presentation not subsequently submitted for publication.*)

L. CARPENTIER (France)

Krsmanovič and Buturovič (5/7) ont rendu compte des résultats obtenus dans l'étude des pressions s'exerçant à l'extérieur des revêtements à l'occasion du percement du tunnel d'Osenik, sur la ligne de Sarajevo-Plöce, dans la zone où le revêtement avait subi des pressions suffisamment fortes pour entraîner des désordres majeurs en sa reconstruction. Il paraît intéressant de présenter les résultats analogues obtenus au cours du percement du tunnel de Monaco, sur la ligne de Marseille à Vintimille, par la Société nationale des chemins de fer français.

Le tunnel de Monaco, dont la section est en forme de fer à cheval, d'un diamètre intérieur moyen de 10 m environ, traverse sur un peu plus de 1 km les marnes cénomaniennes, argileuses ou calcaires des retombées montagneuses aboutissant à la mer (fig. 16). La région a été fortement éprouvée par les plissements alpestres qui ont provoqué, en particulier, le chevauchement d'une écaille calcaire de Lias de plus de 100 m de haut sur les marnes cénomaniennes avec les dislocations et contraintes orogéniques correspondantes. Ces marnes, de nature et consistances diverses, présentent des résistances à la compression simple de l'ordre de 30 à 40 kg/cm.ca., un angle ϕ variable de 35 à 45° et une cohésion de 2 à 9 kg/cm.ca. Elles accusent des pressions de gonflement de 6 à 12 kg/cm.ca. Leur teneur en eau naturelle est voisine de 6 pour-cent environ pour une teneur en eau de saturation de 8 pour-cent. Leur module d'élasticité est de l'ordre de 2 000 à 3 000 kg/cm.ca. Plusieurs éboulements s'étant produits au cours du percement de la marne et de fortes pressions se manifestant sur les boisages des galeries, on a disposé des batteries de vérins plats de 50 à 60 cm de diamètre entre le revêtement et le terrain pour tenter d'évaluer es réactions.

Nous donnerons ici les résultats obtenus en deux sections bien différentes du point de vue géologique, sous des hauteurs de recouvrement du même ordre (100 et 110 m environ). Dans la première section (voir coupe géologique transversale, fig. 17) les mesures ont été faites à l'aide de deux batteries de 11 vérins plats, situées à 1,50 m l'une de l'autre, pour s'assurer de la validité des mesures (km 1 228 et 1 229,60). La fig. 18 donne le diagramme des pressions relevées dans la première batterie km 1 228. Pour être certain que les valeurs lues aux appareils n'étaient pas le seul résultat du hasard, on a ramené uniformément à 3 kg/cm.ca. la pression des vérins, 6 mois après leur pose. Le retour rapide des pressions à leur valeur initiale a montré la fidélité des mesures. Dans la batterie voisine (km 1 229) on a obtenu des résultats analogues: les reins apparaissent comme fortement chargés et la clef est déchargée.

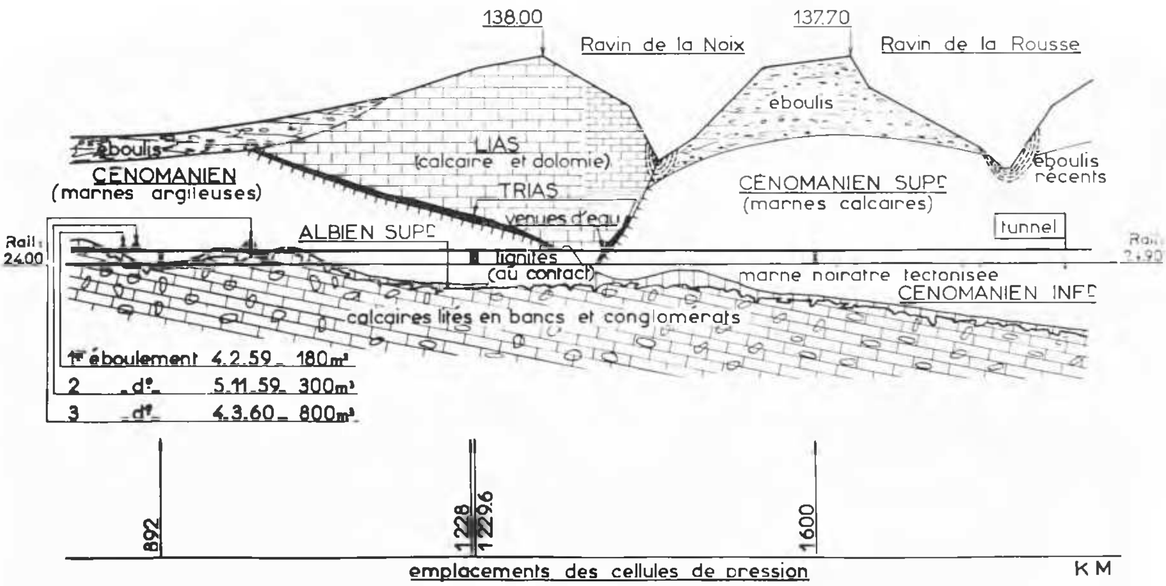

FIG. 16. Coupe géologique en long.

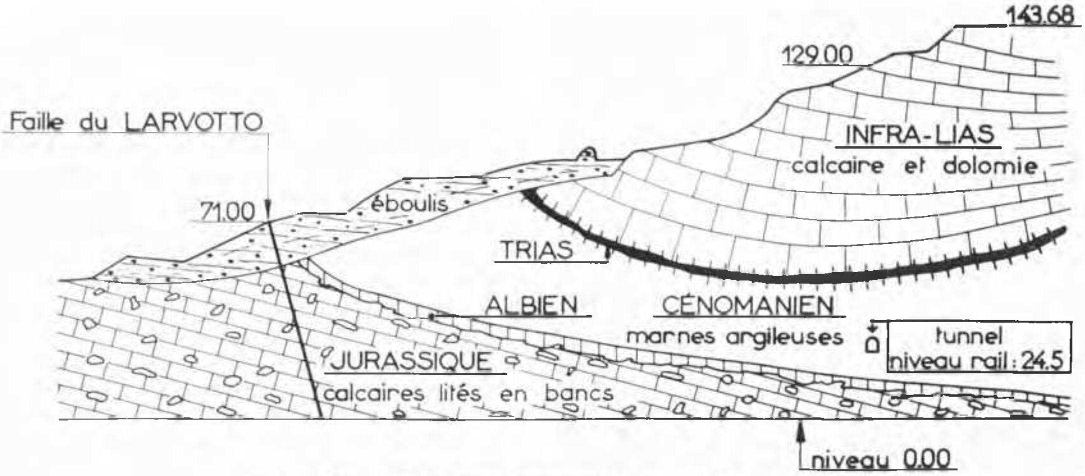

FIG. 17. Coupe géologique transversale—km 1 228 et 1 229.6

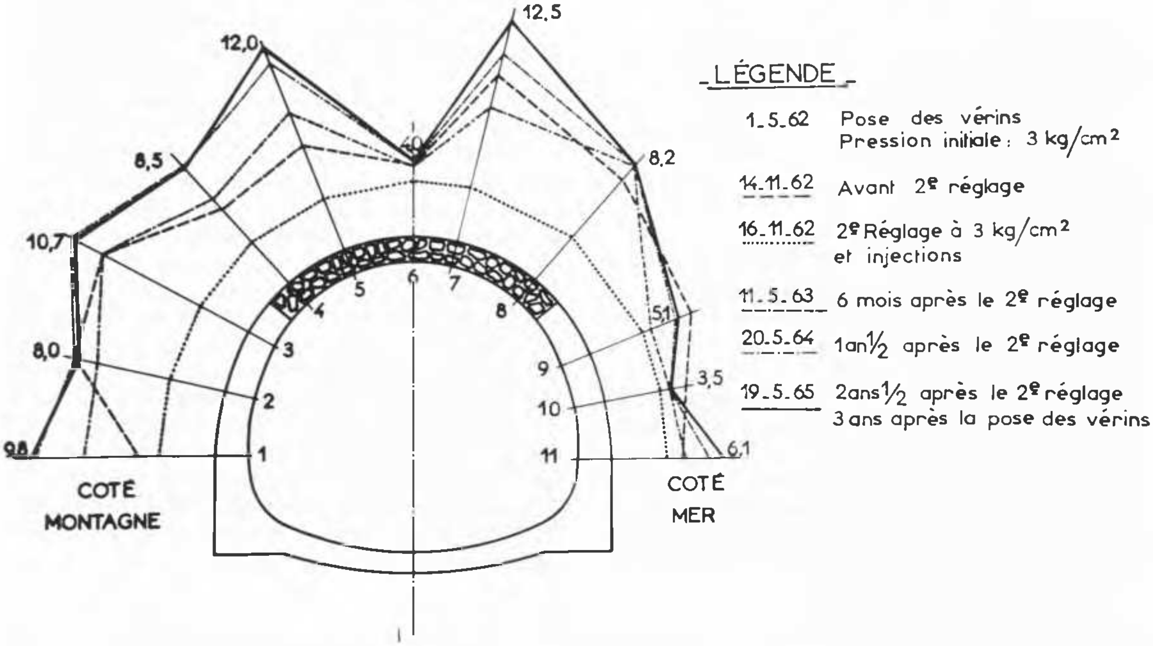

FIG. 18. Pressions derrière le revêtement—km 1 228.

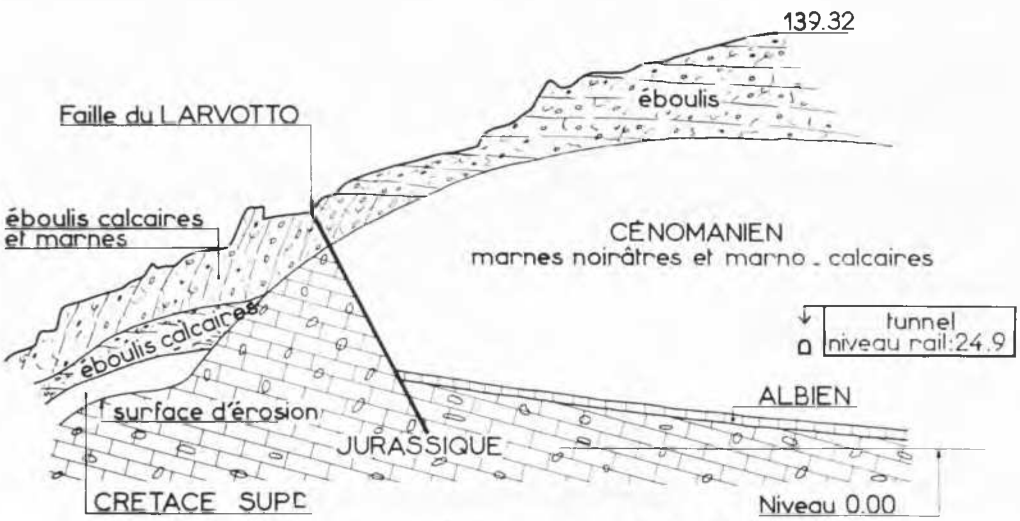

FIG. 19. Coupe géologique transversale—km 1 600.

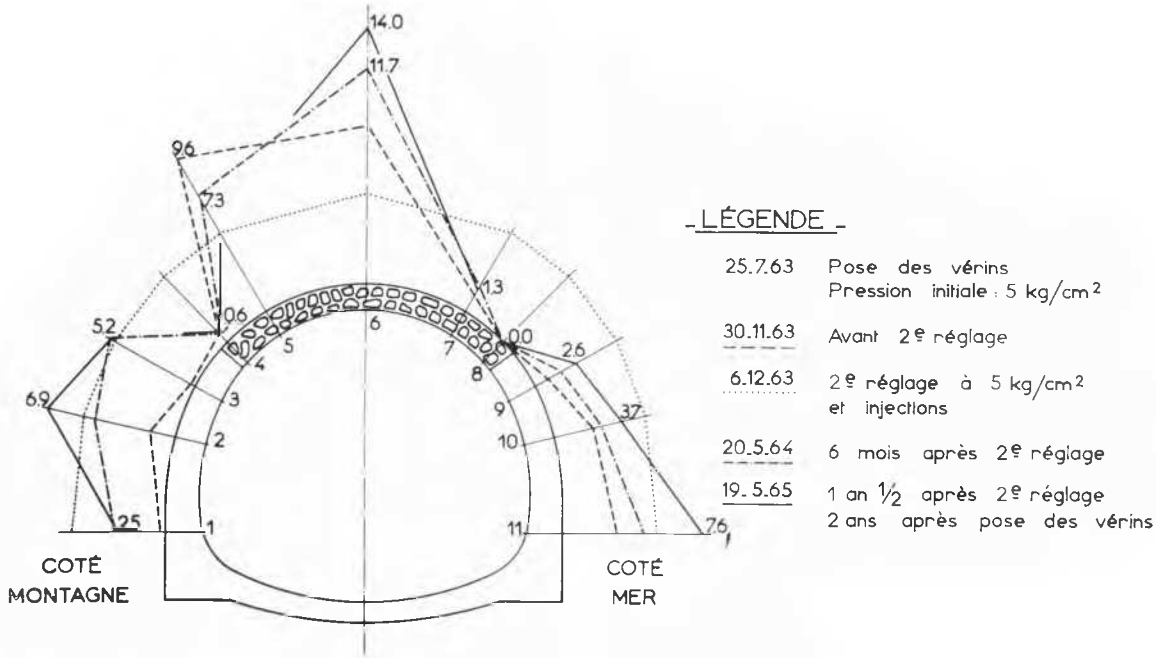

FIG. 20. Pressions derrière le revêtement—km 1 600.

Dans la deuxième section, km 1 600 (voir coupe géologique transversale, fig. 19) où des pressions élevées avaient été observées sur les boisages, c'est la clef qui accuse une surcharge considérable (fig. 20).

Dans les deux sections, et comme dans le cas signalé par MM. Krsmanović et Buturović, la pression est donc loin d'être uniforme autour du revêtement et les maxima de contraintes paraissent présenter une certaine symétrie. La pression maximum locale se situe entre 50 et 70 pour-cent du poids des terres, la pression moyenne semblant largement inférieure à la moitié de ce poids.

L'état des contraintes paraît donc dépendre non seulement des caractéristiques physiques du terrain et du revêtement, mais encore de l'histoire de la formation rocheuse et des procédés d'exécution du tunnel. Nous sommes d'accord avec MM. Krsmanović et Buturović pour penser que de nombreuses mesures et essais sur place sont nécessaires avant de pouvoir déterminer avec quelque certitude les caractéristiques dimensionnelles des revêtements de tunnel.

J. D. LEWIN (U.S.A.)

The General Reporter and several contributors of papers have reported remarkable progress in gadgetry for determining forces in various types of soils. Naturally, testing is only of value if its results can be accepted as reliable and applied practically. One is astonished at the accuracy of prognoses in which one predicts conditions within 10 per cent, or even better of forces and settlements, which will occur in the actual project. This reliance on test data often leads to disastrous results. Two examples will illustrate my point.

A short time ago in New York, a 7-ft-wide sewer trench had to be excavated. The depth of the trench was 25 ft (Fig. 21). The top 7 ft were miscellaneous fill with soft organic silty clay below. Soil samples were obtained, tested, and analysed by experts. The results were plotted (Fig. 21a) and the earth pressure was assumed to act as a trapezoid at 70 per cent of maximum earth pressure. A total load of 33.8 kpf dictated a DP 2 steel sheeting and bracing (Fig. 21a). When the work was started, the sheeting and bracing were severely damaged. Actual forces in the field were measured (Fig. 21b), and it was necessary to strengthen the sheeting (ZP 38) and wales, and to add an additional wale at the subgrade. Sheeting was 4.4 times stronger, the cantilever was shortened to take 2.5 greater loads, and the wales were 3.5 times stronger. The sheeting actually became 4.4 × 2.5 = 11.0 stronger. This is an error of 1,100 per cent!!!! Although the total earth pressure only increased from 33.8 k to 43 k, the location of the resultant had a determining effect.

A few years earlier, a 19.5-ft-diameter tunnel was being built through solid rock. The material was strong, thickly

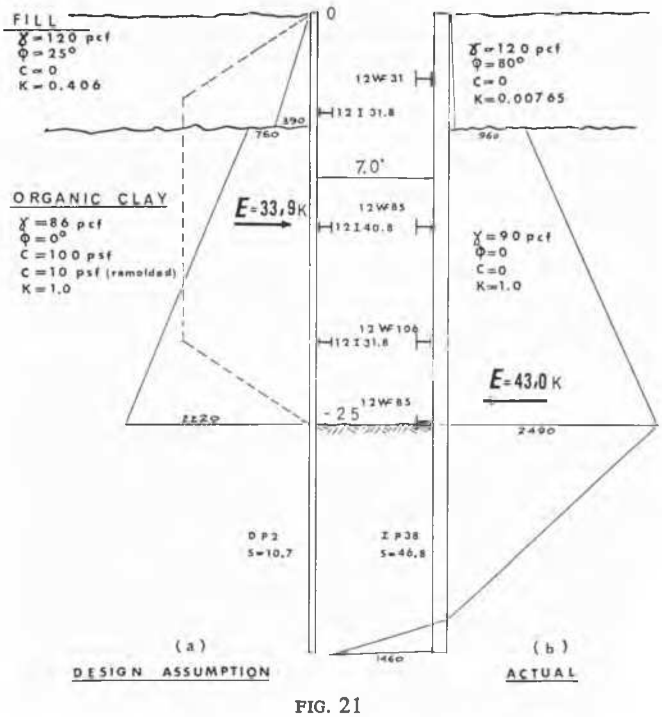

FIG. 21

laminated sedimentary rock, which was extremely watertight. All tests and theories led to a belief that the overburden of 82 ft would produce stresses less than 100 lb/sq.in. When the excavation reached this location, a cave-in occurred. Subsequent investigation established stresses of 16,000 lb/sq.in. These were 160 times greater than those computed, an error of 16,000 per cent! The reason for the large stresses was that the lamination was bent under tectonic conditions and the tunnel was passing through the compression side of the stratum. Obviously, the tectonic stresses were still "frozen" and the excavation triggered and released these forces.

Thirty years ago, a rational basis was required for designing earth structures and foundations, which led to laboratory investigations and the development of theories. This was necessary and this Society performed exceptional services to the profession. The last decade, however, has been a circus of decadence where mathematical gamesmanship replaces the study of the behaviour of soil and rock. I am sure that no person in this room seriously believes that the soil understands our mathematics or our hypotheses or our limit states of design. It is for us to *learn* from the soil and not to *teach* the soil how to behave.

Gentlemen, I believe that our endorsement of the publication of speculations, no matter how brilliant and ingenious they may be, is nothing but an intellectual fraud perpetrated on the profession. Any engineer looks to our proceedings as to truth, as to a bible. Instead, he is presented with fables. I know that many of you here will agree that this must be stopped.

The time has come when the pendulum must swing back to reality. Now, our efforts must be directed, not toward laboratories and universities, but toward municipal agencies which should modify their building codes to require installation of settlement monuments in every structure on soft soil or on piles. Thus, each municipality will be able to study the behaviour of soils in its community and be guided by facts and not fiction. Similarly, there should be laws requiring installation of appropriate devices to measure movements and seepage of dams and reservoirs. By advancing such legislation, the members of this Society, in my opinion, could perform the greatest service to the profession and to the public.

Chairman van Mierlo

Thank you gentlemen. We have come to the end of the Session and I would now ask the General Reporter for his remarks.

General Reporter Mencl (Czechoslovakia)

I would summarize the results of this very interesting discussion in the following points.

With regard to the general problem of the applicability of the classical theory of plasticity, Professor Brinch Hansen discussed the occurrence of two types of shear failure—line and zone. Nevertheless, even with this distinction, dilatancy remains a very important factor, because line failure occurs with dilatant materials in general; hence the necessity of observing volume changes in the shear-strained material is to be emphasized. I think that a similar finding results from Professor Vesić's contribution.

2. The discussion of the question of the order of succession of partial safety factors has resulted in the recommendation of a single solution for both active and passive problems. Since only one method is possible with active problems it is recommended that for passive problems is to be adapted to it. Yet this recommendation interferes with methods adopted for analysis of bearing capacity of foundations. Hence it is suggested that the Executive Committee appoint a committee for the elaboration of the question. The importance of establishing a standard for the order of succession of the steps of statical analysis has been supported by all.

3. With the statical analysis of flexible walls, the trend seems to be to both analyses: that of the actual behaviour of the structure as well as that of the imaginary limit state of stability. As mathematical tools allow ever more rapid procedures, the economy of the practice of calculation is no longer a factor.

4. The value of earth pressure in clayey soils appears to be governed by the pressure at rest as well as by swelling phenomena, whereas stability aspects are of subordinate importance.

5. The analysis of the statical behaviour of flexible tubes is of great importance and should be the subject of discussion at the next conference.

6. A mutual field of problems in soil and in rock mechanics has been demonstrated. The application of bolts for increasing the stability of slopes in soils has been presented as an excellent example.

(The remarks of the General Reporter for Session 7 made to the Closing Session appear on p. 595.)

Chairman van Mierlo

Thank you, Professor Mencl, for your comments, and now it is my duty to close this Session. Before I do so, I wish to thank Professor Mencl for his work as General Reporter. I know very well the efforts that this task involves. It is a time consuming job and must be done along with one's professional work. Also, I want to thank the members of the Panel, and the oral discussors for their contributions, and the audience for its attentiveness. I close the Session.

WRITTEN CONTRIBUTIONS CONTRIBUTIONS ÉCRITES

J. R. F. Arthur (Great Britain)

It appears worthwhile to re-emphasize and expand one point made in paper *5/1*, "An examination of edge effects in plane-strain model earth pressure tests," written by Mr. Roscoe and myself. In this work, plane strains adjacent to a side wall and in the centre plane of the model were compared. Under the particular conditions of these tests there were no significant differences in the strains in two planes and this result indicated that side friction did not play a large part in controlling the behaviour of the soil.

However, side friction in models of this type is largely influenced by the following factors: the material of the side walls, the composition, shape, and size of the soil particles, and the pore fluid present in the soil. In the test described the side walls were made from glass which is an exceptionally hard and smooth material. The sand particles were almost single-sized (~ 0.050 in. in diameter) and rather rounded. The sand was laboratory dry so that air was the pore fluid.

Different test conditions need to be investigated before assuming that plane strain will be achieved through the whole width of the sample. This statement is not intended to imply that close limitations are bound to exist. Fig. 22

FIG. 22. Rapid drawdown failure on model slope of fine sand.

FIG. 23. End view of failure shown in Fig. 22.

shows a rapid drawdown failure in a model slope of fine sand with a heavy liquid (specific gravity ~ 2.2) as pore fluid. Fig. 23 shows an end view of this failure and suggests that even in a rather complex case like this one a good approximation to plane strain may be achieved.

ACKNOWLEDGMENT

This experiment was carried out by Mr. Hanson and Mr. Lass as part of their undergraduate course at University College, London and the discussor gratefully acknowledges their help.

T. K. CHAPLIN (Great Britain)

In Fig. 3 of the writer's paper *(2/12)* a general pattern was suggested for the dilatancy-porosity relationship. Since the paper was written, some preliminary experiments have been carried out to elucidate the patterns of dilatancy under less usual conditions. In the first series of triaxial compression tests, sands were sheared at very low constant cell pressures. The dilatancy rate generally was virtually constant over a considerable range of strain, despite the considerable expansion, then rather suddenly began to decrease steadily.

The exploration of this phenomenon appears to be that until a certain strain has been reached, which is not by any means a very small strain, the particles have not undergone enough shearing to be able to take up an essentially different geometric arrangement compared with their initial arrangement. Once there has been enough strain for re-arrangement to begin, then the dilatancy rate can change and localized failure surfaces can develop within the sample.

When one considers the shearing of a mass of sand, it is apparent that the change of rate of expansion during a test may not give a change in stress ratio corresponding even to the smaller change which would be predicted by Rowe's expression. In a dense sand, the expansion which is being suppressed by the increasing stress level causes energy to be stored within the grains. Some preliminary experiments, designed to give some idea of the order of that effect, suggest that the absorption of energy within the grains may be closely comparable to but somewhat less than the transfer of energy across a boundary when the sample is allowed to expand freely. Accordingly one should not expect that changes of volume change rate will necessarily make more than a relatively minor difference to the value of the principle stress ratio. The ordinary boundary energy correction may not convert the results of tests on dense sand and loose sand to a common curve, but the magnitude of frictional absorption cannot adequately be analysed on the basis of external effects alone. Full account needs to be taken of the absorption of potential energy within the particle structure. The real problem is how to recover and so measure that potential energy withou losing much of it by friction.

S. HUECKEL (Poland)

Professor Mencl, the General Reporter of the Division, opens his excellent study by comparing the material contributed to this Conference with discussions which took place at the Paris Conference in 1961. I observed some continuity in the topics set forth by him for discussion, in that two of the topics discussed in Paris are indirectly implied in the problems Professor Mencl proposes for discussion, viz. (1) comparison of the hypotheses underlying different methods of calculation of active and passive earth pressures, and definition of limits of the application of these methods and (2) factors of safety for design.

The General Reporter presented them in an altered form, correctly assuming that some limitations are indispensible and narrowed down the range of problems, stressing, in the first instance, the influence of the phenomenon of dilatancy of soils, and in the second problem, the question of fixing the most compatible calculation schemes.

I should like to touch upon the first two topics proposed by the General Reporter. Furthermore, with special reference to the paper by myself and my collaborators *(5/5)*, I should like to add a few remarks on the verification of theories by experiments, a problem which formed the third topic of discussion in Paris.

I do not pretend to be able to answer the first question posed by the General Reporter; still, I intend to convey a few remarks I came upon when again perusing the material contained in the discussion at the Paris Conference. It appears to me that both in some opinions expressed in the discussions and in the summary of the then General Reporter a starting point for at least a partial solution of the first question of Professor Mencl can be found. As will be recalled, special stress in those discussions was laid on two items, viz., that the theories of earth pressure should take kinematic conditions into consideration, and that they should also consider the influence of dilatancy of the soil.

The kinematic conditions have been already satisfied by the theory of Professor Brinch Hansen (Brinch Hansen and Lundgren, 1960). As for the other theories, we have been assured that the kinematic problem can be solved by methods analagous to those applied when solving the static problem (Sokolovski, 1961). It would be interesting to learn if further investigations were made in this respect, and with what results (Hueckel, 1961, suggestion 4 to item No. 1).

With respect to the second condition mentioned, with good reason, by Professor Mencl, it appears that this problem does not admit of solutions by way of the "classical" theory of plasticity, but rather calls for the application of an elastic-viscous-plastic model or, in brief, for a transition to rheological analysis. The studies in this domain, pertaining to the general problems of soil mechanics (but not yet applied to the questions of earth pressure), are already numerous, the Symposium of Rheology and Soil Mechanics (Grenoble, 1965), among others, furnishing rich material in this respect. Its results might contribute to an answer for Professor Mencl's question.

Passing to the second topic set forth by Professor Mencl, I should like to stress that it implies the problem of the safety factor, so much discussed in Paris. As a rule, I am in accord with the General Reporter's idea that it is highly desirable to introduce some standards into the methods of calculating the stability of retaining walls. I am not sure, however, whether the proposed schemes actually cover all the possibilities. Personally, I see the possibility of also solving this problem by applying partial safety factors, an idea which found its full application to soil mechanics in work by Professor Brinch Hansen (Brinch Hansen and Lundgren 1960).

In other branches of engineering, the probability methods of objective determination of factors of safety find an ever wider application. I could quote quite a number of recent studies using these methods in connection with soil mechanics. I tried myself (Hueckel, 1963) to adapt such a method to the system of partial safety factors proposed by Professor Brinch Hansen. Nevertheless, I concluded that in view of the hitherto insufficient statistical data, such a method is not likely to find a practical application.

This led me to propose another simplified semi-probability method of determining partial safety factors in a way which, though arbitrary, is what I should term "controlled arbitrariness." It is a way of determining the partial safety factors while taking into consideration, on the one hand, such essential factors as the importance of the structure, the likelihood of basic assumptions and hypotheses of the statical computations in question, the susceptibility of the structure to displacements and, on the other hand, the accuracy of the methods used to determine the corresponding values of load or the shearing strength of soil, the scatter of these values, the exactitude of work, and exactness of work supervision (Hueckel, 1964).

I am not certain whether this method can be considered fully developed. No doubt, if it were to find its application in practice, it might need some modifications. However, I think it presents a possible way to determine the safety factors more objectively than when using the conventional safety factors. Hence it might contribute to the rationalization of stability computations of retaining walls, the actual aim of Professor Mencl's second discussion topic.

Finally I should like to mention, although it is outside the scope of topics outlined by Professor Mencl, the question of experimental verification of theories which was widely discussed in Paris. I recall the vivid exchange of opinions between Professor Brinch Hansen and Dr. Bjerrum regarding the earth pressure distribution on the wall of a retaining structure. Among others, Professor Brinch Hansen (1961) stated then: ". . . I admit that it would be very nice if we knew the stresses existing at every time in the soil and on the wall, but I believe it is completely impossible to do this with our present knowledge. . . . So, there again, I have to say that, if we can predict the stresses in the state of failure, we should really be satisfied for time being."

I confess that these words constituted one of the motives for submitting the paper of my collaborators and myself *(5/5)* to this Conference. In it we describe an apparatus permitting the determination of the values of unit passive earth pressure acting on a plate pressing against the soil and to state the results obtained from some sets of measurements. It appears to me that the most important conclusion to be drawn from these experiments is the statement that the distribution of passive earth pressure on the plate surface under the given circumstances is principally the same both for the phase of elastic strains in the soil and for the state of failure.

It is my firm belief that it is only by experimenting to the largest extent possible that we can obtain reliable answers to any questions. For this reason I regret that the papers before the present Conference appear to be less concerned with the problems of experimental verification of theories of earth pressure than was the case in the preceding one.

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H. Militzer (Germany)

The high level of competence of the present physical and electronic measuring techniques allows the increasing use of modern geophysical methods and instruments for solving engineering problems. In the case of storage basins, such methods can contribute to the detection of bedrock irregularities and to the solution of rock problems without causing undue disturbance to the rock medium. Because rock pressures, developed beneath the surface when structural loads are applied, are primarily mechanical in nature, seismic methods are of special importance in their investigation. For many years, these procedures have been used as reliable working methods in engineering geophysics, the "separate

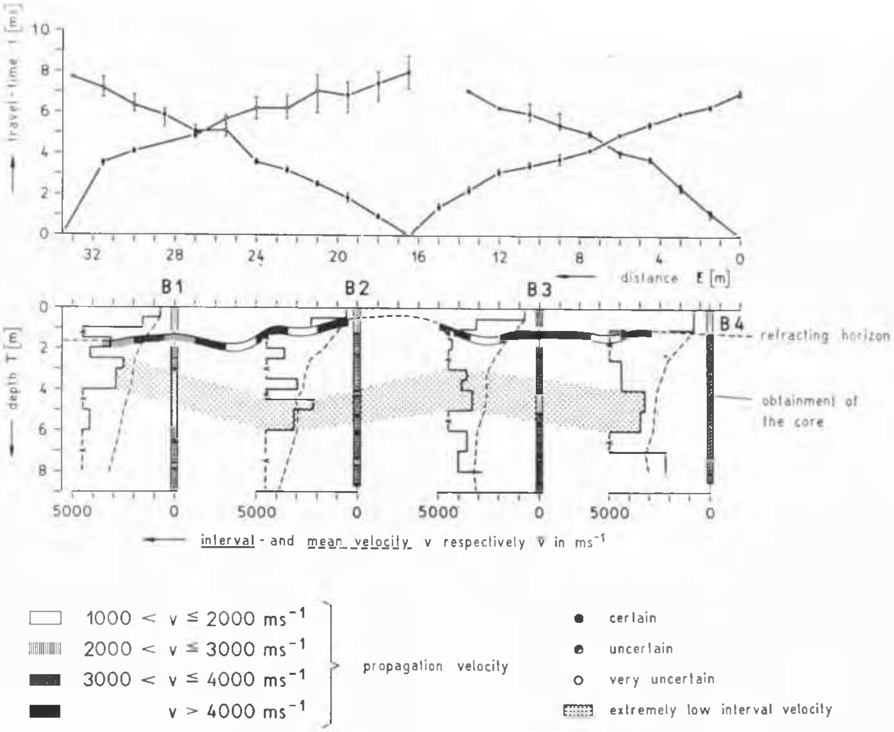

FIG. 24. Results of complex seismic investigations of an exposed foundation surface consisting of argillite.

impulse method," in particular, being used to explore the structure as well as to determine the characteristic value of the building ground. These procedures open new perspectives in the determination of the moduli of deformation and elasticity, particularly in connection with large loading and shearing tests.

The velocity of propagation of elastic waves is known to be a function not only of the dynamic modulus of elasticity and the shear modulus, but also of density and Poisson's ratio. As a first approximation, it is sufficient to consider only this velocity of propagation and to introduce it as a characteristic parameter for the physical classification of underground material.

In many cases, especially in the investigation of solid rocks, this has already resulted in valuable findings.

Fig. 24 illustrates the results of a research project involving refraction seismics which show that conditions may vary considerably both laterally and vertically in solid rock, partly by weathering and partly by shooting and blasting for pit excavation. Awareness of this variability might be a basic requirement, not only for the establishment of loading and shearing tests *in situ* but also for fixing of the final foundation level. However, seismic explorations using blows and blasting in solid rocks are rendered extremely difficult due to the great propagation differences between the subsurface zone, which is often very jointed and shattered, and the underlying "healthier" rock. Difficulties also arise as a result of laminations and stratifications. Special borehole logs for shallow exploration having been developed recently, additional geophysical studies of the drill hole have proved to be very useful not only for purposes of exploration but also for the determination of the characteristic values *in situ*. The result of such a complex operating method is shown in Fig. 24, and reveals the following: (*a*) the shape of the horizon found by seismic refraction; (*b*) the variation of the average and interval velocities measured by sinking the geophone; and (*c*) the obtaining of the core.

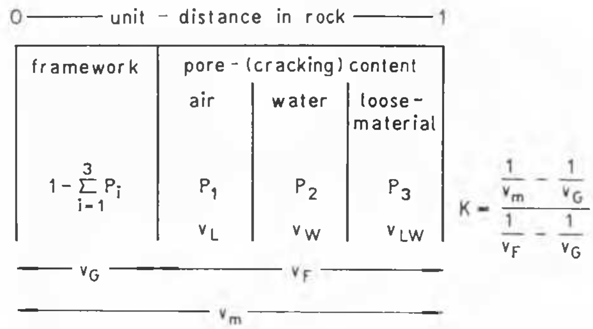

FIG. 25. Skeleton sketch of the equation of mean time where K = degree of pore (cracking) content and propagation velocity of elastic waves in various media is represented as follows: v_L = air, v_W = water, v_{LW} = loose material, v_F = pore content, v_m = *situ*.

Without discussing details of the results shown in Fig. 24, we can easily derive that (1) in good agreement with sudden variations of the interval velocity, the refracting horizon reflects the shape of relatively non-absorbing subsurface beds of slate which, because of the great difference of the velocity of propagation from $1000 < v > 4000$ m/sec on an average, must show a very strong seismic screening effect, and that (2) short-range seismic research of the borehole has great advantages over evaluating the obtainment of the core and, when suitably used, supports with the greatest possible accuracy the geological conceptions concerning the structure of the foundation area which are of great importance to the builidng trade.

First of all, factual findings have been described based only on test data obtained experimentally, and lacking any theoretical abstractions or mathematical manipulations. Certainly, however, we would be bad engineers and scientists if our efforts did not tend to exhaust completely the information from the measured data. One opportunity for use of the velocity of propagation of elastic waves for obtaining additional data is the so-called equation of mean time used with good results in exploration geophysics for a quantitative analysis of the porosity in reservoir rocks.

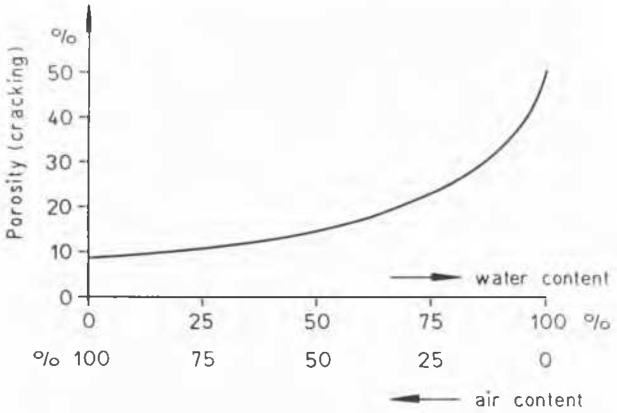

FIG. 26. Variation of the coefficient of cracking with the joint filling.

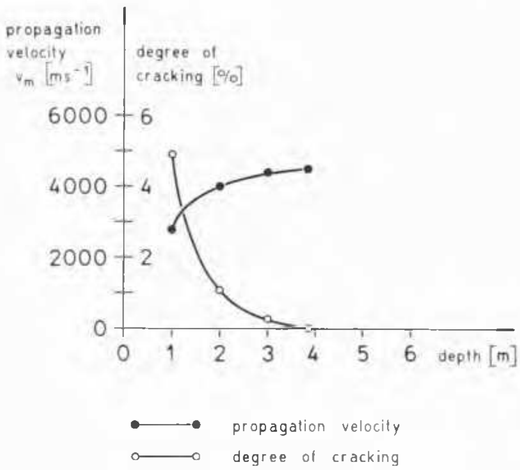

FIG. 27. Variation of the velocity of propagation determined by experiments and jointing of the solid basement rocks in a borehole in the construction of a tunnel.

The fundamental idea of the equation of mean time is that the propagation time of an elastic wave in a porous medium always equals the total of the travel time in the port filling and in the supporting framework. In the investigation of the solid rocks, it is not so much the porosity that is placed into the foreground, but rather the jointing, the determination of which requires the knowledge of the velocity of propagation in a material free of fissures, in a weak rock just as in the jointing including its filling. The velocity of propagation in a material lacking fissures can be determined in selected samples by the various methods of the ultrasonic technique. A special method is the so-called "Schlierenmethod."

Likewise, the determination of the velocity of propagation in a fractured medium by measurements in the solid rock meets with no difficulties. This method may take place with the accuracy characteristic of the seismic methods. Only the ignorance of the proportion of the air and water contents prevents the absolute determination of the degree of cracking.

From Fig. 26, we see that in case of a joint filled with 100 . . . 65 per cent air and 0 . . . 35 per cent water, respectively, the error of a quantitative determination of the degree of cracking does not exceed 50 per cent. In virtue of the comprehensive investigations, the type of rock does not play a part in this respect, because the behaviour of the velocity of propagation as a function of the amounts of water and air, respectively, is practically the same for velocities of propagation from 2000 to 5000 m/sec.

With due regard to the described quantities of influence, there are good possibilities for measuring the velocity of propagation of elastic waves to obtain the degree of cracking quantitatively in boreholes as well as shown in Fig. 27.

From Fig. 27, it appears not only that the degree of cracking, as expected, decreases with increasing velocity of propagation, but also that complex geophysical methods of investigation at their present stage of develoment are eminently suitable for supporting our ideas af the nature of the building site by the results of exact physical measurements *in situ*.

Y. NISHIDA (Japan)

I wish to comment on Eq 2 in paper *5/8* (*Luscher and Höeg*). When the externally applied radial pressure on a soil ring at the radius of r_0 is p_0 and the internal pressure on the tube at the radius of r_i is p_i, the following relationship can be obtained through the condition of stress equilibrium, if the tube deforms inwards, since the radial pressure is in the passive state in the outer zone and is in the active state in the inner zone:

$$\frac{p_o}{p_i} = \left(\frac{R}{\gamma_i}\right)^{[(2\sin\phi)/(1-\sin\phi)]} \left(\frac{r_o}{R}\right)^{[(-2\sin\phi)/(1+\sin\phi)]},$$

where R is the radius of the boundary between the passive state zone and the active state zone. R should be determined by other conditions of soil deformations or relating factors. The authors' expression of Eq 2 is the case of $r_o = R$. I would like to ask why they can take this to be the case, when they applied the radial pressure externally on a soil ring in their experiments.

W. W. RATTAY (Germany)

Professor da Costa Nunes reported on problems of anchors in the ground. Written discussion of this report will certainly be of interest to all experts, and for this reason, I present the following remarks.

THE PROBLEM

In the state of rupture an exact determination of passive earth pressure (bearing capacity of anchor) depends mainly on whether it is possible to define the form of rupture in front of and behind the wall and to tell how those forces which are acting along the surfaces of rupture, are calculated. In the state of rupture when sliding lines occur in the soil in front of and behind the wall the whole volume within the rupture surfaces "high" must be in equilibrium (Fig. 28). From this condition it follows that

$$W_1 + W_2 + R_2 + W_w + R_1 + A = \sigma. \quad (1)$$

This formula can be simplified somewhat if one thinks that

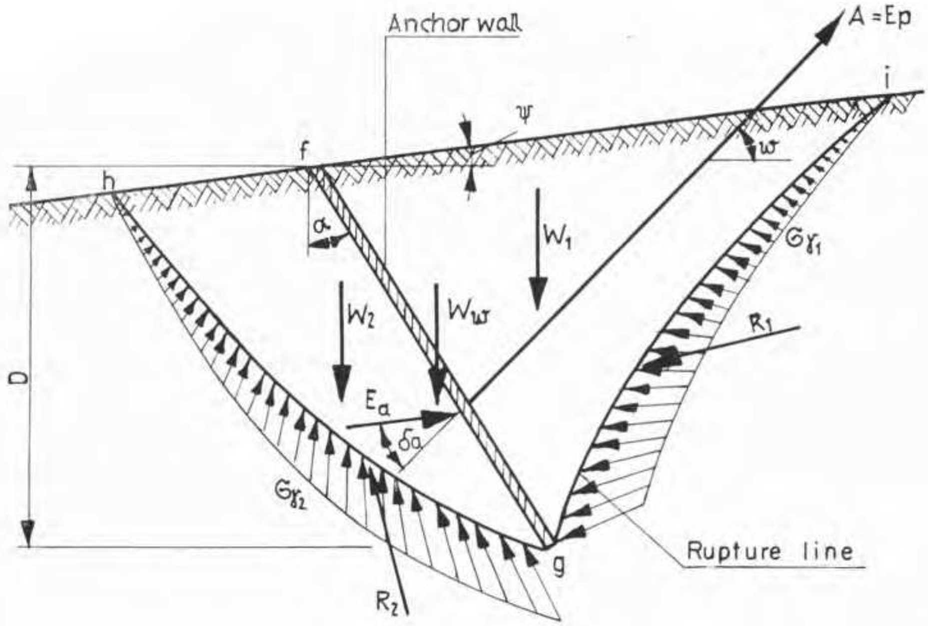

FIG. 28. Anchor wall showing forces acting within the rupture lines.

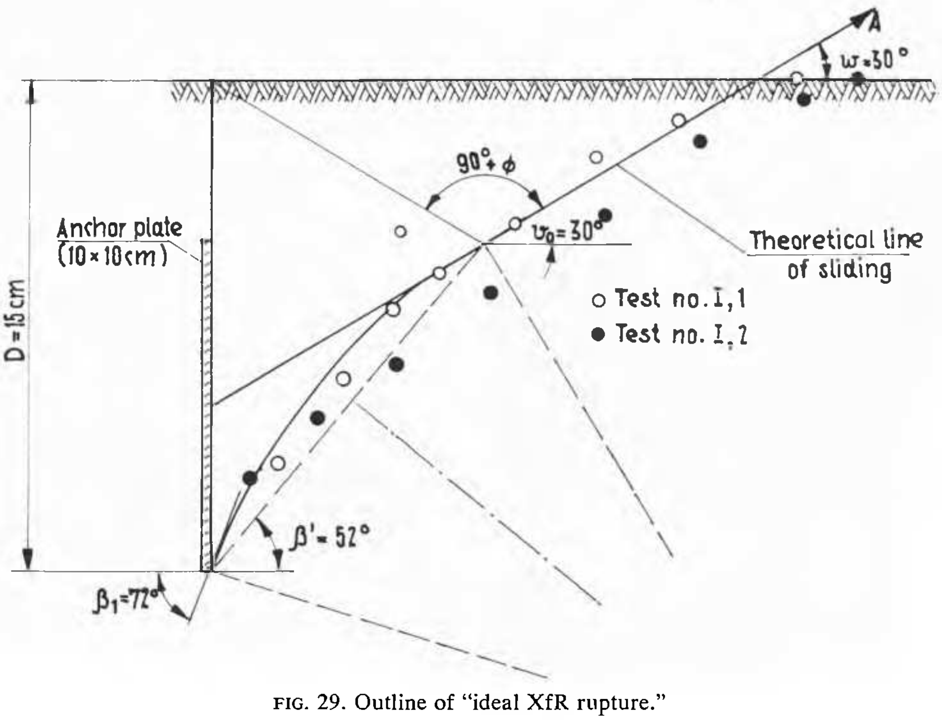

FIG. 29. Outline of "ideal XfR rupture."

the forces W_2 and R_2 produce the resulting active earth pressure E_a, which nowadays can be calculated by usual methods. Then Eq (1) reads

$$E_a + W_w + W_1 + R_1 + A = \sigma. \qquad (2)$$

To solve these formulae, however, we must know the form of the rupture line gi and the distribution of stress $\sigma_{\gamma 1}$ along this line or the direction of resulting resistance R_1. According to Brinch Hansen (1953) the following assumptions must be fulfilled: (*a*) the rupture line must be kinematically possible; (*b*) the rupture line must be statically possible; and (*c*) in any point of the sliding, Coulomb's failure criterion must be valid.

EXPERIMENTAL RUPTURE LINES

To establish the rupture lines, we carried out experiments with model plates. The tensile force was inclined at $\omega = 30°$ to the horizontal. The soil was placed and compacted in layers. A soil that is prepared in such a manner may be designated as homogeneous with a good degree of exactness. The soil properties were: $\gamma = 1.65$ grams/cu.cm. $\phi' = 30°$, $c' = 0.15$ kg/sq.cm, $w = 8.2$ per cent. The aluminium-sheet anchor plates were set in a glass box at inclined angles $\alpha = 0°$ and $30°$ and with various depths of embedment. At both sides of the plate there was enough space for free formation of an extended rupture in the soil. After rupture occurred, we carefully removed half the soil. Then the line of rupture

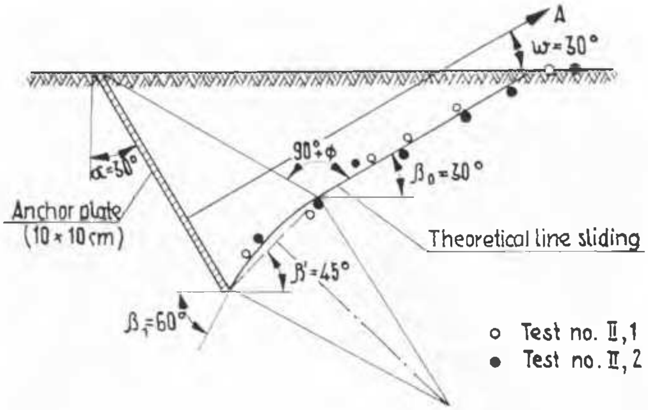

FIG. 30. Outline of P rupture.

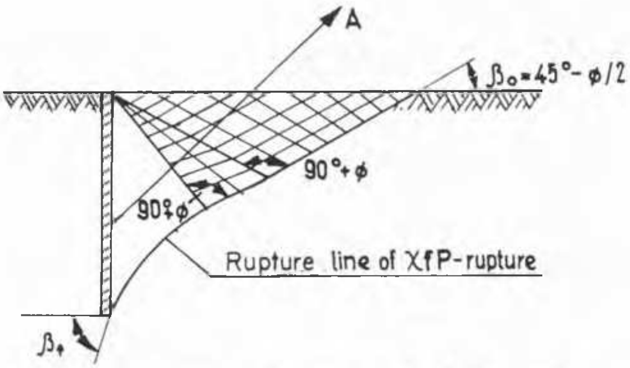

FIG. 31. Formation of rupture line, as XfR rupture.

was measured and the results are shown in Figs. 29 and 30. Only the tests with vertical anchor plates showed an active zone behind the plate. Behind the inclined plates a stable slope was formed.

CALCULATION OF RUPTURE LINE

In preliminary examinations (details of which are not described here), we found the best approximation of the results by a combined line and zone of rupture (Fig. 31) according to the theory of Brinch Hansen. For the purpose of practical calculation (Fig. 32) the author neglected the small concave part of sliding line. The rupture line shown in Fig. 32 may be called an "ideal XfR rupture," which does not form in nature. In accordance with Eq (2) for an ideal XfR rupture, Fig. 32 shows all forces acting within the rupture surface. The resulting resistance R_2 is a function of the forces T_0, N_0, T_1, and N_1 as well as the moments M_0 and M_1. Because the slope of the rupture line at the wall cannot be calculated separately, it is impossible to draw the rupture line gi.

The three unknowns (angle β_1, bearing capacity A, and the point of application of E_a), however, can be calculated on the basis of the static conditions of equilibrium for the rupture condition.

(a) Equilibrium of all force components acting perpendicularly to the direction of stress in the anchor cable (A):

$$\sum W\cos\omega + E_a\sin(\delta_a+\omega-\alpha) - \sum T\sin(\omega-\beta') - \sum N\cos(\omega-\beta') = 0. \qquad (3)$$

(b) Equilibrium of all force components acting parallel to the direction of stress in the anchor cable (A):

$$\sum W\sin\omega - E_a\cos(\delta_a+\omega-\alpha) + \sum T\cos(\omega-\beta') - \sum N\sin(\omega-\beta') - A = 0. \qquad (4)$$

(c) Equilibrium of all moments at the base of the wall:

$$\sum W\cdot z_w - \sum T\cdot z_t - \sum{}' N\cdot z_n + \sum M_1' + E_a\cdot z_e + A\cdot z_a = 0. \qquad (5)$$

The geometrical conditions also provide,

$$\beta_1-\beta_0 = 2\kappa,\ \beta'-\beta_0 = \kappa,\ D = L\cos\alpha \qquad (6\text{–}8)$$

and $L\cos(\Psi-\alpha) = L_0\sin(\beta_0-\Psi) + L_1\sin(\beta'\Psi)$. (9)

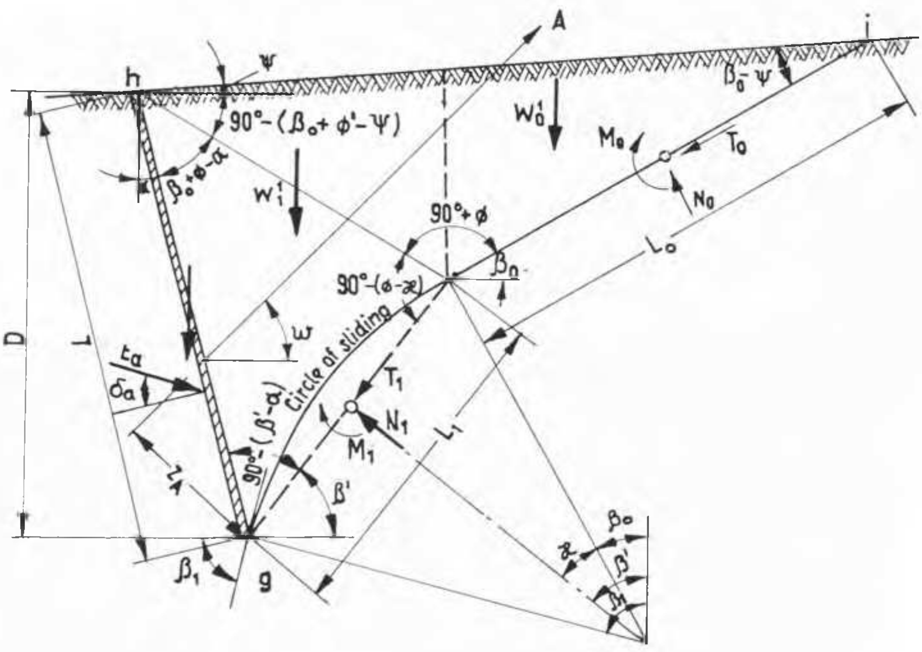

FIG. 32. Forces acting within the rupture lines for an "ideal XfR rupture."

Much of the mathematical labour can be reduced, if the Brinch Hansen tables (1953) are used for the determination of the internal forces within the rupture line, and if the point of application of E_a can be selected to correspond with what usually occurs in most practical situations. Using formulae (6)–(9) we calculate from Eq (3) the angle β_1. Then we calculate from Eq (4) the anchor force and lastly from Eq (5) the anchor distance.

CONCLUSIONS

The calculation of the bearing capacity of anchor walls (problem in two dimensions) by means of an "ideal XfR rupture," is very time consuming. For this reason, the anchor force for a particular example, was calculated according to different methods. The results of the theories of Brinch Hansen (1954) using a rupture line of the type (*S*-rupt.), Prandtl (*P*-rupt.), and Krey (1936) using passive earth pressure are published in Table 1 (Brinch Hansen, 1953). Comparison shows that the complicated XfR rupture can be replaced by the simpler straight *S* rupture, in which the calculated anchor force exceeds that obtained by the more exact method by up to 10 per cent.

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Y. TCHENG (France)

Le Centre expérimental de recherches et d'etudes du Bâtiment et des Travaux publics, avec l'aide de la Fédération nationale des Travaux publics, fait construire actuellement une station d'essais de mécanique des sols dans son domaine de Saint-Paul à Saint-Rémy-lès-Chevreuse, non loin de Paris. Cette station est destinée à mesurer à l'échelle réelle les efforts qu'exerce un sol sur le parement d'un mur auquel on impose un déplacement. La station a la forme d'un canal rempli de sol. Ce canal est barré par le mur maintenue par des vérins hydrauliques amarrés horizontalement à un massif rocheux existant et verticalement à un bac de sable de grandes dimensions recouvrant le canal. Les vérins verticaux et horizontaux sont asservis de façon à obtenir tout mouvement désiré du mur soit en translation, soit en rotation. Le centre de la rotation peut se trouver en un point quelconque déterminé au préalable.

Les réactions fournies d'une part par le rocher et d'autre part par le bac à sable sont respectivement de 2 000 tonnes et de 500 tonnes. Le mur a 3 m de haut et 5 m de large. La mesure des contraintes de poussée ou de butée ne s'effectue qu'au centre du mur afin d'éviter les effets de paroi.

Cette station sera vraisemblablement terminée en 1966. Les premiers essais pourraient être exécutés vers la fin de 1966. Nous espérons qu'à l'aide de moyens techniques aussi importants, des résultats nouveaux pourront être obtenus.

Y. TCHENG et Y. LEBÈGUE (France)

La "poussée" correspond au minimum de la pression exercée par un massif de terre sur un écran qui s'éloigne légèrement du massif; le mouvement peut-être une translation ou une rotation. Des formules théoriques indiquent la valeur du coefficient de poussée qui est fonction des caractéristiques du milieu, de l'inclinaison et de la rugosité du mur, de la pente de la surface libre, ainsi qu'éventuellement de la surcharge. Depuis plusieurs années, le Service Sols et Fondations du C.E.B.T.P. examine expérimentalement ce phénomène et étudie la validité des valeurs théoriques consignées dans des tables (par exemple, celles de MM. Caquot et Kérisel).

DISPOSITIF EXPERIMENTAL

Il est constitué par une cuve de 60 cm de largeur, remplie de sable et dont une paroi est mobile. Celle-ci qui a 25 cm de hauteur, représente un mur de soutènement rigide; elle comporte trois parties verticales, deux latérales de garde pour s'affranchir des efforts parasites sur les parois et une partie centrale sur laquelle est effectuée la mesure de l'effort. Pour que les trois parties suivent ensemble le même mouvement, un "contre-mur" subit le mouvement et le transmet aux trois parties de l'écran en contact avec le sol grâce à un système mécanique du type parallélogramme. L'inclinaison du mur peut atteindre ± 30° par rapport à la verticale.

Le sol utilisé est un sable moyen bien calibré à grains durs et arrondis. La surface libre peut être horizontale ou inclinée.

Pour réaliser éventuellement une charge uniforme et souple sur la surface libre, on utilise un système de rouleaux

FIG. 33. Anneaux dynamométriques—miniatures de fabrication C.E.B.T.P.

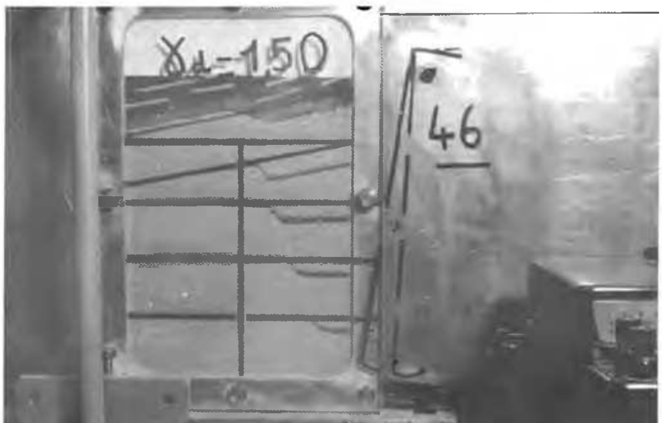

FIG. 34. Fin d'essai: surface inclinée et chargée, mouvement de rotation de l'écran.

chargés séparément et disposés parallèlement à l'écran. Avec un premier appareillage, l'écran effectue un mouvement de rotation par rapport à un axe situé au-dessus de lui.

Actuellement vient d'être mis au point un nouveau dispositif permettant la translation et comportant diverses améliorations. Les déformations nécessaires aux mesures ont pu être réduites grâce à la mise au point d'anneaux dynamométriques, d'environ 2 cm de diamètre, et comportant des jauges collées; ils présentent une déformation totale de l'ordre d'une dizaine de μ pour l'étendue de la plage de mesure, et leur sensibilité est inférieure à 10 gr pour une variation relative de résistance de 10^{-6}. La partie centrale sensible du mur est suspendue à 2 anneaux qui fournissent la composante tangentielle de l'effort; la composante normale est mesurée par trois anneaux en compression, un en haut du mur et deux en bas; ainsi on peut déterminer la position, la direction et la valeur de l'effort. Par ailleurs on peut enregistrer le phénomène, ce qui permet d'avoir des mesures continues pendant tout l'essai. De plus avec une vitesse de déplacement constante, l'enregistrement donne directement le diagramme effort-déformation. Cet appareillage constitue donc un instrument d'expérimentation remarquable, très précis et facile à faire fonctionner.

RÉSULTATS

Après remplissage de la cuve, on observe un effort relativement élevé et faiblement incliné par rapport à la normale à l'écran, c'est une sorte de "pression au repos." Lorsque l'écran s'éloigne, la pression décroit et passe par un minimum qui correspond à la "poussée," corrélativement son obliquité augmente. Cette diminution qui commence immédiatement, est d'abord brutale et importante; dans cette phase l'effort semble décroitre linéairement en fonction du déplacement. En même temps se produit dans le milieu une rupture dont la surface passe par la base de l'écran et coupe la surface libre à une distance d'autant plus faible que le milieu est plus serré. La poussée minimale diminue, lorsque la densité du milieu, et donc son frottement interne, augmentent.

Dans le cas d'un massif seul, les formules sont assez bien vérifiées; la poussée mesurée est voisine de sa valeur calculée; sa variation en fonction de la pente de la surface libre et de l'inclinaison de l'écran présente une allure analogue à celle que fournit la théorie. Mais le point d'application de l'effort est plus élevé; avant le mouvement, il est voisin du tiers de la hauteur H, puis au cours de l'essai, il remonte parfois jusque vers $0{,}45H$. D'autre part le déplacement de l'écran nécessaire pour obtenir la pression minimale semble compris entre 5 millièmes et 1 centième de la hauteur du mur; cette valeur est plus importante que celle observée par Terzaghi au cours de ses essais effectués, il est vrai, avec un écran bien plus important.

Lorsque l'on a affaire à un massif chargé uniformément et verticalement, le phénomène est identique. Toutefois, au cours de la mise en place de la charge sur la surface libre, nous avons observé que la pression sur l'écran augmentait à peu près linéairement en fonction de la charge appliquée.

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

In his oral discussion as a panel member J. Brinch Hansen strongly advocated the use of his ultimate strength method for the limit design of earth retaining structures. In particular, he expressed his feeling that this method was superior to the modified "equivalent beam" method for the design of anchored bulkheads and he stated that one should not "sacrifice quality for the sake of simplicity."

While fully agreeing with this statement in the abstract, I find it difficult to see its application to the point under discussion. After all, one should not equate complexity with quality either. It is true that Brinch Hansen's method, in spite of its complexity, can have useful applications in some rupture cases when electronic computers are available, but it does not provide information required for the design of flexible retaining structures, such as sheet pile braced cuts and anchored bulkheads, the design of which is governed primarily by their deformations and by the resulting earth pressure redistribution.

In the case of braced cuts, the use of the Kötter equation by Brinch Hansen permits the determination of the stress distribution along the failure surface, which is of little practical importance, but, paradoxically, provides only the total resultant lateral earth pressure against the sheeting of the cut and not the distribution of the lateral earth pressure, knowledge of which is essential for the design of the bracing of the cut. Therefore semi-empirical methods, based on field measurements, have to be resorted to.

Generally speaking, it should be made possible experimentally to check the premises of any engineering design theory, so as to subsequently modify it when necessary to correct measured deviations from reality. It is not possible to reproduce at a model scale all the variations of soil stratification which are encountered in practice, especially when cohesive layers are involved for which no laws of direct model similarity are valid. Full-scale field measurements therefore become essential.

Reliable and comparatively inexpensive equipment is now available and is being used with increasing frequency on new anchored bulkheads, permitting thereby the measurement of the key values on which the modified "equivalent beam" design procedure for anchored bulkheads depends. Small inclinometers—like that of Wilson—can be operated within plastic tubes not more than 3 in. in diameter attached to the steel sheet pile wall along its neutral axis. The point of contraflexure, that is of the zero bending moment or "hinge," as well as the location and the value of the maximum bending moment can then be determined with reasonable accuracy at many sites with comparatively little expense. The stress in the anchors can be determined by means of stable strain meters, like the Carlson electric resistivity unit, operated by remote control.

All this permits the control of the performance of the structure at all stages of backfilling or excavation and the determination of the location of the equivalent beam "hinge" for future use under similar soil conditions. By comparison, it does not appear feasible to produce full-scale failures of structures to check the values of the factors of safety involved in the use of Brinch Hansen's limit design procedure.

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

The claim by *Luscher and Höeg (5/8)* that the equalization of pressure all around a tube by pressure redistribution can safely be depended upon and that the tube soil system will fail by high mode buckling or by compression yielding of the tube appears to be justified for idealized bedding conditions and is verified by a number of laboratory investigations in sand. The results of field tests (Weissmann, 1957) obtained with thin-walled tubes placed in cohesive soil using standard construction practices do not, however, support this theory.

In the above-mentioned field tests, 4.5-in.-diameter aluminum tubes of 0.065-in. wall thickness were placed in trenches and backfilled with clay. The height of cover varied between

18 and 48 in. Trucks with wheel loads varying between 1000 and 10,000 lb. were run over the backfilled trenches. Tubes under 18 in. of cover actually collapsed if subjected to 8000-lb. wheel loads. The measured moment distributions indicate clearly that the tubes were subjected to high bending moments and that the failure occurred in bending. The pressures acting on the tubes are only a small fraction of the pressures required for failure in buckling or high yield compression. Hence, it must be concluded that the deformation restraint assumed by the authors occurs only in sand or idealized bedding conditions. For some time after construction under field conditions, the lateral restraint for cohesive soils becomes relatively small and the failure occurs in bending.

The use of corrugated instead of smooth-walled tubes was suggested by the authors to increase the stability of the tubes. This statement is correct; however, such an increase of the stiffness would also reduce the effect of the arching, and it is questionable which effect is more beneficial to assure a properly designed conduit.