

## DISCUSSION

**The Chairman**, in opening the discussion, said that he had followed Professor Haefeli's review of the soil testing methods used in Switzerland and their theoretical basis with a great deal of interest. With regard to the latter, it had, as stated by Professor Haefeli, been influenced to a considerable extent by Mr Bjerrum's theory and investigations, on which he would comment later.

Apart from that, he was especially interested in the cone test. If that test were treated theoretically, it would always be found that the ultimate cone pressure ought to be proportional to the shear strength of the clay. Professor Haefeli's experiments on remoulded reconsolidated clays seemed to confirm that with considerable accuracy.

Unfortunately, undisturbed natural clays practically never showed the expected regularity with regard to the cone test. In Sweden, Norway, and Denmark, it had been used extensively, and a great number of tests had been carried out in order to determine the relationship between the cone pressure and the shear strength as found by other methods.

The calibration curve thus determined had, for natural clays, practically always proved to be more nearly a parabola than a straight line. Moreover, each different type of clay had its own special calibration curve, and the individual variations were quite considerable.

That fact limited the useful application of the otherwise very attractive cone test to such cases, where the type of clay in question had been investigated previously, so that a reliable calibration curve existed.

The Chairman asked Professor Haefeli whether he had arrived at similar results with cone tests on undisturbed, natural samples as on remoulded samples. He was aware that the testing technique might influence the results. In the Scandinavian countries the falling cone was used most often, and different results were obtained if the weight on the cone were applied slowly as, for example, by the spring scale cone. It also made a difference whether the actual lowering of the cone or the depth of the impression in the clay was measured, as the clay surface might fall or rise to some extent around the cone. Finally, frictional resistances in the cone device might influence the results.

Other points of considerable interest in Professor Haefeli's Paper concerned the residual shear strength, the creep of slopes, and progressive failures. All those problems were, of course, of paramount importance for stability analyses.

Professor Haefeli had recommended that such analyses should be made on the assumption that the potential shear strength in the sliding surface was equal to the residual shear strength of the clay. In the case of materials with dangerous structure, Professor Haefeli had recommended, moreover, that only the minimum value measured immediately after failure should be reckoned with. That seemed to be an unusually conservative view, and he asked Professor Haefeli whether it had been confirmed by comparison with actual slides. As far as he knew it was standard practice in most countries to reckon with the shear strength of the undisturbed material, and the reliability of that view had been confirmed by the investigations made by Dr Skempton and Mr Golder, for example.

Another example was the Norwegian "quick-clay," a material which became practically liquid by remoulding, so that its residual shear strength must be exceedingly small. According to Professor Haefeli's considerations it should, therefore, be practically impossible to construct a stable slope in that material, but actually slopes in "quick-clay" were quite stable when the safety factor, calculated on the basis of the shear strength of the undisturbed material, slightly exceeded unity. That was a problem of the utmost practical importance and he hoped that it would be solved without any possibility of doubt.

Turning to Mr Bjerrum's Paper, the Chairman said that it was exceedingly interesting

to witness Mr Bjerrum's bold attempt to simplify the seemingly very complex phenomena connected with the shear strengths of clay. Mr Bjerrum had shown, theoretically and by means of tests on remoulded and reconsolidated clays, that the shear strength of such a clay, when fully saturated, depended only on the water-content at failure but not on testing technique and pore water pressures. If that result could also be shown to be valid for the normally consolidated clays in nature, it would at once solve almost all the present problems concerning the determination of the in-situ strength of such clays. In that case the in-situ water-content of the clay had only to be determined and a few shear tests carried out on samples which, of course, had to be structurally undisturbed but could otherwise be treated at will. He was afraid, however, that it was not going to be as easy as that.

Mr Bjerrum had shown that his results were in accordance with Skempton's  $\lambda$ -theory, when  $\lambda$  was equal to 0. In many cases  $\lambda$  was actually rather small, so that it might be a good approximation to reckon  $\lambda = 0$ . It should be borne in mind, however, that Skempton's theory presumed an isotropic consolidation under a uniform all-round pressure, such as was generally the case in the laboratory. Therefore, it was not surprising that tests on remoulded and isotropically-consolidated samples confirmed the theory.

In nature, however, clays were generally consolidated under different vertical and horizontal pressures, and in that case the  $\lambda$ -theory had to be revised, as had been done by Gibson and himself. In the extended  $\lambda$ -theory different expressions were obtained for the shear strengths found by different undrained tests, and even the simplifying assumption that  $\lambda = 0$  did not eliminate the differences as in the simple  $\lambda$ -theory. The reason for that was that Mr Bjerrum's hypothesis concerning the unaltered effective major principal stress at constant water content was not valid in all cases.

If one considered an anisotropically consolidated clay in situ, where it was subjected to the vertical pressure  $p$  and the smaller horizontal pressures  $Kp$ , and if, for the sake of simplicity, a sample were supposed to be cut loose instantly, then it had to be subjected to an effective pressure  $p_0$  in all directions. If  $p_0$  should be equal to  $p$ , as presumed by Mr Bjerrum, that would require  $\lambda = \infty$ . As  $\lambda$  was equal to 0, the result by that process would be the minor effective stress, which would remain approximately constant. Further, if an undrained unconfined compression test were made on the sample, neither the major nor the minor effective stress would remain constant, in spite of the constant water content. That was due to the fact that in that case the deformations were expansion and recompression, and the corresponding value of  $\lambda$  was far from being 0 or  $\infty$ .

Mr Bjerrum might argue that that was a case of "pre-consolidation" for which he had not supposed his theory to be valid. In a sense, of course, a sample in which the effective stresses were smaller than the stresses in situ might be called "pre-consolidated," but then practically all natural samples would be pre-consolidated, even if they were taken from normally consolidated clay deposits under ideal sampling conditions.

That fact, which implied that for anisotropically consolidated clays different shear strengths might be obtained at the same water-content, considerably reduced the practical importance of Mr Bjerrum's theory. The present problems concerning the testing of natural samples were, therefore, still unsolved. On the other hand, Mr Bjerrum's theory was of considerable theoretical interest and undoubtedly of great value for research work.

Mr Bjerrum had endeavoured to make his definitions and explanations as clear as possible and had, on the whole, succeeded in doing so. In the Chairman's opinion, however, one point needed further clarification. In defining the cohesion and the frictional resistance, Mr Bjerrum had spoken of the shear strengths on different planes through a point. That might be understood in two ways: first, as the potential shear strengths on the different planes in the case of a fixed stress system; and, secondly as the shear strengths which could be developed in the different planes, when those in turn were acting as sliding planes. In the latter sense the problem had been treated by Gibson and himself, whereas Mr Bjerrum was evidently concerned with the former case.

In conclusion, he asked Mr Bjerrum whether he had tested his theory on any natural un-remoulded clay samples or on any anisotropically-consolidated, remoulded samples prepared in the laboratory. The results of such tests would be of the greatest practical, as well as theoretical, interest.

It should be noted, however, that it would not be sufficient to carry out the shear test on a sample without removing it from the device in which it had been consolidated. In order to simulate actual sampling and testing conditions the consolidated sample would have to be relieved of all external pressures before being brought to failure. Otherwise, quite different results would be expected.

**M. J. Florentin** dit qu'il se permet de remercier les conférenciers et il remarque que tous les laboratoires n'ont pas abandonné les essais de torsion. Il rappelle simplement que les essais de torsion peuvent se faire également sur échantillons cylindriques intacts.

Ceci étant, il se reporte à une phrase du Professeur Haefeli dans laquelle il nous dit que pour des essais "non drainés," il préfère faire usage de l'appareil triaxial. Or, c'est justement dans ce cas qu'il veut exprimer la crainte que la compression triaxiale classique a le moins de chance de donner des valeurs expérimentales réellement indépendantes de celles que comportent les hypothèses de départ.

Son doute s'applique au cas de sols argileux saturés d'eau (l'eau étant elle-même supposée incompressible) et à dilatation cubique négative. Pour ces matériaux, la rupture produite par des contraintes appliquées rapidement se fait probablement sous  $\phi = 0$ , mais il pense que ceci résultait plus des hypothèses physiques faites ci-dessus que des essais triaxiaux eux-mêmes conduits rapidement et sans drainage. Il est probable que l'on trouverait le même résultat si, au lieu d'essais rapides, on faisait des essais lents à condition qu'il s'agisse toujours d'essais non drainés.

Il croit que dans les essais triaxiaux non drainés, on ne fait, malgré une contrainte latérale,  $n_3$ , apparemment croissante, qu'un seul et même essai qui donne le même résultat, ce qui prouve que les échantillons étaient homogènes et que l'appareil est fidèle. Mais pour ce même essai, on trace plusieurs cercles du même diamètre, ce qui, sur le diagramme de Mohr, ne peut manquer géométriquement de donner  $\phi = 0$  sans pour cela démontrer expérimentalement l'hypothèse de départ.

Considérons tout d'abord l'essai de compression simple. Si l'échantillon a été préparé avec soin, on observe, au moment d'un essai de rupture rapide et sans drainage, que la surface de l'échantillon, même si elle était mate, devient brillante. (Ceci est la propriété qui permettait à M. Freundlich de distinguer les sables à dilatation cubique négative des sables à dilatation cubique positive. Cette propriété a d'ailleurs été reprise pour différencier rapidement les matériaux plastiques des matériaux non plastiques.) Dans cet essai, on obtient une valeur de la résistance à la compression simple :

$$R_c = n_1 - n_3 \text{ avec } n_3 = 0.$$

Considérons maintenant un essai triaxial sous une étreinte  $n_3$ . A l'intérieur de la membrane en caoutchouc qui sépare l'échantillon de la cellule, c'est par un mince film d'eau (l'échantillon étant à dilatation cubique négative) que se réalise rapidement le contact entre l'échantillon et la membrane. Ce film se met à la pression  $n_3$  ; tout se passe alors comme si on refaisait rapidement l'essai de compression simple sous une étreinte latérale  $n_3$  qui est une contrainte neutre. L'essai ne diffèrait donc pas de l'essai de compression simple. Tout se passe comme si l'on exécutait celui-ci dans une nappe d'eau, à différentes profondeurs sous le niveau supérieur, les échantillons étant amenés à la cote ou l'on veut faire l'essai rapidement, sans qu'il s'établisse un nouvel équilibre de l'eau des pores. On obtient donc la valeur  $R_c$ , mais au lieu de tracer un cercle confondu avec celui de compression simple, on trace un cercle décalé de l'origine de la valeur  $n_3$ .

Tout ceci peut sembler un paradoxe. Il ne s'agit pas pour l'instant de mettre en doute

qu'un matériau sollicité rapidement in-situ puisse n'être soumis qu'à une étreinte neutre. Mais on ne peut estimer l'avoir vérifié en faisant l'essai avec un appareil qui n'introduit qu'une étreinte latérale neutre. On démontre simplement expérimentalement qu'une étreinte neutre ne produit pas d'accroissement de la résistance à la compression d'un échantillon rompu rapidement, mais on ne démontre pas que  $\phi = 0$ . (Toujours pour un sol saturé à dilatation cubique négative, sollicité par des efforts appliqués rapidement.)

Il voudrait d'ailleurs signaler que dans un colloque de Mécanique Rationnelle qui s'est tenu à Poitiers à la fin d'avril pour célébrer le tri-centenaire de la mort de Descartes, M. Mandel, Maître de Conférence à l'École Polytechnique, a repris, avec les équations de la mécanique rationnelle, le problème de la consolidation d'une couche argileuse soumise à des charges distribuées sur sa surface. Il a repris intégralement toutes les hypothèses de M. Terzaghi sauf les conditions au temps  $t = 0$ , immédiatement après application de la charge. On sait que les développements mathématiques de la théorie de M. Terzaghi impliquent qu'à l'origine le tassement initial est nul. M. Mandel suppose que c'est la dilatation cubique initiale que est nulle et il aboutit à des expressions mathématiques qui, autant que M. Florentin s'en souviennent, ne sont intégrables que dans quelques cas particuliers. Ceci lui aurait montré que ses hypothèses initiales conduisent à un tassement instantané important malgré que, comme M. Terzaghi, il suppose l'échantillon saturé et l'eau incompressible.

Il ne s'agit pas pour l'instant de discuter cette hypothèse. Il faudrait d'ailleurs, d'une part, traduire en caractéristiques de mécanique du sol les coefficients introduits par M. Mandel et, d'autre part, se mettre en mesure de pouvoir vérifier expérimentalement ses hypothèses, en ayant soin de choisir un appareil qui ne les exclut pas systématiquement. C'est ainsi, par exemple, qu'avec l'oedomètre, les deux conditions au temps  $t = 0$  ne sont pas différentes puisque, l'expansion latérale étant impossible, dire que la dilatation cubique est nulle revient à écrire que le tassement initial est nul.

Il n'avait nullement l'intention de parler de l'hypothèse faite par M. Mandel qui, se rapportant au problème de consolidation, pouvait sembler hors sujet dans une conférence sur la résistance au cisaillement. S'il l'a fait, c'est parce que de nombreuses méthodes expérimentales ont été, ces jours-ci, accompagnées des mots "échantillons à dilatation cubique, positive ou négative" et qu'il lui a semblé intéressant de signaler que, dans le cas où l'hypothèse de M. Mandel pourrait être vraie, il se pourrait que la surpression initiale de l'eau des pores ne soit pas totale. Il y a des études expérimentales à faire. Toutefois, il faut tenir compte que dans de nombreux cas, tant théoriques que pratiques, les deux hypothèses conduisent probablement à des conclusions analogues. Tout dépend, il pense, des conditions aux limites du problème.

**Dr A. W. Skempton** referred to the problem of determining  $\phi_e$ , the true angle of internal friction of a clay, from the inclination of the shear planes in a compression specimen. He emphasized that that angle could not be determined in any simple way from the angle of shearing resistance found from any test such as the triaxial, Dutch cell, shear box, or ring shear test. He felt that Hvorslev had made a great contribution to our understanding of the problem of the shear strength of cohesive soils, and it was unfortunate that his work had been to some extent ignored in America, where the opinion had been expressed that there was no true cohesion in clays and that the inclination of the Mohr envelope for drained triaxial tests was probably the true angle of internal friction. Some doubts had also been expressed whether there was any relationship between the true angle of internal friction and the inclination of the failure planes in a compression specimen. Tests had been carried out at the Imperial College on a wide range of clay types (remoulded) using Hvorslev's experimental technique, from which  $\phi_e$  had been determined. In addition, unconfined compression tests had been made on those clays remoulded at different water contents, and the inclination of the slip planes carefully measured directly they appeared on the sample. There was found to be a good correlation between the figures for the true angle of internal friction as measured

in these two ways when the shear box results had had an energy correction applied (vide A. W. Bishop). He was glad to hear that Professor Haefeli agreed that the inclination to the horizontal of the slip planes in a compression specimen was related to the true angle of internal friction.

Dr Skempton asked Professor Haefeli why the tension test gave lower values of the strength than the unconfined compression test and what use was made of the tension test in practice.

Dr Skempton observed that Fig. 6 in Mr Bjerrum's Paper was most important. Two series of direct shear tests, both drained and undrained, had been carried out on Zurich tile clay, starting the consolidation process from two different initial water contents. From the two strength/water-content lines so obtained, a unique relation between true cohesion and water content had been calculated. That result added further weight to Hvorslev's hypothesis that a true cohesion did, in fact, exist, and he emphasized again that Hvorslev's expression for shear strength was by far the most generally valid basis for fundamental work so far produced.

**Mr T. K. Huizinga** observed that the true cohesion of a soil resulted from the interaction of the adsorbed water layers surrounding the clay particles. When the particles approached each other more closely, the true cohesion increased, as also did the effective pressures. If, however, chemicals were present in the pore water, the true cohesion would be different, and he therefore wondered if there was, in fact, any "true" cohesion. Perhaps that was why no true cohesion was recognized in the United States.

**Mr A. W. Bishop** referred to the energy correction which he had mentioned previously in the Conference. If a series of drained triaxial tests were carried out on sand, the angle of shearing resistance obtained from the peak point could be plotted against the initial porosity of the sample. If the energy correction were applied to these test results, a second curve was obtained. A series of consolidated undrained triaxial tests on sand gave results which lay close to the corrected curve for the drained tests.

M. Florentin had said that the all-round pressure in a triaxial test did not affect the pore-water pressure in a sample. The evidence presented by Professor D. W. Taylor in the 10th Report on Shear Research on Clays, and evidence to be presented shortly in *Géotechnique* by Bishop and Eldin<sup>1</sup> on sands showed that this reasoning was not valid. It had, in fact, been found that the pore-water pressure in the sample responded exactly to changes in the all-round pressure, and so long as the volume of the sample remained constant, it was found that the apparent angle of shearing resistance was zero.

**Mr L. F. Cooling** said that, in the torsion tests mentioned by Dr Skempton, the position of the Mohr's circle at failure would not be as shown, owing to the effect of capillary pressures which had not been measured. He agreed with Mr Huizinga that the true cohesion depended upon the salts present in the pore water. Tests had been carried out on bentonites at the Building Research Station which showed that effect. A simple slaking test would also indicate the effect. If balls of clay at about "puddle" consistency were immersed in water, a clay saturated with a sodium base would tend to slake quickly, whereas one saturated with an aluminium base would resist slaking and maintain its strength for a very long time.

**Mr H. B. Sutherland** said that perhaps he and Dr Skempton had interpreted in different ways what Professor A. Casagrande had said in discussion regarding the existence of true cohesion in clays. He did not believe that Casagrande was of the opinion that no true cohesion existed in clays.

Regarding the correlation of the true angle of internal friction and the inclination of the failure planes in compression specimens, it was of interest to report that when in Mexico City

<sup>1</sup> BISHOP, A. W., and ELGIN, G., 1950. Undrained triaxial tests on saturated sands and their significance in the general theory of shear strength. *Géotechnique*. 2: 13-32.

he had been shown samples of clay where the inclinations of the failure planes of compression specimens were consistently found to be 45 degrees.

**Professor R. Haefeli**, in reply, said that in practice he used the cone tests especially as a method of determining the sensitivity of remoulded and undisturbed samples. He agreed that it would be too conservative in some cases to use the minimum shear strength (after failure) for design purposes. This might only be necessary when some disturbance of the ground such as explosions or earthquakes, was likely. There had been, in fact, a case in which failure of a 7-degree slope in sensitive marl had been induced by a small explosion.<sup>1</sup> In Switzerland a number of slow progressive failures had also taken place, and therefore precautions had to be taken against the possibility of that happening. He would recommend that the design of dams should be based upon the maximum shear strength, and that a normal factor of safety be used. But a check should be made so that, if the residual strength (which was not identical with the minimum shear strength) were used, the factor of safety did not fall to unity.

He was very interested in the true cohesion and true angle of internal friction discussed by Dr Skempton. He pointed out, however, that, if the inclinations of the failure planes were measured in samples which had been consolidated anisotropically, it would be expected that the structure of the clay would also become anisotropic, and hence they would not bear a simple relation to the true angle of internal friction. Because of that it was the practice now at Zurich to consolidate the samples under an isotropic pressure in large water tanks. Not only was the vertical stress increased to obtain a failure condition, but experiments had been conducted successfully on quite short samples where the horizontal stress had been increased to induce failure, and in that way the angle between the shear plane and the main principal stress (active pressure) had been obtained in two ways.

On the question of tensile tests, he felt that it would be interesting to carry out tests where the tensile stress was applied isotropically and not merely in one principal direction. That was, however, something for the future.

**Mr L. Bjerrum**, replying to the discussion, emphasized that it was very difficult to investigate experimentally the effects of an anisotropical consolidation. The reported tests were carried out with a remoulded and reconsolidated clays, whereas in nature a soil layer was seldom found which showed sufficient uniformity for the cutting of specimens with equal initial conditions. In the American "Triaxial Shear Research" several tests with undisturbed clays were reported and the most uniform non-preconsolidated clays showed the same definite relation between water content at failure and compressive strength as was found with remoulded and reconsolidated clays.

Moreover, an anisotropic consolidation might well induce a preferred orientation of the flaky grains and lead to a condition of true structural anisotropy. In that case there were different magnitudes of the cohesion in the different planes through the same point. Perhaps the angle of internal friction might vary too and, consequently, the significance of the inclinations of the failure planes was somewhat obscured.

The Chairman had mentioned too the change of stresses during sampling. If under a condition of constant volume the external forces were removed, the sample would, in general, compress in one direction and expand in other directions. In such a case the increase of the principal stress in the direction of compression could be neglected and only the decrease of the principal stresses in the direction of swelling needed to be considered. During sampling there was a lateral swelling, and therefore both effective horizontal pressures would remain approximately constant, and the effective major principal stress decrease. That corresponded exactly to the case of  $\lambda = 0$ .

<sup>1</sup> VON MOOS, A., and RUTCH, R. F., 1946. Ueber einen durch Gefuegestoerung verursachten Seeufereinbruch (Gerzensee, Kt. Bern). (On erosion caused by disturbance of terrain at Lake Gerzen, Switzerland.) *Mitteilungen der Versuchsanstalt für Wasserbau und Erdbau, Zurich*. No. 10.