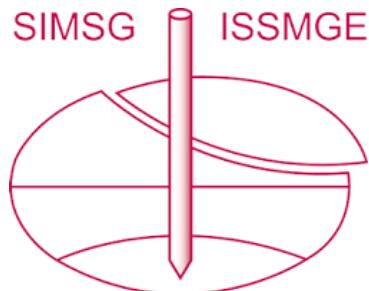


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Foundation of Buildings and Dams, Bearing Capacity, Settlement Observations, Regional Subsidence

Fondations des constructions et des barrages, charges admissibles, observations des tassements, affaissements régionaux

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M. N. Chebib, Egypte

Dr. L. Bjerrum, Norway

Prof. H. Lundgren, Denmark

Prof. E. Schultze, Germany

Mr. S. J. Button, Great Britain

MM. P. Habib et F. A. Soeiro (présenté par M. Habib), France

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M. L. Šuklje, Yougoslavie

Mr. Tan Tjøng-Kie, Netherlands

Mr. W. H. Ward, Great Britain

Written Discussion / Discussion par écrit:

M. M. Bachelier, France

Dr. L. Bendel, Switzerland

Dr. C. L. Dhawan, India

Le Rapporteur général

Comme vous le savez, ma section comporte quelques uns des chapitres les plus importants de la mécanique des sols.

Or le temps mis à notre disposition pour cette discussion est très réduit. Non seulement il m'est donc impossible de vous lire mon rapport, mais il m'est également impossible de vous en lire un résumé. En effet, j'ai reçu jusqu'à la dernière minute un nombre de discussions qui est certainement supérieur à 15. Il m'est donc impossible, matériellement, de permettre à tous ceux qui ont voulu intervenir de venir exposer ici leurs observations. Toutefois, je dois constater la qualité des papiers qui m'ont été remis et que j'ai dû d'ailleurs lire jusqu'à la dernière minute pour certains. Ces papiers seront publiés dans le troisième volume des Comptes-Rendus de la conférence. Je m'excuse donc auprès des personnes qui ne seront pas favorisées ce matin, mais je leur demande de considérer qu'elles auront cependant la possibilité de faire connaître leur opinion sur le sujet qu'elles auront choisi.



M. M. Buisson,
Rapporteur général
Session 4
General Reporter
Session 4

Je vais commencer par nommer les auteurs qui ne pourront exposer verbalement leurs discussions. M. Šuklje m'avait proposé deux interventions, l'une sur le tassement séculaire, l'autre sur la stabilité. M. Bendel m'avait également proposé une intervention concernant les formules analogues à celle de Buisman pour relier la compressibilité à la résistance. Vous savez que, dans mon rapport, j'avais insisté sur l'intérêt pratique de telles formules. C'est donc avec beaucoup de peine que je suis obligé de ne pas les faire intervenir. Ensuite, j'ai également à m'excuser auprès de M. Dhawan et M. Haber-Schaim dont les communications m'ont été remises, comme celle de M. Bendel, à la dernière minute.

The General Reporter opens the discussion and expresses his regrets that owing to the short time at his disposal he will not be able to give the floor to all the speakers who have put their names down for the discussion.

Further, he draws attention to a printing mistake which slipped into his General Report (Proceedings 1953, vol. II, p. 341); the coefficient of consolidation should read: $7 \cdot 10^{-4} \text{ cm}^2/\text{sec}$ instead of $7 \text{ cm}^2/\text{sec}$.

M. N. Chebib

Ainsi que l'a si bien dit M. Buisson dans le second paragraphe de son rapport au sujet des dispositions choisies par les constructeurs dans le but de réaliser une économie maximum compatible avec la sécurité, il m'est agréable de vous rapporter le résultat concluant de mes efforts pour hausser la valeur de la charge moyenne admissible sur le sol sous des radiers généraux. Cette valeur est limitée ordinairement par la valeur des tassements différentiels, tout au moins dans le cas des sols du Caire, dont vous trouverez plusieurs profils dans la communication du Dr Hanna (Comptes Rendus 1953, vol. I, p. 366) et ceci sans avoir recours à des radiers rigides qui sont extrêmement coûteux.

Par un choix judicieux de la position des colonnes dans le bâtiment, par exemple en posant les colonnes de la périphérie de 1 à 1,5 m vers l'intérieur et en limitant le radier aux côtés extérieurs de ces colonnes, ce qui fait que le radier est plus petit que la surface du bâtiment, il en résulte que les efforts sur le sol sont répartis inégalement avec une valeur maximum sur le pourtour extérieur. La valeur moyenne sous les colonnes du pourtour excède d'environ 100% les valeurs moyennes sous les colonnes centrales (Fig. 1).

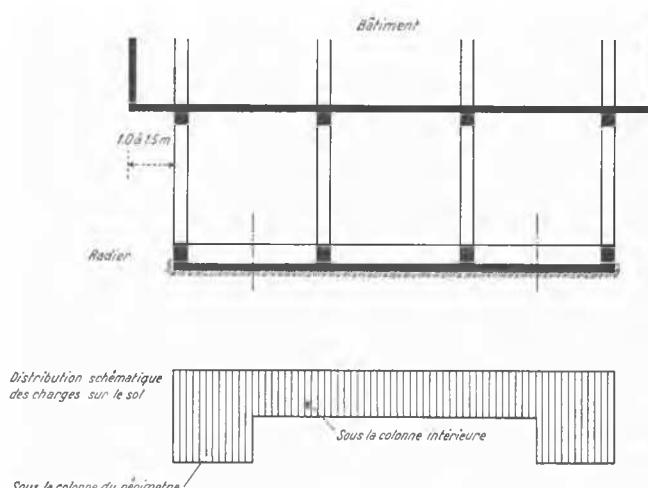


Fig. 1 Répartition des efforts sous le bâtiment
Pressure Distribution Beneath the Building

Par ce moyen, j'ai atteint des charges excédentaires aux charges originales du sol de l'ordre de $1,8 \text{ kg/cm}^2$ et des tassements totaux de l'ordre de 8 cm, et ceci sans aucun préjudice aux bâtiments conçus généralement sous forme d'ossature de béton armé avec remplissage de murs en briques.

De même dans le cas de bases isolées, comme l'a si bien étudié M. Schultze (Comptes Rendus 1953, vol. I, p. 454), il m'est apparu que les tassements différentiels étaient surtout dus, au moins dans les bâtiments d'habitation, à l'effort cumulatif des bases adjacentes, influence à laquelle j'ai remédié en augmentant l'unité de charge sur le sol dans les bases de la périphérie.

En résumé l'ingénieur peut diminuer les tassements différentiels, sans avoir recours aux constructions rigides, en faisant varier les charges unitaires sur le sol. Cette variation peut être établie, suivant les propriétés du sous-sol sous le bâtiment et la valeur de la charge moyenne.

The engineer can reduce the differential settlements, without using rigid structures, by taking different values for the pressures on the soil. These values are determined by the soil properties and the total mean pressure.

Le Rapporteur général

Cette communication comme vous le voyez se rapporte et complète les intéressantes observations que notre Président M. Hanna a faites dans les Proceedings (Comptes Rendus 1953, vol. I, p. 366). Par malheur, cette communication ne m'était pas parvenue, comme je l'ai signalé dans les Proceedings, avant de faire mon rapport. Toutefois, j'appelle votre attention sur ce qui a été signalé par M. Hanna, car ses constatations sont de première importance. Elles se reliaient d'ailleurs fort bien à celles que j'ai faites moi-même à Tunis sur les vases qui constituent les sols de la ville basse de Tunis. L'expérience montre que l'on peut augmenter le travail du sol sous des radiers qui n'ont pas été démolis. Je partage l'opinion de notre Président puisque les observations faites montrent que le tassement n'est pas important; on peut penser que l'on se trouve sur la branche très faiblement ascendante de la courbe d'hysteresie. Mais, indépendamment de cette cause de faible tassement, je crois aussi qu'il faut voir là une conséquence du fait que l'on ne touche pas au radier. L'on n'impose pas au sol des vibrations qui diminueraient sa capacité portante et qui augmenteraient également sa compressibilité.

The General Reporter states that Mr. Chebib's paper confirms the results obtained by Mr. Hanna. He regrets that he was not in a position to comment on Mr. Hanna's paper in his General Report since it was submitted too late. The General Reporter has himself made similar observations in Tunis.

Dr. L. Bjerrum

In Norway we are very often faced with the problem of estimating the depth to which an excavation in soft clay can be taken without risking a shear failure, which means that the bottom of the excavation is pressed up under the weight of the overburden around the excavation.

In shallow excavations an estimate of the critical depth can be based on a conventional stability analysis with a circular sliding surface. In deep excavations, however, this leads to too high safety factors, as a failure can take place without mobilizing the total shear strength of the upper clay layers.

In the past few years, research carried out in England has now succeeded in the development of formulæ for the bearing

capacity of deeply buried foundations. These formulae may easily be turned around and used also for a calculation of the stability of deep excavations. In the formulae for the bearing capacity we need only to find the depth, in which the bearing capacity is equal to two times the overburden pressure, involving the same shear stresses as in an excavation at the moment of failure. This results in the expression:

$$\text{critical depth} = N_c \frac{\text{shear strength}}{\text{unit weight}}.$$

In this formula the shear strength is the value found below the bottom of the excavation. The coefficient N_c depends on the width/depth ratio and the form of the excavation. The value of N_c can preferably be found in a paper delivered by Professor Skempton to the Building Research Conference in 1950.

In connection with the design of a new subway in Oslo, the Norwegian Geotechnical Institute took the opportunity to study the validity of this formula.

A circular test shaft of 1.4 m diameter was excavated through an upper crust of stiff clay down to a soft sensitive clay with a shear strength of 0.12 kg/cm². The excavation was continued to a depth of ca. 7 m and at this stage a failure took place. The bottom of the shaft heaved ca. 2 m.

At the moment of failure the value of the coefficient N_c was 10.8 corresponding to a width/depth ratio of 5. According to the bearing capacity formula a design value of 9 is found.

This means that the full scale experiment shows satisfactory agreement with the theory. At the stage of failure the theoretical safety factor was 0.86, which means that the use of the bearing capacity formula in the actual case would lead to a critical depth which is a little on the safe side.

L'auteur examine la question de la profondeur maximale à donner à des excavations dans une argile tendre sans risquer la rupture par glissement. Il montre que la détermination de cette profondeur critique par l'interprétation d'une formule de Skempton pour la stabilité des fondations profondes a donné de bons résultats dans un cas pratique.

Prof. H. Lundgren

The problem to be discussed now, will be illustrated by Fig. 2, which demonstrates that the ultimate bearing capacity of a continuous footing is $q = \frac{1}{2}\gamma BN_y$. However, there is

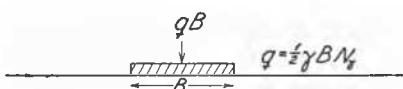


Fig. 2 Ultimate Load on a Continuous Footing on Sand
Pression maximum sur une fondation longue sur sable

great disagreement as to the value of the coefficient N_y . Mr. Buisson states in his general report that the problem now should be considered as solved. However, it is actually the opposite, since it seems that today we know only that the value of N_y is probably somewhere between 14.8 and 36.0. Mr. Buisson concludes that Mr. Mortensen and I in our paper (Proceedings 1953, vol. I, p. 409) must have made a mistake; as I shall show to you now, we are in complete disagreement with the General Reporter.

First, Mr. Buisson does not concede that we can find and integrate two ordinary differential equations. This objection must be due to a misunderstanding, as our equations in principle are exactly the same as developed by v. Kármán (1926),

Caquot (1934) and Ohde (1938) for similar problems of earth pressures. Of course, we must, like all our fellow authors, solve the equations by a numerical, step by step integration. Therefore, I can take Mr. Buisson's statement only as a further indication that the task of a General Reporter is much too heavy to cover such a large section.

Second, Mr. Buisson draws the conclusion that our approximate value $N_y = 14.8$ must be wrong, because Meyerhof (1951),

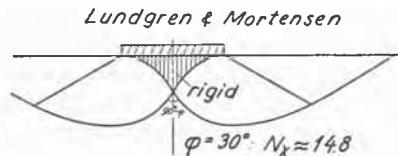


Fig. 3 Statically Correct Figure of Rupture
Figure de rupture statiquement correcte

Caquot (4/4) and Ohde find a value of about 22.7. Now, in order to see how our theory of plasticity works, let us consider Fig. 3. The curvilinear triangle under the footing is rigid and moves down as a whole with the footing. On the other hand,

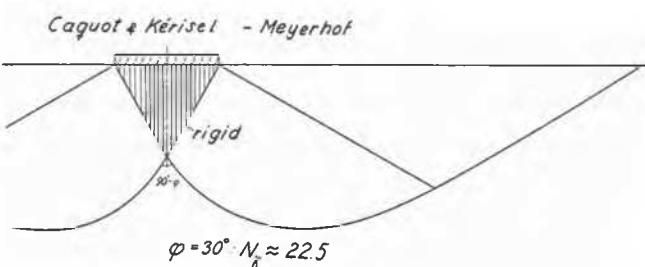


Fig. 4 Figure of Rupture Assuming Rigid Triangle
Figure de rupture supposant un triangle rigide

Fig. 4 shows how the problem is solved by Caquot and Meyerhof who both assume a rigid straight triangle under the footing with an angle at the apex of $90^\circ - \varphi$.

In Mr. Buisson's report, we must first object that Ohde, as far as we know, has never published the quoted result of 22.5. The reference of Caquot to Ohde (1949) is incorrect, but we suppose that Caquot is referring to the result that Meyerhof (1951) found by checking his theory by Ohde's method for the calculation of passive earth pressures on rough walls. However, Meyerhof applies both methods to the same figure of rupture, which also Caquot considers. Hence the agreement of their results is not surprising. But the figure is statically incorrect. It is a curious fact, however, that Ohde (February 1952) has actually shown, that the figure of rupture investigated by Meyerhof and Caquot must be incorrect and give too high results. Ohde states that the limiting rupture line must touch the base of the footing.

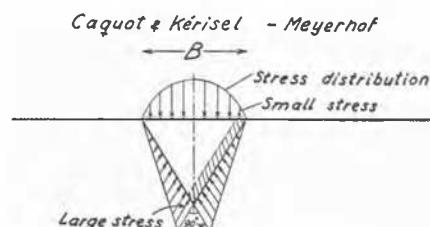


Fig. 5 Demonstration of Rupture in the "Rigid" Triangle
Preuve de la rupture dans le triangle «rigide»

The proof of the incorrectness is very simple, although I must admit it took us some time to find the mistake; since *Caquot* and *Meyerhof* are such brilliant people, they also make brilliant mistakes. Both suppose that the rigid zone below the footing is enclosed by two straight lines, which will also be seen in Fig. 5. Let us consider the dotted line close to and parallel to the right boundary of the rigid zone and, furthermore, the equilibrium of the shaded area. This is, on the one side, acted upon by stresses which increase linearly from the edge of the footing to the apex, and which form an angle φ with the normal. The stress on the shaded area from the footing is infinitely small, whereas the stress on the other short side of the shaded area (near the apex) is large. Considering also the relatively small weight of the shaded zone, it can be shown by a numerical analysis, which need not be given here, that the resulting stress on the dotted line makes an angle of more than φ with the normal. This means that there must be a rupture along this line, i.e. the figure is statically incorrect. The only figure that has been shown, until now, to be statically correct is Fig. 3.

Since the other authors do not consider the deformations, I shall, because of lack of time, leave them out of discussion today. However, for conclusion I must state that the clue of the whole problem is in the "kinematically admissible" figures of ruptures and that all figures considered hitherto are, for this reason, incorrect. It should be added that, according to recent (unpublished) investigations by Mr. *K. Mortensen*, our whole concept of frictional angles needs fundamental revision.

L'auteur répond aux remarques du Rapporteur général (*Comptes Rendus* 1953, vol. II, p. 339) sur son mémoire (*Comptes Rendus* 1953, vol. I, p. 409). La différence entre la valeur $N = 14,8$ et $N = 22,7$ ainsi qu'indiquée par M. *Caquot* provient du fait que ce dernier se base sur une figure de rupture théoriquement incorrecte (*Comptes Rendus* 1953, vol. I, p. 337). L'auteur précise certains termes qui n'ont pas été correctement interprétés par le Rapporteur général.

Le Rapporteur général

Certains se sont plaints, il y a deux jours que les discussions n'étaient pas animées. Je pense que maintenant votre opinion va commencer de changer. Il est certain que le sujet exposé est de ceux qui méritent réflexion. Vous serez donc d'accord avec moi qu'il est absolument impossible dans une séance comme celle-ci de répondre point par point à M. *Lundgren*. Il est bon de réfléchir aux objections qui viennent d'être faites et je propose que M. *Lundgren* se réunisse avec nous entre deux séances, par exemple, pour examiner son papier, que j'avais reçu seulement dix minutes avant le commencement de cette séance.

Après examen de la discussion de M. *Lundgren*, je constate, comme il le fait remarquer, que son exposé avait été mal interprété dans mon rapport. Cela vient du fait que les termes employés par M. *Lundgren* dans son exposé «Equations différentielles ordinaires» m'ont incité à penser qu'il s'agissait d'équations différentielles pouvant être résolues d'une façon effective par l'analyse. La méthode de résolution des équations de *Kötter* transposée par M. *Lundgren*, est, dans le fond, voisine de celle employée notamment par MM. *Caquot*, *Kerisel* et *Meyerhof*. La discussion reste néanmoins ouverte, car il s'agit de savoir si les solutions, analytiquement satisfaisantes, proposées par MM. *Lundgren* et *Mortensen*, le sont expérimentalement. A ma connaissance, les essais effectués jusqu'à maintenant ne semblent pas montrer des formes de surface limite aussi incurvées que celles proposées. Seuls des essais pourront peut-être départager les opinions. Je pense, comme M. *Mandel*, que le point de vue théorique exprimé par M. *Lundgren* est

exact et conforme à ce qui avait déjà été exposé par M. *Bonneau*¹⁾. Comme le fait justement remarquer M. *Mandel* il est toutefois probable que le schéma de calcul est trop simple et que l'angle de frottement s'élève, dès que le chargement est suffisant par augmentation de compacité. C'est sans doute pour cette raison que les résultats trouvés par la méthode simplificatrice classique de M. *Caquot* coïncident bien avec les résultats expérimentaux, autant qu'il est possible de le constater en égard aux variations rapides des facteurs N_y et N_q en fonction de l'angle de frottement.

Il faut en tous cas remercier MM. *Lundgren* et *Mortensen* de leurs recherches qui ne peuvent manquer d'aboutir pratiquement à la résolution de problèmes dont la solution est loin d'être évidente.

The General Reporter confirms that he did not interpret quite accurately some expressions in the paper submitted by Messrs. *Lundgren* and *Mortensen*. Nevertheless the question remains open as to whether the theoretical considerations have been confirmed by the tests.

Prof. E. Schultze

The discussion concerns the very interesting paper by Dr. *Meyerhof* (*Proceedings* 1953, vol. I, p. 44).

The calculation of the factor of safety of a foundation under vertical eccentric loads was made on the basis of the theory of *Prandtl* and *Buisman* first by Mr. *de Beer* (1949) and then extended by myself (1952) to eccentric and obliquely loaded foundations. Apart from this *Ohde* (1951) determined somewhat lower bearing capacity factors for oblique loads. Although *Meyerhof* (and partly *Ohde*, too) started from hypotheses other than those of *Prandtl* and *Buisman*, the results found by myself in some cases differed but slightly: the method to be used for the calculation of the bearing capacity factors should be therefore the simplest. *Meyerhof's* semi-empirical method for the calculation of the bearing capacity factors has the disadvantage of supplying formulæ considerably more complex than the others, and furthermore of requiring, knowledge of an earth pressure coefficient the value of which cannot be determined immediately but must be found through experiments. Yet no corresponding improvement in accuracy can be obtained.

All methods demonstrate the rapid decrease of the bearing capacity factors with the inclination α of the load and the extent of eccentricity. In the case of a more shallow foundation D with a medium angle of internal friction ($\varphi = 30^\circ$), according to all proposals, very thick retaining walls and eccentric and obliquely loaded foundations are necessary, often these considerably exceed the size hitherto considered as sufficient. On re-calculating existing retaining walls, it has nearly always been found that their factor of safety against foundation failure lies far below 1.0.

All mentioned methods are theoretically irrefutable; yet their results do not agree with practical experiments. In certain cases this may be explained by the fact that the angles of internal friction φ are in reality larger than in the calculation; namely the bearing capacity factors increase rapidly with larger angles of internal friction which is observed in all studies concerning the resistance of the earth. Certainly this is not always the case. A mistake must have been made in the fundamental hypotheses of the above mentioned methods; this mis-

¹⁾ *Bonneau*, M. (1938): Etude de la fondation rectiligne et de la fondation circulaire. *Annales des Ponts et Chaussées*, avril, p. 497.

take, however, has not yet been discovered and leads to exceedingly low factors of safety. In making practical use of such studies, great care is needed to avoid uneconomical design.

References

- Beer, E. de (1949): Grondmechanica, Deel II: Funderingen. Standard Boekhandel, Antwerpen, Brüssel, Gent, Leuven, p. 41-51.*
- Ohde (1951): Grundbaumechanik, in: Hütte, des Ingenieurs Taschenbuch, vol. III, p. 923-925.*
- Schultze, E. (1952): Der Widerstand des Baugrundes gegen schräge Sohlpressungen. Bautechnik 29, p. 336-342.*

L'auteur discute l'article de M. Meyerhof sur la résistance à la rupture sous des charges excentrées verticales et sous des charges obliques. Il relève qu'il a obtenu des résultats très rapprochés par une méthode plus simple. Tous les calculs donnent une diminution rapide de la force portante en fonction de l'inclinaison de la charge et de l'excentricité. Des expériences sur des ouvrages existants montrent que les résultats des études théoriques sont par trop pessimistes et qu'ils ne tiennent pas suffisamment compte du comportement réel des massifs. Il convient d'appliquer les études théoriques avec une certaine réserve.

Mr. S. J. Button

Mr. Buisson, in his report, has suggested that in estimating the bearing capacity of a two-layer cohesive soil, the slip surface is more likely to be made up of circular arcs, having different radii in the two layers.

Even if this is so, the values of the bearing capacity factor in Figure 2 of my paper, will lie within the limits defined by the horizontal line at $N_c = 5.5$ which represents a very thick upper layer and the straight line passing through the point $N_c = 0$ which represents the condition when the upper layer is very thin. In other words, these both represent homogeneous conditions.

The values of N_c when C_2 is greater than C_1 are substantially the same whether the different radii are assumed or not, since as the strength of the lower layer is increased, the slip surface soon becomes tangential to its upper surface and then N_c remains stationary.

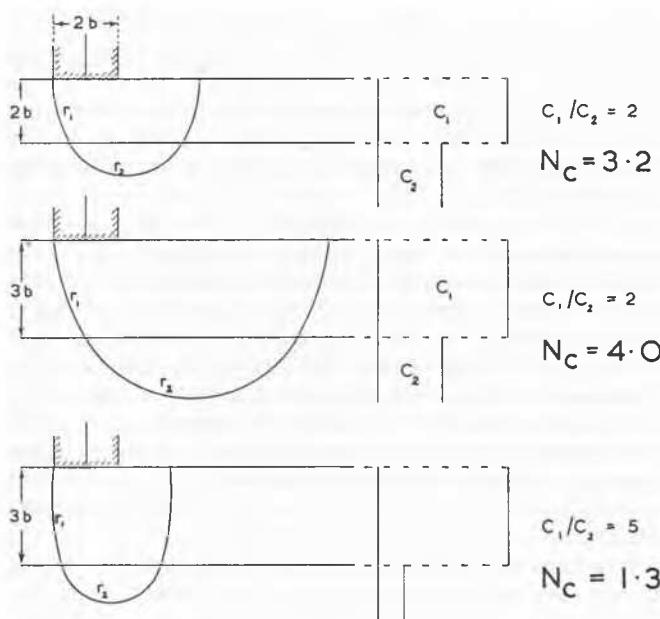


Fig. 6 Bearing Capacity Factors for a Two-Layer Cohesive Soil
Facteurs de portance pour un sol cohésif bicoche

I have calculated the bearing capacity factors for a soft lower stratum for a few cases, assuming that the radii of the slip circles are proportional to the cohesion, and the bearing capacity factors, obtained in this way are less than those assuming a single circle. Fig. 6 shows the slip surfaces which give the lowest values of the bearing capacity factor in 3 cases. These indicate a tendency of the footing to punch through the upper layer, which would be expected in practice.

I hope that revised values of the bearing capacity factor will be obtained soon for the case when the footing rests on a firm clay lying over a softer one. But, on the other hand, this condition is likely to lead to complications apart from the ultimate bearing capacity, such as settlement from the compressibility of the soft clay. Hence, the more useful practical problem of deciding on the necessary depth needed to obtain a required bearing value, knowing that there is a firm stratum below, should be predicted sufficiently accurately for practical purposes from the present figures.

L'auteur discute la suggestion du Rapporteur d'utiliser pour le calcul de la stabilité dans un sol bicouche cohésif une surface de glissement composée de plusieurs arcs de cercle de rayons différents.

Le Rapporteur général

En ce qui concerne le sujet exposé, la discussion n'est pas close; je crois que dans le cas actuel il faudrait attendre que des essais sur modèles réduits, qui sont faciles d'ailleurs à organiser, montrent exactement quelles sont les conditions modifiées de rupture, lorsque les deux couches qui se succèdent n'ont pas les mêmes caractéristiques de cohésion. Nous remercions néanmoins M. Button de sa très intéressante intervention, mais je ne puis que rappeler ce que j'ai déjà demandé dans mon rapport général, à savoir qu'il est nécessaire dans un cas comme celui-ci de confronter les résultats théoriques et les essais. Je vous rappelle qu'il y a déjà quelques années, M. Frontard avait eu l'idée de faire des essais de modèles réduits avec de la graisse consistante. Ces essais ont été repris par M. Šuklje de Yougoslavie, qui ne peut pas, malheureusement, prendre la parole aujourd'hui, et c'est bien regrettable parce que ces essais étaient extrêmement intéressants dans la voie qui nous est tracée par M. Button. Vous pouvez vous reporter aux Comptes Rendus du Congrès qui comporteront cette discussion, comme je l'ai déjà dit au début de cette séance.

The General Reporter recommends tests on reduced scale models in order to determine the behaviour of the line of rupture in a two-layer cohesive soil, for instance making use of a fat of a certain consistency as proposed by Messrs. Frontard and M. Šuklje.

MM. P. Habib et F. A. Soeiro

La migration de l'eau dans les sols non saturés, sous un gradient de température, a été parfois rendue responsable du gonflement de certains sols ou de l'instabilité des ouvrages. Le mécanisme de cette migration ne trouve pas encore une interprétation unanime (*Maclean et Pamela, 1946; Winterkorn, 1947*). En particulier, la phase dans laquelle le mouvement se produit fait l'objet de diverses théories; nous voudrions apporter une contribution à ce problème précis.

Nous avons effectué l'essai suivant au Laboratoire du Bâtiment et des Travaux Publics: des éprouvettes cylindriques (longueur = 11 cm, section = 25 cm²), ont été préparées avec du limon d'Orly ($LL = 34\%$, $LP = 19\%$) compacté à une teneur en eau convenable. L'eau de malaxage contenait une solution d'iode de potassium dont l'iode était radioactif. Chaque

éprouvette, après moulage, présentait une teneur en eau homogène et une activité radioactive γ constante dans sa masse. Les éprouvettes imperméabilisées extérieurement ont été soumises à un gradient de température. Après huit jours, les répartitions des teneurs en eau et des activités radioactives ont été relevées. L'iode de potassium n'étant pas volatil, la variation de la concentration permet de reconnaître la part du mouvement de l'eau en phase liquide ou en phase vapeur.

A la fin de nos essais, nous avons trouvé une augmentation de teneur en eau aux extrémités froides des échantillons, ce qui est un résultat classique, et une augmentation de l'activité γ vers l'extrémité chaude. Ceci indique un mouvement en phase vapeur du côté chaud (pression de la vapeur d'eau du sol p_1) vers le côté froid (pression de la vapeur $p_2 < p_1$), et, en même temps, un mouvement dans la phase liquide en sens inverse, sauf, ce qui paraît improbable, si les ions iodé se sont déplacés seuls, au milieu des films d'eau immobiles, par exemple sous l'action d'un champ thermoélectrique. Des essais sont en cours pour vérifier ces conclusions. Dans le transfert d'eau, le mouvement en phase vapeur est le plus important, ce qui entraîne l'augmentation de teneur en eau du côté froid.

Si le mouvement a lieu dans la phase liquide, il ne peut être attribué qu'à une différence de succion. On en déduit que la succion S_1 du côté chaud est, ou a été, pendant une partie de l'essai, inférieure en valeur algébrique, à la succion S_2 du côté froid.

A l'état final, l'équilibre peut être dynamique ou statique. L'équilibre dynamique ne peut avoir lieu que si le mouvement dans les deux phases s'effectue en sens inverse. L'existence des deux mouvements est d'ailleurs prévisible par le principe de *Le Chatelier*. L'équilibre sera statique quand le mouvement d'une des phases sera impossible: soit état à phase liquide divisé (faible teneur en eau), soit état saturé. M. Jennings a récemment indiqué la possibilité d'un tel mouvement (*Jennings, Heyman, Wolpert, 1952*).

Les résultats expérimentaux (*Maclean et Pamela, 1946; Soeiro, 1953*), montrent que le transfert d'eau est très petit à ces états limites. Ceci montre que les gradients des potentiels disponibles sont très petits.

En effet, aux basses teneurs en eau, la pression de la vapeur d'eau du sol est peu affectée par les changements de température habituels dans les sols, et est surtout déterminée par la teneur en eau. On a pu arriver à la même conclusion par des considérations théoriques (*Soeiro, 1953*).

Pour les teneurs en eau de saturation on a montré (*Croney et Coleman, 1948*) que les changements de succion du fait des variations de température du sol, sont très petits. Si le sol n'est pas chargé et s'il est saturé en présence d'eau libre, le potentiel est rigoureusement nul à toutes les températures (essais de *Jennings, Heyman et Wolpert, 1952*) et le mouvement d'eau est évidemment nul.

Il nous a paru utile de présenter ces résultats car la connaissance précise du mode de migration de l'eau sous l'influence d'un gradient thermique doit permettre d'apprécier plus exactement l'influence du gradient thermique. En particulier une couche de sable sous une fondation routière n'empêchera pas le déplacement de l'eau sous l'effet thermo-osmotique; au contraire, celui-ci risque d'être aggravé puisque la circulation de retour en phase liquide ne pourra pas se produire.

Références

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Soeiro, F. A. (1953): Mouvement de l'eau dans les sols. Etude de l'effet d'un gradient de température. Rapport interne, Laboratoire du Bâtiment et des Travaux Publics, juin.

Winterkorn, H. F. (1947): Fundamental Similarities Between Electro-osmotic and Thermo-osmotic Phenomena. Proceedings 27th Ann. Meet. of the Highway Res. Board. December.

The authors have carried out tests on unsaturated soil samples with admixture of a radioactive solution of iodine. When the samples are subjected to temperature changes, the moisture content increases on the cooler side, whereas on the warmer side the authors note an increase of the radioactivity. They draw the conclusion that in unsaturated soils during temperature changes, the vapour phase moves towards the cooler side, and the liquid phase towards the warmer side. Finally the conclusions to be drawn from their observations are discussed.

Mr. J. E. Jennings¹⁾ (presented by Mr. Wolpert)

The General Reporter, Mr. Buisson, has stated that I favour the thermo-osmosis theory as an explanation for the migration of moisture towards the foundations of buildings erected on partially saturated soils. This is not the case: all work on this matter in South Africa points to the fact that thermo-osmosis plays a very secondary part in the migration of moisture which takes place after the construction of a building. Experimental data are not yet available to prove what the cause of the migration actually is. At this stage, we are investigating the possibilities of a changed set of forces governing the soil moisture conditions, this change being brought about by the new set of conditions created by the interference by the building, with the conditions of evaporation at the surface of the soil. The field of study is very difficult; it involves the measurement of negative pore water pressures of a high magnitude.

It is important in all heaving foundation problems to distinguish between seasonal desiccation due to present climate, and additional desiccation due to previous climates or, alternatively, due to extraneous water-depleting factors, such as tree-growth. There is evidence in South Africa of desiccation which still persists after the droughts of the last interglacial era; deep desiccation due to recent tree-growth is very common. By far the worst cracked buildings are found on sites where trees have been felled immediately prior to building operations.

It is not a necessary condition for heaving that the soils should all be heavy clays. Many cases exist of heaving on relatively light clay soils. The three factors, depth of profile to water table, degree of desiccation in the profile, and degree of activity of the clay elements must be considered together. The most satisfactory method of classifying the soils is on the Casagrande plasticity chart, but there is no sure guarantee that soils which lie below the *A* line will be non-expansive. In fact they can be expansive if the desiccation and depth of profile factors are correct. The only rule regarding the soil types is that, if they lie above the *A* line, then there is more probability of heaving.

It is important not to consider the "corners-down" cracking as necessarily one of settlement. Invariably it has been

¹⁾ Director, Building Research Institute, Pretoria, Union of South Africa.

found in South Africa that this evidence is merely a demonstration that the corners of the buildings have heaved to smaller extent. So far there has been no evidence of reversal of movements with dry and wet seasons, even on the perimeter of buildings and this may be due to the fact that the observations have not yet been carried over a sufficient number of years. However, the observation of the seasonal effect superimposed on the general heaving curve tends to show that seasonal effects in buildings are of secondary importance, and only of any significance at all after the moisture conditions in the foundation soil have become relatively stable. It is the primary heaving movement that causes the greatest damage in South Africa. Sometimes, with very rapid conditions of moisture migration, severe damage is evident before the building is complete: in other cases the damage may take a very long time to show itself.

The question of impermeable aprons round the buildings has been mentioned by the General Reporter. In South Africa these have not been very successful. The movements which take place are dependent on the profiles which are very variable, and it is common to find large differential heavings due to this cause alone, making surrounding aprons of little value.

Probably the most important factor in heaving conditions is an appreciation of the differences between movements of points established on a covered area, such as a building, and the movements of points in open fields. In the latter case cyclic movements are observed: in the former case the movement is upwards, approaching an asymptote, with a cyclic movement superimposed on the general curve of movement. The two sets of conditions are fundamentally different and one should not attempt to apply the conclusions based on the measurements of points in the field to the probable movements of a building.

L'auteur se défend d'avoir prétendu que l'hypothèse de la thermosmose constitue une explication satisfaisante des déplacements de l'eau sous les bâtiments. La cause de ces déplacements est encore obscure et les études expérimentales difficiles. Il convient de distinguer entre la dessication saisonnière due au climat actuel et les dessications dues à des climats antérieurs ou à des causes extérieures. Dans le cas de l'Afrique du Sud les effets saisonniers sont de peu d'importance. La construction de rideaux d'étanchéité autour des bâtiments a été un échec.

Le Rapporteur général

Les communications très intéressantes de M. Jennings mériteraient un commentaire, malheureusement nous sommes pressés par le temps. Toutefois, je crois qu'il faut absolument éviter que nous fassions des conclusions trop générales du fait d'observations effectuées dans un seul pays. Par exemple, M. Jennings montre que les variations saisonnières n'ont pas une très grande importance en Afrique du Sud. Elles ont une très grosse importance en Afrique du Nord et les papiers publiés en Amérique du Nord montrent également que cette influence saisonnière est très importante. Il ne faut donc faire aucune généralisation, mais étudier le sol avec soin dans chaque cas particulier. En ce qui concerne l'influence de la végétation, je pourrais citer personnellement un exemple: celui d'une église construite, il y a environ 50 ans à Saxe. A côté de cette église se trouve une petite place sur laquelle aucun arbre n'avait été placé jusqu'à il y a quelques années. Il y a trois ans la municipalité a voulu ombrager cette place. Des arbres ont été plantés et l'effet n'a pas tardé à se montrer, l'église est maintenant complètement craquée du côté où les arbres ont été plantés. Il est nécessaire de reprendre en sousœuvre les murs et les colonnes de cette église.

The General Reporter believes that seasonal variations play a more important role than assumed by Mr. Jennings, but the extent of their influence varies in different countries. He gives an example of damage to a building solely due to the presence of nearby trees.

Prof. G. P. Tschebotarioff

Prof. Lorenz (Proceedings 1953, vol. I, p. 406) describes an interesting method of determining the dynamic "bedding value" of a soil system under a heavy vibrator. It is however not clear on what grounds the dynamic "bedding value"—or any other coefficient determined by means of tests with vibrators having a comparatively small contact area with the soil—may be applied to the determination of the vibration characteristics of a machine foundation with a larger contact area with the soil.

It has been shown that the dynamic modulus of soil reaction and also the natural frequency of a foundation are strongly influenced by the size of the contact area with the soil (Tschebotarioff, 1951, pp. 569–572, 586–588, and Tschebotarioff, 1953).

It should also be noted that Fig. 1 of the paper by Prof. Lorenz on which the author bases his calculations, expresses the stress-strain characteristics of a given soil by a single curve. Actually, if by "strain" is meant the settlement, there should be a family of curves for different sizes of contact areas. On the other hand, if by "strain" is meant a unit deformation, valid for all sizes of loaded areas, it would be interesting to learn how it is to be determined in practice since the effect of the size of area on the settlement will vary considerably with the type of soil encountered.

References

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Tschebotarioff, G. P. (1953): Performance Records of Engine Foundations. Symposium on Soil Dynamics, American Society for Testing Materials.

L'auteur prie le Prof. Lorenz de justifier son calcul à l'aide du module instantané (déterminé avec une surface de contact relativement faible) des vibrations du socle d'une machine comportant une grande surface de contact avec le sol. Il demande également des éclaircissements sur la Fig. 1 de l'article du Prof. Lorenz; celle-ci montre une seule courbe pour la relation avec la surface de contact comme paramètre.

Prof. E. Indri

Me référant à la conclusion de l'intéressante étude de MM. Bellier, Frey et Marchand (Comptes Rendus 1953, vol. I, p. 319), qui souhaitent l'extension des mesures sur les barrages, je désire signaler que la Società Adriatica di Elettricità a en cours une série de mesures pour les barrages du Lumiei, Val Gallina et Valle di Cadore (voûte) et de Pieve de Cadore (voûte-poids). Ces mesures portent tant sur les efforts dans les barrages, enregistrés au moyen d'instruments placés dans le béton, que sur les mouvements des barrages et du rocher enregistrés au moyen d'un réseau trigonométrique et de niveling de haute précision.

Les mesures sont encore en cours d'exécution, mais les premiers résultats ont indiqué l'importance des déformations des barrages et du rocher, aussi bien de la fondation que des appuis.

Pour ce qui regarde la différence entre le coefficient d'élasticité du béton et du rocher, les expériences exécutées par l'ISMES (Istituto Studi Modelli e Strutture) pour les barrages

de la S.A.D.E. concordent au moins qualitativement avec les résultats des expériences de M. *Bellier* et les conclusions du rapport général de M. *Buisson* pour la différence de la valeur du coefficient soit pour le rocher en place et en échantillons, soit pour le béton et le rocher.

L'interprétation des mesures est laborieuse dès qu'il s'agit de phénomènes complexes et qui se superposent, mais la S.A.D.E. espère pouvoir communiquer bientôt les résultats de ses études.

The author, referring to the paper by Messrs. *Bellier, Frey and Marchand*, states that a series of measurements are now being taken at different Italian dams. The results up to now obtained show agreement with the conclusions of the above-mentioned paper.

Prof. K. Terzaghi

The Term "conclusion" assigned to my discussion involves a gross exaggeration, because I cannot even attempt to formulate in a ten-minute talk any adequate conclusions concerning the vast array of topics included in the fourth session. I can do not more than comment on what appear to be the most controversial subjects among those which have been dealt with by the General Reporter. One of them is the secondary time effect.

Secondary time effects were described prior to the First International Conference in Cambridge, Mass. So far not much progress has been made in explaining the mechanics of this process, but the number of case records at our disposal has vastly increased. These records disclose a remarkable variety of secondary time effects. In most instances the secondary time effect is unimportant compared to the primary one, but in some regions such as the area covered by Mexico City the opposite relation prevails. The settlement due to secondary time effect, such as that in Mexico City and in the coastal districts of Holland, increases with the logarithm of time but in others it appears to increase in simple proportion to time. The Post Office Building of Bregenz, Vorarlberg, which has been observed for almost half a century and the Gulf Building in Houston, Texas, are examples.

The variety of the phenomena constituting the secondary time effect indicates that these phenomena are produced by at least two independent processes, governed by different laws. One of them is the secondary time effect which follows the primary consolidation of laterally confined soil samples in the laboratory. The secondary compression of such specimens increases approximately with the logarithm of time. If the thickness of the layer or layers of clay responsible for the settlement of a structure is small compared to the horizontal dimensions of the loaded area, the secondary settlement is likely to proceed in accordance with a similar law. It represents the result of grain adjustment under lateral confinement. However, if the clay strata are thick the secondary settlement due to grain adjustment combines with another one due to lateral displacement of the clay, because the application of the load subjects the clay to what can be compared to a squeeze test and the shearing stresses produced by this type of loading are associated with creep, unless they are very low.

The results of laboratory tests which I performed some twenty years ago indicated that the creep of the silty clay which was used for the tests started as soon as the load on the cylindrical clay specimens became equal to about one half of the unconfined compressive strength of the clay. The results of in-situ shear tests which were performed by Vattenbyggnadsbyran in 1929 at the site of the storage dam Swir 3 in U.S.S.R. in 1929 on a highly plastic devonian clay have led to similar

conclusions. For other clays the ratio between creep load and ultimate strength may be different but so far no other experimental data are available. Whatever this ratio may be the rate of creep increases with increasing load and at constant load it appears to be independent of time.

In the field the most conspicuous manifestation of the lateral flow of clay under load is the gradual increase of the distance between ore-retaining walls which has been observed on many ore yards in the Great Lake Region in the United States located above thick beds of glacial clay. On some of these yards this distance has increased in a few decades by amounts up to five feet in spite of the fact that the ore load never exceeded the bearing capacity of the clay strata. The lateral flow of the clay produced by the creep is inevitably associated with a subsidence of the clay surface. Under constant load this subsidence would increase in simple proportion to time, whereas the subsidence due to grain adjustment under lateral confinement increases with the logarithm of time. Hence, the relation between secondary settlement and time can be very different depending on the percentage-participation of the settlement due to creep.

In the field the lateral flow of the clay out of the space located below a heavy structure could be observed by means of a sensitive tiltmeter such as the instrument which has recently been developed by Prof. *Stanley D. Wilson* of Harvard University. By means of such an instrument the gradual lateral displacement of the centerline of a flexible casing inserted into a drillhole can accurately be measured. Unless and until such field observations and corresponding creep tests in the laboratory are performed on very different clays, the secondary time effect will remain a highly controversial subject.

A second controversial topic involves the regional subsidences and their effects on structures and mechanical installations. This topic appeared on the programme but not in the proceedings of the fourth session.

The term regional subsidence indicates the formation of a bowl-shaped depression with a diameter of at least a mile and a depth of at least several feet. It may be due to the cumulative weight of the structures erected in the process of the growth of a city, to the lowering of the water table due to pumping from many wells, to oil production or to a combination of two or all of these causes.

If the changes responsible for a regional subsidence do not produce an increase of the effective stresses in the subsoil by more than a few tons per square foot, no significant regional subsidence occurs unless the subsoil contains layers of highly compressible soil such as beds of soft clay and the trend of the subsidence can fairly reliably be predicted on the basis of the results of laboratory tests. The horizontal displacements associated with the subsidence are inconsequential. The subsidence of Mexico City and of the Santa Clara Valley in California are classical examples.

On the other hand, if the artificial changes in the stress conditions of the subsoil involve an increase of the effective pressures by tens of tons per square foot, important regional subsidences may ensue even when the subsoil consists of layers of relatively incompressible material such as shale or sand. Supplementary pressures of this magnitude can be produced only by large-scale pumping operations such as the extraction of oil from sand strata located at great depth or the withdrawal of large quantities of water from groups of deep wells. On account of the great depth at which the seat of settlement is located and the character of the materials subject to compression a forecast of the subsidence on the basis of the

results of laboratory tests is impracticable and the horizontal displacements associated with the vertical subsidence may be important enough to have adverse effects on industrial establishments. An outstanding example is the subsidence of Terminal Island in Southern California produced by the extraction of oil from depths ranging between about 2,000 and 5,000 feet. The bowl of subsidence has a width of several miles. On the bottom of the bowl are located the steam power plants of the Southern California Edison Co. and over the slopes are scattered various important industrial establishments, harbour installations and a large dry dock. At the present time the maximum subsidence is about 17 feet. Since the ground surface is already located well below high tide level, dikes had to be built and pumping machinery installed.

A few years ago, when I faced the problem of estimating the ultimate maximum subsidence of Terminal Island I intended to take advantage of the fact that the records of the oil companies contained a vast amount of data concerning the void ratio of the oil sands at different depths below the surface. When these data were plotted against the logarithm of the effective overburden pressure it was found that all these points were located in the close proximity of a straight line. However, the value of the compression index corresponding to this line was so high that the increase of the effective pressure associated with the oil extraction should already have produced a subsidence of 300 feet and not of 15 feet provided that the compression index of the oil sands were equal to that obtained from the diagram. The startling discrepancy between the computed and the observed subsidences can only be accounted for by time effects. According to the geological reports the deposition of the oilbearing formation, with a thickness of more than three thousand feet took place at a rate of less than one hundredth of an inch per year whereas the increase of the pressure responsible for the observed subsidence took place within less than twenty years.

Since there is no known relationship between the geological compression index and the index corresponding to a relatively rapid pressure increase, I was compelled to estimate the ultimate settlement by a process of extrapolation from the observed relationships between the measured pressure decline in the oil wells and the corresponding subsidence.

Since the seat of the subsidence of Terminal Island is located at a great depth, the subsidence produces intense compressive strains at the center of the depressed area within the uppermost hundreds of feet whereas in the peripheral part the same strata are subject to tension. As a consequence, at the center of the bowl of subsidence pipe lines buckled whereas at the peripheral parts they were torn apart. By observation it was found that the horizontal strain increased at every point approximately in direct proportion to the vertical subsidence at that point.

Regional subsidences due to the compression of relatively incompressible strata are also produced by pumping from deep wells for the water supply of manufacturing plants which consume large quantities of water. Such conditions prevail for instance at the sites of pulp and paper mills in Southern Texas. The water is drawn from aquifers located at a depth up to 1,500 feet and the piezometric surface is lowered by several hundred feet over areas with a width of more than a mile.

As a result of the pumping operations a bowl of subsidence is formed. The topography of the bowl reflects the shape of the lowered piezometric surfaces for the different aquifers as well as the variations of the average compressibility of the strata affected by the pumping operations. The differential

settlement due to the variations of the compressibility of the strata in horizontal directions can be many times greater than that due to the slope of the lowered piezometric surface, but the cost of a reliable survey of these variations by boring and testing would be prohibitive. It is also impracticable to determine by theory the maximum tilt which can be tolerated at the site of the different units of the manufacturing plant without running the risk of serious interference with the operation of the machinery. Therefore adequate decisions concerning the location of and spacing between the wells can only be made on the basis of reliable case records. Realizing the intrinsic value of such records the owners of several of the large manufacturing plants in southern Texas started several years ago to keep detailed records of the settlement produced within their property lines by the operation of their watersupply systems. The spacing between the reference points is so chosen that the results of the periodic surveys furnish reliable information concerning all the irregularities of the topography of the bowl of subsidence, because the influence of these irregularities on plant operation is much more important than that of the overall dimensions of the bowl of subsidence.

The practical problems associated with regional subsidences are only one of the many examples of the importance of the function of adequate case records in the field of applied soil mechanics.

L'auteur commente tout d'abord le phénomène du tassement secondaire. Ce dernier est soumis à deux lois: lorsque le tassement primaire est suivi, dans l'œdomètre, d'un tassement secondaire, il varie en proportion du logarithme du temps. Ceci est dû à un réajustement des grains.

Une seconde influence est due à l'écoulement plastique et entraîne un mouvement de fluage. La rapidité de ce mouvement de fluage est proportionnelle à la charge et, pour une charge constante, est indépendante du temps.

L'auteur discute ensuite les affaissements régionaux causés par une forte augmentation des charges, par exemple à la suite de l'érection de bâtiments nouveaux, ou de l'extraction d'eau ou de pétrole du sous-sol.

Dans le cas de faibles contraintes (<10 sq.ft.) les affaissements régionaux d'importance ne peuvent se produire que lorsque le sol est très compressible. Dans le cas de charges considérables, par exemple pompage de grandes quantités de liquide du sous-sol, on peut constater des tassements secondaires même dans des sols peu compressibles. Toutes prédictions regardant les tassements sont rendues difficiles du fait que des mouvements latéraux se produisent; il convient alors d'appliquer une échelle géologique du temps.

M. M. Bachelier

Les remarques formulées par M. le Rapporteur général de la Session 4 au sujet de notre communication (Comptes Rendus 1953, vol. I, p. 327) nous conduisent à insister sur les points suivants:

1° Le module de réaction varie avec la profondeur, vraisemblablement d'une manière discontinue. Sous la fondation, le module de réaction a été supposé constant car il aurait été hasardeux d'extrapoler les résultats obtenus à 5 m de profondeur. La fondation a d'ailleurs été établie à 6 m de profondeur parceque l'on avait ainsi l'assurance que la valeur de la pression de rupture était d'au moins 30 kg/cm².

2° L'objet des essais était de déterminer le module de réaction pour les massifs de fondation. Le problème a consisté à passer du module de réaction mesuré avec une plaque de 53 cm de diamètre au module de réaction correspondant à la surface de base des massifs.

Notre seule hypothèse a été de supposer, conformément à la théorie de l'élasticité, que les modules de réaction étaient en raison inverse des diamètres.

(1) The modulus of subgrade reaction varies discontinuously with depth; it was supposed constant underneath the foundation to avoid an extrapolation of the results obtained at 5 m.

(2) The purpose of the tests was to determine the modulus of subgrade reaction of the soil under the caissons from the results of a plate bearing test on a plate 53 cm in diameter. It was supposed—and this was the only assumption—that as required by the theory of elasticity, the modulus of subgrade reaction decreases with increasing diameter.

Dr. L. Bendel

Since 1938 I have used a formula similar to *Buisman's* for the prediction of settlement. This is as follows:

$$S = \int_0^{\infty} K \log \left(\frac{p_0 + \Delta p}{p_0} \right) dh$$

where

S = settlement in mm

K = is the percentage compression-index which has a constant value and is found by increasing the pressure

p = ($p_0 + \Delta p$) from 1 to 10 kg/cm²

p_0 = 1 kg/cm²; for the determination of K , the value of $p_0 + \Delta p = 10$ kg/cm²

dh = the thickness of the stratum in mm.

See vol. III, Session III, p. 143.

The value of K is determined in the laboratory by means of oedometer, normal triaxial or dynamic-triaxial apparatus. It can also be determined in the field in a plate-bearing test, or from a cone penetration test. Some results obtained for the value of K are given below:

(1) Relation between K value and Terzaghi's value

$$K = \frac{\ln 10}{1/a(1+e_0)} = \frac{C_c \ln 10}{1+e_0}$$

e = void ratio

(Proceedings 1953, vol. II, p. 336; *Terzaghi and Peck*, 1948, p. 64).

(2) Relation between K value and cone penetration

$$K = K_a + \sqrt{Kt \log \frac{C'_a + \Delta C}{C_a}}$$

$(C'_a + \Delta C) = C$ = observed penetration of the cone

C_a = penetration of 10 mm

K_t = compression index, if the penetration value ($S_a + \Delta S$) equals 10 C_a

K_a = compression index, if the penetration of the cone is $C_a = 10$ mm and $\Delta C = 0$ mm.

Soil	K_a -value for $S_a = 10$ mm	K_t -value for $(C_a + C) = 10 C_a$ in mm
Sandy layer	3 ÷ 4%	3 ÷ 5%
Mixture of sand, loam and peat	6 ÷ 8%	8 ÷ 10%

(3) Relation between K values and compression tests

We found in oedometer tests the following values for K :

Clay and marl (10–15% water content)

$K = 0.005$ to 0.05 or 0.5 to 5%

Clay and marl, very wet $K = 0.05$ to 0.6 or 5 to 60%

Clay with peat $K = 0.01$ to 0.3 or 1 to 30%

Very fine sand $K = 0.05$ to 0.1 or 5 to 10%

(4) Relation between K values and triaxial tests

Here the K value depends on the magnitude of the lateral pressure.

For instance, for clay:

Lateral pressure	"K" value
0 kg/cm ²	29%
0.2 kg/cm ²	26%
0.4 kg/cm ²	23%
1.0 kg/cm ²	20%

(5) Dynamic K value

If we have a great number of rapid repetitions of loading, for instance increasing from 2 to 20 cycles per second, the settlement increases suddenly; that is at the moment of resonance between the natural frequency of the soil and the exciting frequency. Beyond the resonance range, the K value is generally small, because the compaction of the soil during the resonance period is great. For instance: for a fine sand with 10% loam (0.02 mm) and 12% water content the K value varied as follows:

before resonance phenomena: $K = 4.0\%$

after resonance phenomena: $K = 1.5\%$

(6) The K value as affected by the size of the foundation

If the surface of a plate is great in relation to the load on the plate, the settlement S can be determined by applying the formula (1):

$$S = \int_0^{\infty} K \log \left(\frac{p_0 + \Delta p}{p_0} \right) dh \dots \quad (1)$$

But if the surface of a plate is small in relation to the load, the settlement can be determined with formula (2):

$$S = S_0 \left(\frac{p_0 + \Delta p}{p_0} \right)^{K_0} \quad (2)$$

Conclusion: The improvement of *Buisman's* formula permits the elucidation of different problems and facilitates the settlement calculation if we know the compression index K .

It is possible to determine the compression index K in various ways: e.g. with the penetration cone, the oedometer, and with the normal and dynamic triaxial apparatus under different lateral pressures. The index K depends on the relationship between the size of the plate and the load.

The author has developed a formula for the computation of settlements which is similar to Mr. *Buisman's*; further, he gives examples of its application.

Dr. C. L. Dhawan¹⁾

It has been observed that soils having an identical mechanical analysis show different engineering properties. These differences are mostly due to the nature of the soil colloid, and to the type of the clay mineral. In the arid and semi-arid zones of our country, we have seen that sodium soils are comparatively more resistant to consolidation than calcium soils. But they are more prone towards swelling as compared with calcium soils. In the light of these observations due consideration should be given to this aspect of the question in foundation problems.

The second point which merits attention is the shrinkage effect of clays belonging to the montmorillonite group. In India we have large areas of black cotton soils. The examination of failures of a large number of buildings built on black cotton foundations indicate that the fault is due to shrinkage. The only remedy for keeping such type of soils stable is to maintain the moisture content constant.

This can be accomplished in two ways:

- (i) Placing an apron round the building in order to prevent seasonal changes in the soil beneath foundation;
- (ii) Placing the foundations of the buildings so deep that there is very little variation of the moisture content.

The third point which needs consideration is the great effect of the temperature on the consolidation of soil. As the temperature of the sub-soil varies with depth, the kinematic viscosity of the water also varies, hence due allowance should be made in computing settlement on the basis of consolidation test results.

The bearing capacity also varies with the fluctuations in temperature.

L'auteur insiste sur l'importance des propriétés colloïdales des divers sols. Aux Indes, par exemple, les sols sodiques sont moins compressibles que les sols calciques, mais plus sensibles au gonflement. Il rend attentif au phénomène de retrait observé aux Indes dans les sols dits «black cotton», phénomène qui ne peut être empêché que par le maintien d'une teneur en eau constante. Enfin l'auteur propose de tenir compte de l'influence importante de la température sur la viscosité de l'eau et, par conséquent, sur la vitesse de consolidation des sols.

Prof. D. Haber-Schaim

To determine the influence of the weather on a soil surface, we may examine a part of it and draw conclusions in respect of the whole.

If we construct on this surface a building or a similar body, the homogeneity of the surface as regards the atmospherical influence will be disturbed. Different influences come from all points of the compass. Neither sun nor rain nor wind act "simultaneously" in all directions. The soil swells more quickly on the windward than on the leeward side. All atmospherical influences act simultaneously and unequally all around the building, and this is the reason why swelling and shrinkage are uneven around and beneath the building.

A surface which was even before the action of the weather will be warped by these particular influences. This is sufficient to alter all the conditions of support of the building. Also discontinuities at the corners and phase-shifts are due to these influences. For instance, a direct consequence of this is the settlement of the corners of a building.

¹⁾ Physical Chemist, Irrigation and Power Research Institute, Amritsar, Punjab, India.

It is not easy to estimate the changes in the reaction forces in order to take the necessary precautions, but these may be studied experimentally for one kind of soil at a time. By building a rigid test structure equipped with the necessary apparatus to register simultaneous observations, it would be possible to study the changes in the soil stresses in their relationship to rainfall, temperature and the like around and beneath the structure.

It would be desirable that large laboratories should study these problems and thus provide opportunities for valuable research work.

Dans le but de déterminer l'influence complexe du climat (insolation, pluie, vent) sur les contraintes et les déformations du sol au voisinage de bâtiments, l'auteur propose d'ériger une construction rigide d'essai munie des installations d'enregistrement nécessaires.

M. G. Josselin de Jong²⁾

En étudiant l'article intéressant de M. Mandel (Comptes Rendus 1953, vol. I, p. 413) qui traite d'une façon mathématique et rigoureuse la consolidation de couches d'argile saturée sous l'influence de chargements extérieurs, il nous a paru dommage que dans cette analyse il n'ait pas été possible de satisfaire toutes les conditions aux limites qu'on voudrait y imposer.

Nous voulons proposer cependant de séparer les influences de dilatation du massif (engendrant des pressions d'eau interstitielle) et de rotation (n'affectant pas ces pressions) tout comme sont traités les problèmes de vibrations.

On peut alors introduire des fonctions additives et satisfaire n'importe quelle condition aux limites, non seulement concernant les contraintes (ou les déplacements) des grains, mais en outre toute étanchéité ou perméabilité de surface influençant les pressions d'eau interstitielle.

Un exemple de cette conception sera donné dans un article qui est en cours de préparation et que nous avons l'intention de publier sous peu.

The author regrets that Mr. Mandel in his interesting paper on the consolidation of saturated clay layers under external loads (Proceedings 1953, vol. I, p. 413) has not taken into consideration all the threshold conditions. Some of the effects due to dilatation and the surface permeability will be the subject of an article to be published in the near future.

Dr J. Kerisel et Prof. A. Caquot

Nous sommes en accord avec les observations de M. Jorgen Brinch Hansen relatives à son calcul.

Dans notre «Traité de mécanique des sols», nous avons établi la force portante d'un terrain au niveau de la surface horizontale, lorsque la plaque d'appui est *lisse*. C'est ainsi qu'à la page 276 de notre «Traité de mécanique des sols», nous indiquons pour les diverses valeurs de l'angle de frottement φ le coefficient caractéristique S_1 . Pour $\varphi = 30^\circ$ nous obtenons 7.391.

Nous avons d'autre part soumis au Congrès une table pour surface rugueuse disposée en dièdre d'angle $\pi/4 - \varphi/2$ avec le plan de symétrie vertical. Le coefficient pour $\varphi = 30^\circ$ est alors 22.69. Mais la table n'est valable que si la surface chargée est liée à un tel dièdre rugueux. Le coefficient varie du reste avec l'angle du dièdre lié à la surface. Nos tables du «Traité de mécanique des sols» déterminent 48.51 pour un angle dièdre deux fois plus faible que le précédent, avec le même angle de

²⁾ Lab. voor Grondmechanica Delft, Pays-Bas.

frottement. Réciproquement, le coefficient diminue si l'angle du dièdre augmente. Quand on aboutit ainsi à la simple surface de contact horizontale le coefficient est très inférieur à 22.69 pour $\varphi = 30^\circ$.

La présence du plan de symétrie détermine une profonde modification au régime qui s'établirait en système indéfini d'après les caractéristiques des équations aux dérivées partielles. L'interférence des deux systèmes symétriques diminue la capacité de résistance de la plaque et nécessite une étude spéciale très poussée. L'expérience montre souvent la formation d'un dièdre non conforme aux données de cette interférence.

Mais il ne faut pas oublier que dans ces essais, l'angle de frottement n'est pas constant, la compacité augmentant avec la charge et déterminant une amélioration du frottement variable dans la masse. Il s'agit alors d'un autre problème.

The author agrees with the remarks made by Mr. Brinch Hansen (Proceedings 1953, vol. II, p. 170) regarding his own paper. He draws attention to the importance of the rugosity of the loaded surface; the tables in his paper are valid only for a rough surface at a dihedral angle $\pi/4 - \varphi/2$ being bounded by the loaded surface. The author also gives some numerical examples for $\varphi = 30^\circ$.

Prof. H. Lorenz

Prof. Tschebotarioff (Proceedings 1953, vol. III, p. 157) believes that the influence of the contact area should be expressed in the stress-strain diagram of the soil. He refers to his papers (Tschebotarioff, 1951, 1953), where he shows that reduced natural frequencies decrease with increasing area.

Quite the contrary has been obtained by Degebo tests. Table 1 (see Lorenz, 1950) contains results of forced vibration tests carried out with a vibrator which is set up so that its weight decreases with its surface area. Thus always the same static pressure of 0.27 kg/cm^2 was in action. Although the baseplate area ranged only from 0.25 to 1 m^2 , it seems to me that these tests give better information on the influence of the area than the tables and diagrams, published by Tschebotarioff. Neither Table 1 nor Fig. 1 (Tschebotarioff, 1953) can be quoted completely because different types of exciting forces, different foundation types and different methods of determination are assembled. All tests with other than purely vertical exciting forces, all tests on pile foundations and all results from other than forced vibration tests have to be eliminated. Then only cases 12 and 13 (Newcomb's Records) remain applicable. As the Degebo tests, compiled in Table 1 of my paper (Lorenz, 1950) eliminate all other influences but the area, it seems proved that the natural frequency for a constant static soil pressure increases with increasing baseplate area.

It would not be correct to conclude from the Degebo tests quoted that increasing the contact area always increases the natural frequency, because I have shown in my paper (Proceedings 1953, vol. I, p. 406), that not the static pressure alone, but both static and dynamic pressures determine the true "bedding-value" or any similar dynamic coefficient. The increase of natural frequency due to increasing area is brought about by the reduction of total stress, because only the static pressure is held constant during the test series whereas the total pressure decreases with the increasing area when the exciting forces remain constant.

Tschebotarioff's opinion that a family of curves should replace the stress-strain diagram shown in my paper (Proceedings 1953, vol. I, p. 406, Fig. 3), seems to be influenced by his knowledge of the static "bedding-value", which of course is

highly dependent on the contact area, but the static "bedding-value" has only name and dimension in common with the dynamic one. The dynamic bedding value does not deal with plastic settlements and depends only on the elastic behaviour of the soil. As in dynamic problems the soil certainly does not act as part of an oscillating mass, it seems also logical that the stress-strain diagram should be independent of the surface area.

References

- Lorenz, H. (1950): Der Baugrund als Federung und Dämpfung schwingender Körper. Bauingenieur 25, Heft 10, S. 365ff.
Tschebotarioff, G. P. (1951): Soil Mechanics, Foundations and Earth Structures. McGraw-Hill Book Co.
Tschebotarioff, G. P. (1953): Performance Records of Engine Foundations. Symposium on Soil Dynamics, American Society for Testing Materials.

A l'opposé du Prof. Tschebotarioff l'auteur a observé que la fréquence naturelle des vibrations diminue en fonction de l'augmentation de la surface de contact. Il insiste sur la différence entre le module des réactions statique et dynamique. L'auteur est d'avis que la relation pression/déformation dans les problèmes dynamiques est indépendante de la surface de contact du fait que dans ces problèmes le sol ne saurait être considéré comme partie de la masse oscillante.

M. J. Mandel

J'ai lu avec beaucoup d'intérêt la remarquable communication de MM. Lundgren et Mortensen (Comptes Rendus 1953, vol. I, p. 409). Dans le cadre de l'hypothèse classique de Coulomb (angle de frottement interne $\varphi = \text{constante bien déterminée}$) la solution donnée par ces auteurs me paraît complète et irréprochable. En particulier, dans le cas d'une fondation rugueuse, le contour du coin qui s'enfonce sans se déformer se trouve déterminé d'une manière rigoureuse à partir des conditions à la limite imposées.

En supposant ce contour formé de deux droites inclinées de $\pi/4 - \varphi/2$ sur la verticale, on n'obtient pas au contraire une solution entièrement satisfaisante, puisque, d'après le théorème de Bonneau (cf. Ravizé, Poussée des terres, p. 14) les contraintes le long de ces droites devraient obéir aux équations de Kötter ce qui n'est pas le cas.

Analysons cette objection; les équations de Kötter n'étant pas satisfaites, il en résulte qu'à l'intérieur du coin, sur les éléments d'arcs parallèles à son contour et infiniment voisins de celui-ci, on trouve des contraintes faisant avec la normale un angle supérieur à φ : c'est précisément de cette impossibilité, signalée par M. Lundgren dans sa discussion que découle le théorème de Bonneau.

Ceci dit, l'hypothèse classique est une hypothèse simplificatrice. En réalité, en raison du tassement du sable sous la fondation, l'angle φ dans la région non déformée me paraît devoir être plus élevé que dans la région qui se déforme. Si ceci est exact, d'une part l'objection précédente disparaît, d'autre part la valeur calculée par MM. Lundgren et Mortensen pour le facteur N_y doit être inférieur à la valeur réelle de ce facteur.

The author remarks that Messrs. Lundgren and Mortensen's theory is a direct consequence of Coulomb's law with constant φ ; further the authors' analyses by means of a rectilinear corner leads to a contradiction if we assume φ to be constant. It seems plausible that the value of φ in the stressed zone is lower than in the unstressed zone which leads him to think that the N_y -value indicated by Messrs. Lundgren and Mortensen is lower than the true value.

M. J. Mandel

La discussion de M. *de Josselin de Jong* me donne l'occasion d'indiquer que, depuis la rédaction de ma communication (Comptes Rendus 1953, vol. I, p. 413), j'ai réussi à traiter de façon tout à fait rigoureuse le problème mathématique proposé en tenant compte, non pas de conditions à la limite simplifiées, mais des conditions à la limite exactes. Ce travail, qui répond au désir exprimé par M. *Josselin de Jong*, fera l'objet d'une publication, lorsque seront achevés les calculs numériques nécessaires pour la mise des formules en abaque.

With reference to the remarks made by Mr. *Josselin de Jong* on his paper (Proceedings 1953, vol. I, p. 413), the author states that he has succeeded in treating the problem with rigorous accuracy, i.e. taking into consideration the exact threshold value. Results will be published in the near future.

Dr. G. G. Meyerhof¹⁾

I have read with interest the paper by Prof. *Lundgren* and Mr. *Mortensen* (Proceedings 1953, vol. I, p. 409) and agree with M. *Buisson*'s comments that without full details and experimental evidence it is difficult to express an opinion on the authors' analysis.

Both Prof. *Caquot* and Dr. *Kerisel*'s Paper (Proceedings 1953, vol. I, p. 336) and my extension (1951) of Prof. *Prandtl* and Dr. *Ohde*'s analysis agree for all practical purposes. Moreover, extensive laboratory experiments and the limited field data available (Meyerhof, 1948, 1950, and 1951) with footings on sand indicate that the observed bearing capacity and the mechanism and extent of failure are in reasonable agreement with these methods of analysis. A similar agreement exists when the same general procedure is applied to cohesive materials and compared with test results (1951), as well as in the case of eccentric and inclined loads on both sands and clays (Meyerhof, 1953).

Until further evidence is presented by the authors it may therefore be concluded that the present methods of estimating the bearing capacity of foundations are adequate for all practical purposes.

References

- Meyerhof, G. G. (1948): An Investigation of the Bearing Capacity of Shallow Footings on Dry Sand. Proc. Second Int. Conf. Soil Mechanics, vol. 1, p. 237.
Meyerhof, G. G. (1950): The Bearing Capacity of Sand. Ph.D. (Eng.) Thesis, University of London.
Meyerhof, G. G. (1953): The Bearing Capacity of Foundations under Eccentric and Inclined Load. Proc. Third Int. Conf. Soil Mechanics, vol. 1, p. 440.

L'auteur traite de l'analyse de MM. *Lundgren* et *Mortensen* sur la force portante d'empattements sur sable par la théorie de la plasticité. Il est d'accord avec le rapporteur général qu'il faut attendre les résultats d'essais sur place pour savoir si cette nouvelle théorie est en meilleur accord avec l'expérience que celle de MM. *Caquot*, *Kerisel* et *Meyerhof*.

Dr. G. G. Meyerhof¹⁾

In reply to Prof. *Schultze* the author would like to point out that for shallow foundations (depth/width less than 1) the earth pressure coefficient K_s has no appreciable effect on the bearing

capacity, as shown by the author elsewhere (Meyerhof, 1952). The new bearing capacity factors can therefore readily be applied in such cases without introducing K_s , which is facilitated by the graphical presentation of the results given in the paper.

The theoretical rapid decrease of the bearing capacity with greater inclination of the load is fully supported by the author's loading tests on surface footings. Subsequent tests by the author using footings at a depth equal to the width and using simple portal frames (Meyerhof, 1953) were also found to be in agreement with the theory. When analysing the stability of foundations in practice using the correct shearing strength of soils, it will be found that the proposed theory gives a rational estimate of the bearing capacity of foundations under eccentric and inclined loads.

References

- Letter G. G. Meyerhof (1952): to Géotechnique, vol. 3, p. 93.
Meyerhof, G. G. (1953): Some Recent Foundation Research and its Application to Design. Structural Engineer, vol. 31, p. 151.

En réponse à M. *Schultze* l'auteur expose que, pour des fondations de faible profondeur, le coefficient K_s de la poussée des terres n'exerce pas une influence considérable sur la capacité portante, et par conséquent peut être négligé.

La rapide diminution de la portance en fonction de l'inclinaison de la charge a été confirmée dans des essais. Dans la pratique la théorie est applicable pour autant qu'on introduit dans le calcul une valeur correcte représentant la résistance au cisaillement.

Mr. M. Mikasa²⁾

Owing to the restricted space at my disposal I could not explain thoroughly the new calculation method introduced in my paper (Proceedings 1953, vol. I, p. 436). In reply to the General Reporter's remark on this point, our fellow researcher Mr. *M. Mikasa* who has put forward the new theory intends to give here a brief explanation of the method (*H. Matuo*).

The Basic Idea of the New Theory

I will confine the present discussion to the case of a clayey foundation directly loaded on its surface. In calculating the settlement speed of such foundation we attribute the cause of the settlement entirely to consolidation except in the case where either an abrupt rupture or a slow plastic flow of the soil takes place. I consider this method unsuitable for the present problem, and intend here to propose another method of calculation, the basic idea of which is as follows:

(1) The settlement is divided into two parts, one due to the deviatoric shearing deformation of the soil structure, S_d , and

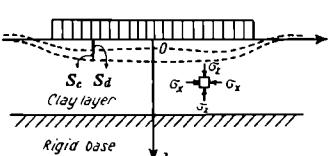


Fig. 7 Division of Settlement into two Parts: S_d and S_c
Répartition du tassement en deux parties: S_d et S_c

the other S_d due to the compression or consolidation of the soil, S_c . The former which is omitted in the normal calculation is in most cases as important as the latter (Fig. 7).

¹⁾ Lecturer at the Oosaka Municipal University, Nisioogimat, Kita-ku, Oosaka, Japan.

²⁾ Foundation Company of Canada, Montreal, Que., Canada.

(2) Generally S_d does not take place at the moment of loading, as is usually thought, but gradually increases with time. Though it sometimes seems as if the whole settlement takes place according to the consolidation theory, S_d follows its own law independent of the consolidation phenomenon.

(3) We have not yet enough information about the law governing the speed of S_d . But it is probable that in most cases it takes place in the early stage of the settlement process.

The Calculation Formulae

Starting from the above considerations I derived (*Mikasa, 1951*) the following formulae to calculate the values of S_d (in my previous paper, S_s) and S_c , assuming that (1) the clay particles and water are incompressible, (2) the clay structure is isotropic and elastic and obeys *Hooke's law*, and (3) the clay is saturated with water. Some ideas as to the validity of these assumptions will be submitted later.

(1) Using the condition of no volume change, the initial excess hydrostatic pressure is proved to equal the additional mean pressure at that point, i.e.

$$u_{t=0} = \frac{\sigma_x + \sigma_y + \sigma_z}{3} \quad (1)$$

where $\sigma_x, \sigma_y, \sigma_z$ are the initial normal stresses caused by the surface load in the direction of the X, Y, Z axes, Z axis being vertical.

(2) Thus the initial effective normal stresses are:

$$\begin{aligned} \bar{\sigma}_x &= \sigma_x - u = \frac{2}{3} \{ \sigma_x - \frac{1}{2} (\sigma_y + \sigma_z) \} \\ \bar{\sigma}_y &= \sigma_y - u = \frac{2}{3} \{ \sigma_y - \frac{1}{2} (\sigma_z + \sigma_x) \} \\ \bar{\sigma}_z &= \sigma_z - u = \frac{2}{3} \{ \sigma_z - \frac{1}{2} (\sigma_x + \sigma_y) \} \end{aligned} \quad (2)$$

(3) Under these stresses the clay structure will deform without any volume change as follows:

$$\begin{aligned} \varepsilon_x &= \frac{2(1+\mu)}{3E} \{ \sigma_x - \frac{1}{2} (\sigma_y + \sigma_z) \} \\ \varepsilon_y &= \frac{2(1+\mu)}{3E} \{ \sigma_y - \frac{1}{2} (\sigma_z + \sigma_x) \} \\ \varepsilon_z &= \frac{2(1+\mu)}{3E} \{ \sigma_z - \frac{1}{2} (\sigma_x + \sigma_y) \} \end{aligned} \quad (3)$$

where E, μ are *Young's modulus* and *Poisson's ratio* of the clay structure respectively.

(4) From equations (3) we find that

$$E_0 = \frac{3E}{2(1+\mu)} \quad (4)$$

$$\mu_0 = \frac{1}{2}$$

where E_0, μ_0 are the initial *Young's modulus* and *Poisson's ratio* of the clay as a whole.

(5) Thus the surface settlement due to such a deformation amounts to

$$S_d = \int \varepsilon_z dz = \frac{2}{3} \frac{1+\mu}{E} \{ \sigma_z - \frac{1}{2} (\sigma_x + \sigma_y) \} dz \quad (5)$$

In the case of homogeneous foundation, S_d can be calculated directly by the elastic theory, the elastic constants being given in (4).

(6) The final settlement is given by

$$S_f = \int \frac{1}{E} \{ \sigma'_z - \mu(\sigma'_x + \sigma'_y) \} dz \quad (6)$$

where $\sigma'_x, \sigma'_y, \sigma'_z$ are the final stresses.

(7) The settlement due to consolidation

$$S_c = S_f - S_d \dots \dots \dots \quad (7)$$

cannot be reduced generally to a simple form, but

(8) in the case where $\mu = 0$ we obtain the following formula

$$S_c = \int \frac{u_{t=0}}{E} dz \quad (8)$$

and can apply in the calculation of settlement speed, the ordinary method based upon the consolidation theory, using the initial excess hydrostatic pressure given in (1).

Some Considerations and Suggestions Relating to the Actual Computation

(1) On the calculation of S_d and S_c

(a) The elastic constants of the clay structure E, μ should be found from the consolidation test of the triaxial compression test that allows for the complete drainage of the excess water. The undrained test will give the constants for the clay as a whole.

In addition to that the time effect may cause considerable variation in the results. I recommend the use of E , obtained from the consolidation test for the calculation of both S_c and S_d , assuming that $\mu = 0$, because this procedure seems to give comparatively reliable results in spite of its extreme simplification. Thus we obtain

$$E = \frac{1}{m_v}, \mu = 0 \quad (9)$$

where m_v is the coefficient of volume decrease of the clay structure.

Actually m_v is not a constant, but a function of the consolidation stress. We use its mean value or the secant modulus in the ordinary settlement calculation. Whereas in calculating S_d , we should use the tangent modulus at the preloading stress, if the clay is a normally consolidated one, because S_d is likely to occur before consolidation has reached a measurable value. (In the case of over-consolidated clay, further research must be done.) This will give a fairly high value of S_d ; the final settlement also increasing. This phenomenon is observed in soft isotropic foundations. But as a rule the foundation is to

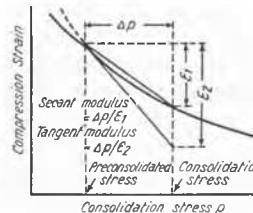


Fig. 8 Compression Strain vs. Consolidation Stress
Effort de compression en fonction de la pression de consolidation

some extent anisotropic due to its stratification and thus the occurrence of S_d is prevented. In such cases I think it advisable to use the secant modulus also for the calculation of S_d for the purpose of neutralizing the effect of anisotropy to some extent (Fig. 8).

Further research must be done for the assumption $\mu = 0$ but the actual value of μ is likely to be rather small. If so, the error is not serious compared to the experimental one.

Needless to say in the case of an unhomogeneous foundation E should be determined as a function of location.

(b) For the stresses $\sigma_x, \sigma_y, \sigma_z$ caused by the surface load, it is considered satisfactory (*Terzaghi, 1936*) to apply *Boussinesq's* solutions for the isotropic homogeneous semi-infinite elastic body. The initial stresses in (1) and (5) should be calculated

with Poisson's ratio 1/2, while the final stresses should be calculated with Poisson's ratio for the clay structure itself (μ). But if we assume that $\mu = 0$, σ'_x and σ'_y do not affect the value of the final settlement, and σ'_z is always equal to σ_z . Therefore we need not calculate the final stresses.

(c) The distribution of S_d and S_f in x, y direction under the flexible circular load is well illustrated in Terzaghi's figures (1948, p. 426). We must be aware, however, that all the curves are drawn for the same Young's modulus, while in our problem the Young's modulus varies during the consolidation process in accordance with the variation of Poisson's ratio, the rigidity remaining the same.

(d) As far as the value of the initial excess hydrostatic pressure in the actual clay is concerned I cannot agree with Dr. A. W. Skempton's theory (1948). As this matter can only be fully discussed at considerable length, I will confine the present discussion to my personal views (cf. discussion to J. Mandel's theory).

In short the initial excess hydrostatic pressure is divided into two components, one that is due to variation in the mean pressure: u_p , and the other that is due to the variation in the deviatoric (shearing) stress: u_d . u_p is equal to the additional mean pressure itself, and is given in (1). u_d is developed by a property of the clay structure called "dilatancy", that shearing stress affects the volume strain. We may consider two sorts of dilatancy in the clay structure. One is due to anisotropy, and the other to disturbance of the structure.

For the value of u under anisotropy-dilatancy, the following formula is easily derived:

$$u (= u_p + u_d) = \frac{\sigma_x + \sigma_y + n\sigma_z}{2 + n} \quad \dots \quad (10)$$

where n is the ratio of the modulus of elasticity in X, Y direction to that in Z direction.

For the value of u due to disturbance-dilatancy, there is no likelihood that there exists a law applicable in every case. But according to the experimental results already published (Rendulic, 1937; Taylor, 1948; Bishop and Henkel, 1953), the fraction u_d is likely to become a small value of the second order, when the shearing stress is small. If so, (1) can be used under small stresses as the first approximation.

If the accurate value of u under the given condition is known, we should calculate the value of S_c from (8) using $u = u_p + u_d$, and the value of S_d in two ways: (a) when the dilatancy is due to anisotropy, from $S_d = S_f - S_c$, where S_f is to be found from (6), (b) when the dilatancy is due to disturbance, from (5).

When dilatancy is due to disturbance, the recovery of the disturbance during the consolidation process may decrease the fraction of S_c due to u_d . This point is beyond our present knowledge.

(2) On the estimation of the settlement speed

(a) As to the manner in which S_d proceeds, we have not enough information yet. Thus we have no alternative at present but to draw the settlement curve assuming that S_d takes place at the moment of loading. The difference between the curve and the actual one indicates how the occurrence of S_d is retarded (cf. Proceedings 1953, vol. I, p. 438).

(b) As to the manner in which S_c proceeds, we must consider the three-dimensional consolidation. This is a difficult problem even with the simplest assumptions adopted before, especially when the foundation is not homogeneous. In addition to that there are in the case under consideration such difficulties as

were pointed out by Terzaghi (1948, p. 295). Thus we have no reliable method for working out the three-dimensional consolidation. Fortunately the effects of the three-dimensional water flow and of the three-dimensional shrinkage on the settlement speed are opposed. I have shown (Mikasa, 1951) the value of the coefficient of volume decrease m_v in the three-dimensional consolidation is $2(1 - \mu)$ times greater than in the one-dimensional case for an isotropic homogeneous semi-infinite elastic body. This makes the settlement slower and neutralizes to some extent the effect of the three-dimensional water flow. The conditions are considered the same for the clay under investigation. Therefore I think that the adoption of the ordinary one-dimensional consolidation theory in calculating the process of S_c will not lead to serious errors.

Discussion on the Traditional Opinions on the Problem

I should like to discuss briefly the traditional opinions on the settlement of a clayey foundation, though I fear that my words may be considered as lacking in respect to the authorities to whom I owe much.

It seems to me that no definite opinion prevails on the settlement due to the shearing deformation of the soil, and the general conceptions on this point are vague. For example M. A. Biot, in his paper (1941) about three-dimensional consolidation, asserts that the cause of the settlement of clayey foundation is the "consolidation", when he might just as well have deduced the different conclusion from his theory which is based on almost the same assumptions as mine. An author writes in his book that the clay has a high compressibility and a low deformability at the same time without any further explanation. All such opinions supporting the traditional calculation are based on the fact that the foundation seldom subsides at the moment of loading, but in most cases settles gradually in the course of time. My interpretation of the phenomenon is stated at the beginning of this paper. But almost everybody considers that the settlement due to shearing deformation should take place at the moment of loading, and attributes the gradual settlement solely to the consolidation phenomenon.

A. Casagrande (1942) explained the small initial settlement by the anisotropy of the clay structure due to its stratification. The anisotropy may exist to some extent, but it seems insufficient to explain the small initial settlement if we consider that S_c occurs instantaneously. We cannot help considering the retardation of S_d , and then the initial small settlement does not account for the smallness of S_d .

J. Mandel's theory (1948) is based on the same conception as Skempton's theory on pore pressure (1948). The author states that the clay is not deformed so much under an anisotropic stress, because it can expand only very little in the direction of the minor principal stress owing to its low expansibility, and this justifies the opinion that S_d is very small. This argument, I think, confuses "extension" in shearing deformation with "expansion", i.e. the inverse process of compression.

K. Terzaghi and O. Fröhlich explained (1936) the small initial settlement by the natural hard soil structure, and recommended the use of the one-dimensional calculation in such cases by introducing the notion of "relaxation in the consolidation region". But it seems to me more reasonable to consider the "relaxation under shearing stress", which leads directly to the idea stated at the beginning of this paper.

May I add a few remarks on the method tried by L. F. Cooling (1948) and perhaps by many others, because it is a method that is likely to occur to anyone who considers the

problem, and rejected because it is not fully understood. Its aim is to ascertain the amount of the initial settlement by means of the elastic theory using the elastic constants found in the unconfined compression test or the triaxial compression test. This method is in principle the same as mine, only if "the initial settlement" is replaced by "the settlement due to deformation", and subtracted from the amount of the settlement due to consolidation.

If it had been possible to obtain perfectly undisturbed samples, this method would have been successful, because then the natural hard structure or the anisotropy in the field would have been preserved also in the laboratory, and a clay specimen would have deformed at the same speed as in the field under the same stress. But the observed settlement took place always much slower than expected considering the laboratory test results, and led the researchers to the conclusion that in the field the settlement due to deformation was negligibly small. This discrepancy between laboratory test and field observation means that the so-called "undisturbed samples" had suffered a certain amount of disturbance. It cannot be explained by anything else. It is clearly a mistake to attribute the discrepancy to anisotropy in the natural foundation (*Casagrande*, 1942). Therefore, so long as we allow for the clay in the field the compressibility obtained in the consolidation test, we must attribute to it also a deformability of the same order though it may be indeformable in a duration as short as in the laboratory test. (Of course in case of a severe anisotropy, this is not correct.) This deformability should be found in a triaxial test of the same duration as the consolidation test, or, as I have already recommended, in the consolidation test itself.

If the disturbance affects the deformation speed of the clay so much, it may affect the ultimate elastic constants as well. On this point, however, the settlement data published hitherto seem to remove our doubts. But it is certain that so far no perfect reliability data has been attained at present. Therefore, it can never be claimed that the new method always agrees better with the actual settlement process than the ordinary one. However, if we intend to inquire into the truth of the matter, we must stop having recourse to ambiguous methods relying on accidental coincidence.

Calculation for an artificial islet

A settlement calculation for an artificial islet was carried out by the members of the Mitsui Colliery Co. under the direction of *S. Morita*. They adopted the calculation method summarized in this paper. The effects of dilatancy, three-dimensional consolidation, and retardation of S_d were abridged because no accurate information had been gained. But fortunately the result so far is satisfactory enough. It may be of much interest to report the whole process of calculation, which involves the application of several further ingenious devices for problems such as the consolidation of multi-layered foundation, the effect of tidal change, etc. But these data have not yet been published owing to their immense volume.

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En complément à l'article 4,23 (Comptes Rendus 1953, vol. I, p. 436) l'auteur expose les éléments de sa théorie nouvelle pour le calcul des tassements. Il considère deux composantes des tassements dans une argile isotrope et saturée: une part S_d causée par les déformations dues au cisaillement du sol, et une autre S_c causée par la consolidation et la compressibilité du sol; cette dernière correspondant au calcul classique.

Mr. R. S. Pulido y Morales

In almost all parts of the Island of Cuba there are found clays which, although firm enough to bear the loads imposed by houses of brick, stone or precast blocks, are subject to considerable seasonal movements. The annual alternation of rainy and dry periods causes swelling and shrinking of the clay to a depth of 6 feet below the surface.

Drying or dehydration of the soil colloids causes a shrinkage of the soil mass; conversely a rapid absorption of water causes unequal swelling. Both horizontal and vertical ground movements are caused by these volume changes. Many clays of this island show an average volumetric change of more than 135 per cent.

These climatic movements are considerably reduced in paved urban areas; damage is generally found only in suburban and rural areas. A close examination of the building movements in these cases generally established that the cracks occur, or open and widen, during the dry period, and that they partially close during the rainy period.

The building mentioned by Prof. *Tschebotarioff* (Proceedings 1953, vol. I, p. 473) was constructed during the second World War; at that time buildings were not so well constructed. Many other houses built later are in perfect condition. The occurrence of shrinkable clays is common in tropical countries. The cracks occur in badly constructed houses with very poor foundations as mentioned by the author.

L'auteur donne quelques indications sur l'effet des variations saisonnières à Cuba. Elles provoquent un assèchement (accompagné de retrait) et un gonflement des argiles jusqu'à une profondeur de 1,9 m. Dans les régions urbaines pavées ces variations sont très atténueées. L'exemple cité par le Prof. *Tschebotarioff* concerne un bâtiment très mal construit.

M. E. Robert

Nous avons calculé par les méthodes de l'équilibre limite introduites par M. *Caquot* la contrainte qui sous une obliquité α détermine la rupture d'un sol de fondation lorsque la base d'appui et la surface libre présentent des inclinaisons i et i' (Fig. 9).

En terrain pulvérulent la valeur en est

$$q = \bar{\omega} h \cos i' \frac{(\cos \alpha + \sin \varphi \cos \gamma) e^{2s \operatorname{tg} \varphi}}{\cos i' - \sin \varphi \cdot \cos \gamma'}$$

avec

$$\sin \gamma = \frac{\sin |a|}{\sin \varphi} \quad \sin \gamma' = \frac{\sin |i'|}{\sin \varphi} \quad s = \frac{\pi}{2} - (i + i' + \omega + \omega')$$

2ω = angle de valeur absolue $\gamma + |a|$ et de signe contraire à a

$2\omega'$ = angle de valeur absolue $\gamma' - |i'|$ et du signe de i'

$\bar{\omega}$ = poids spécifique du terrain

h = profondeur d'enfoncement du massif.

Cette formule ne tient pas compte du poids propre du terrain dans le dièdre BAB' . Elle est approchée, mais sa validité théorique est bonne pour les fondations étroites.

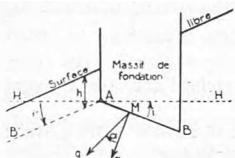


Fig. 9

Calcul de la force portante pour les charges obliques en terrain pulvérulent
Computation of the Bearing Capacity for Oblique Loading in Non-Cohesive Soils

Nous avons donné des tables numériques qui facilitent les applications numériques.

Ces dernières montrent l'importance des modifications apportées à la contrainte de rupture lorsqu'on fait état des variables α , i et i' . Elles peuvent être grandes et ne sont pas en tous cas négligeables. Ainsi en terrain sableux ($\varphi = 40^\circ$) les valeurs $\alpha = 5^\circ$

$$i' = 20^\circ$$

$$i = 10^\circ$$

divisent par quatre la contrainte de rupture calculée en supposant la contrainte orthogonale à une base d'appui horizontale et dans un terrain horizontal.

En terrain cohérent on n'aboutit pas à une expression rigoureuse sous forme explicite de la contrainte de rupture.

Nous donnons une valeur approchée.

Notre étude a été appliquée à la fondation sur des enrochements immersés et nous avons calculé les pressions de rupture en fonction des talus, de la risberme au niveau d'appui et de l'obliquité.

Ces valeurs sont compatibles avec des observations faites en 1933 à St-Malo sur des tronçons en cours de construction pour le prolongement du Môle des Noires. Elles ont servi d'éléments d'appréciation pour des fondations dans des ports français.

Référence

Robert, E. (1948): Annales des Ponts et Chaussées, juillet-août 1948.
Généralisation de la formule de Caquot pour le calcul des fondations.

The author indicates a generalisation of Caquot's method for calculating the bearing capacity of non-cohesive soils under inclined loads. He gives also approached values for cohesive soils. The values obtained by calculation agree fairly well with the results of observations made at St-Malo.

Messrs. M. Rocha, J. Laginha Serafim and A. F. Silveira¹⁾

The results obtained in recent years from the observation of concrete dams as well as from model tests show to how large an extent the behaviour of these structures can be affected by the deformability of the foundation rock. In a like manner,

the results of the structural analysis of dams by the "trial-load" method and by methods based on the theory of shells (Tölke and others) show that if a value is arbitrarily assigned to the modulus of elasticity of the foundation rock, this may lead to considerable deviations in the distribution of loads between arches and cantilevers as well as to significant differences in the shape of the load diagrams for the arches. In the case of badly cracked hard rocks, the problem becomes particularly important. As a matter of fact, the deformation of the rocky masses may be large, even though laboratory tests on rock specimens may lead to high values of the modulus of elasticity; in addition, both laboratory and field measurements show that the modulus of elasticity is likely to vary within a very wide range, even for rocky masses of homogeneous external appearance and identical petrographic structure. The contribution presented by the authors is, therefore, a most valuable aid in dam studies.

Apart from the laboratory tests and field tests in tunnels (see Discussion p. 145) the Laboratório Nacional de Engenharia Civil had a number of strain gauges placed in the foundation rock of the Castelo do Bode and Venda Nova dams. The gauges are of the same type as those referred to by the author, the strains measured being much larger than those observed in the concrete. The influence exerted by the dead load on the foundation rock during construction can be seen from diagrams of these strain gauges. From one of those diagrams the modulus of elasticity of the foundation rock has been computed as about 30,000 kg.cm². This appears to be too low if account is taken of the good condition of the rock at the bottom of the valley (crystalline shale) as well as of its modulus of elasticity as determined by laboratory testing of specimens (200,000 kg.cm²). This may be explained by assuming that blasting brings about cracking of the rock. Should this be the case, the modulus of elasticity would increase quickly with depth, and much care should be recommended in drawing conclusions from such observations.

The data collected from tests in galleries, carried out before and after grouting (see Discussion p. 206) using large loading areas, and their comparison with the tests on properly selected drill cores, are, however, of assistance in assigning a magnitude to the modulus of elasticity of the foundation rock and to its variations from one place to another, which magnitude could be used for design with a reasonable degree of dependability.

Les auteurs confirment les remarques de MM. Bellier, Frey et Marchand (Comptes Rendus, vol. I, p. 319) sur l'influence des déformations des fondations sur le comportement des barrages. Ils citent les résultats d'observations identiques faites à l'aide d'extensomètres placés à l'intérieur des roches de fondation et indiquent que les essais de galeries et de carottes de sondages dûment choisis donnent également des éléments satisfaisants pour la prévision, pour les projets, du module d'élasticité des fondations (voir également discussion sur article 7/1 Comptes Rendus 1953, vol. II, p. 145).

M. L. Šuklje

M. Button (Comptes Rendus 1953, vol. I, p. 332) a traité la stabilité des fondations avec la méthode $\varphi = 0$ pour les cas où la résistance au cisaillement change avec la profondeur.

Je me permets d'exprimer à ce sujet les constatations suivantes:

Les surfaces de glissement les plus défavorables ne partent pas toujours du bord de la bande de charge. Il faut en tenir compte surtout dans les cas où la fondation repose sur une couche peu résistante d'épaisseur limitée.

¹⁾ Laboratório Nacional de Engenharia Civil, Lisbon, Portugal.

Dans les cas susmentionnés, il faut bien distinguer les charges flexibles et les charges rigides. Pour une charge souple uniformément répartie, la charge de rupture ne change pas avec le rapport entre la largeur de la bande de charge et l'épaisseur de la couche. Toutes les surfaces de glissement circulaires ayant une inclinaison déterminée, et dont le centre se trouve sur l'axe passant par le bord de la charge, sont équivalentes.

Pour des raisons analogues, les surfaces de glissement moins profondes deviennent plus défavorables lorsque la résistance au cisaillement augmente avec la profondeur, au moins pour une fondation reposant sur la surface du sol. Il en faut tenir compte en déterminant le coefficient de sécurité, bien que la rupture totale, fréquemment, ne soit atteinte que lorsque la surface de glissement s'approfondit considérablement.

Les constatations exposées ci-dessus ont été corroborées par les essais en modèle réduit exécutés aux Laboratoires du Bâtiment et des Travaux Publics à Paris (avec une graisse constante) ainsi qu'au Laboratoire de la Mécanique des Sols à l'ETS de Ljubljana, Yougoslavie (avec une vase) et cela pour des bandes de charge de profil rectangulaire, triangulaire, parabolique ou en gradins ainsi que pour des charges rigides. En outre ces essais ont montré:

a) qu'on peut obtenir, pour les fondations superficielles sur des couches d'épaisseur limitée, des solutions analytiques satisfaisantes par la méthode de surface circulaires ayant à la surface du sol une inclinaison $\pi/4$, pourvu qu'on tienne compte parallèlement d'une poussée quasi-hydrostatique à l'intérieur de la couche;

b) ils ont vérifié les résultats analytiques et expérimentaux de Meyerhof pour les charges planes rigides.

The author comments on the application of the $\varphi = 0$ method in cases where the shearing resistance changes with depth, and on the position of the most unfavourable slip circles. Further, he establishes a comparison between tests carried out by using a grease of a certain consistency, respectively a mud.

The possibility of arriving at satisfactory analytical solutions by applying the circular failure surface method is checked in these tests. Dr. Meyerhof's results are also commented on.

M. L. Šuklje

Si nous cherchons les causes de l'affaissement séculaire, nous devons tenir compte du fait que la relation logarithmique entre les déformations et le temps n'est pas un phénomène qui entre en jeu dans les systèmes «à deux phases» seulement. Nous nous en sommes aperçus par exemple en comprimant une vase pulvérisée et sèche fortement agitée préalablement dans l'œdomètre. L'affaissement logarithmique très net a commencé déjà quelques minutes après l'augmentation de la charge (de 0,274 à 0,5 kg/cm²); après 120 heures de consolidation nous avons immersé l'échantillon; l'infiltration de l'eau a causé un court tassement très intensif suivi, quelques minutes après, par un nouvel affaissement «séculaire» avec presque le même gradient que celui de l'échantillon sec. La Fig. 10 montre les résultats de deux essais de ce type. Nous avons constaté des comportements analogues bien que moins nets pour d'autres matériaux, par exemple pour une poudre de calcaire artificielle.

Dans l'opinion du Rapporteur général, M. Buisson, l'affaissement séculaire est la conséquence d'une déformation excessive, donc d'une contrainte trop élevée. Je suis d'accord avec lui, c'est la conséquence d'une contrainte trop élevée, non pas dans le sens d'une résistance limite du sol, mais dans le sens d'un effort dépassant la résistance actuelle du système granulaire. Les grains, entourés ou non de leur gangue d'eau ad-

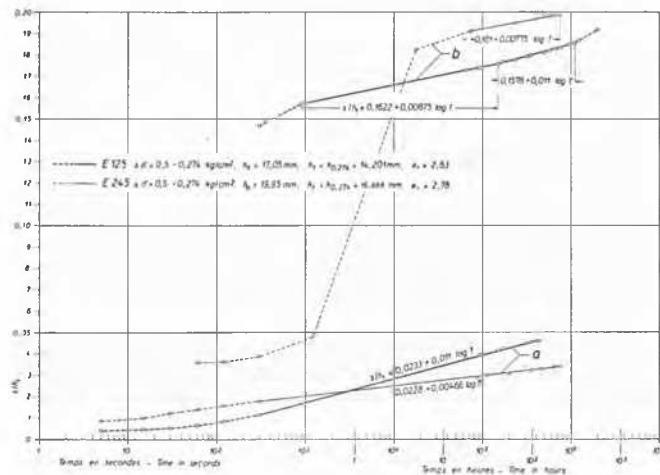


Fig. 10 Courbes de consolidation de la poudre sèche d'une vase: a) avant et b) après l'infiltration par l'eau
Time-Consolidation Curves of the Dried Powder from a Mud:
(a) Before and (b) After Infiltration by Water

sorbée, glissent en cherchant une position plus stable, et à mesure que le système prend une position plus stable, le glissement interne des grains diminue et la résistance plastique – appelée ainsi par le Prof. D. W. Taylor – faiblit.

La limite entre la consolidation hydro-dynamique et la consolidation «séculaire» est d'habitude très claire, au moins en ce qui concerne les essais œdométriques, même dans les cas où l'affaissement séculaire est très important. La construction connue de Casagrande ou celle de Taylor pour la détermination de la fin et de la valeur finale (100%) de la consolidation primaire suivant la loi de Terzaghi, nous a très bien servi même pour des vases très poreuses, manifestant une grande «résistance plastique». La Fig. 11 le montre pour une vase du Lac de Skadar (Yougoslavie). Bien qu'elle soit suivie d'un affaissement séculaire de presque le même ordre de grandeur, la consolidation primaire s'accorde très bien à la théorie de consolidation selon Terzaghi.

Dans les cas où le diagramme des surpressions d'une couche compressible peut être remplacé approximativement par un rectangle, les résultats des essais œdométriques nous permettent, selon la théorie de Terzaghi, d'extrapoler le temps de la

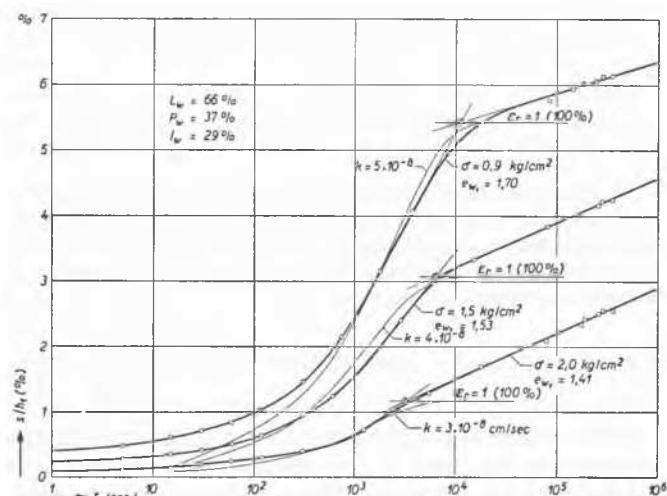


Fig. 11 Courbes de consolidation d'une vase du Lac de Skadar
Time-Consolidation Curves of a Mud from the Skadar Lake

fin de la consolidation hydro-dynamique dans le rapport $(H/h)^2$ des carrés de la hauteur de la couche et de celle de l'échantillon. L'application du même principe d'extrapolation pour l'appreciation du développement de la consolidation séculaire amène l'expression

$$\epsilon_r = 1 + b \log \left\{ \frac{T}{t_0} \left(\frac{h}{H} \right)^2 \right\}$$

ϵ_r étant le rapport entre la déformation spécifique correspondant à un temps quelconque $T > T_0$ et celle correspondant au temps T_0 de la fin de la consolidation hydrodynamique, b étant une constante résultant de l'essai oedométrique et t_0 étant T_0 de l'essai oedométrique.

Quelques résultats d'essais oedométriques faits avec des échantillons de hauteurs différentes ainsi que quelques mesures des tassements sur le terrain semblent confirmer approximativement la validité d'une telle extrapolation. Elle devrait cependant être justifiée par des observations expérimentales plus exactes et plus nombreuses que ne le sont celles dont nous disposons actuellement. Elle ne s'accorde pas à l'expérience mentionnée par le rapporteur général, M. *Buisson*, et qui est aussi la nôtre, c'est-à-dire que la perméabilité des vases devient très petite avec les petits gradients hydrauliques, si, d'autre part, nous tenons compte des constatations faites par *D. W. Taylor* que l'élimination de l'eau interstitielle, dans la période séculaire, s'effectue aux gradients minimes.

The author comments on tests carried out with a view to investigate secondary settlement. He explains this phenomenon as a sliding movement of the grains tending to settle in a more stable position. Further, the author develops a method of determining the duration of secondary settlements by using the law of similarity which enables us to apply the test results to full-scale experiments.

Mr. Tan Tjong-Kie¹⁾

Mr. *Mandel's* paper (Proceedings 1953, vol. I, p. 413), as Mr. *Buisson* justly pointed out in his General Report, deserves much attention. I would like to remark here that the same theory according to which the soil-skeleton is assumed to be elastically reversible, porous and filled with water, was also presented by *Biot* in 1941.

The first assumption already takes it for granted that the deformations should be limited, whereas the secondary time-effect is not taken into account. As this flow phenomenon is an integral part of the behaviour of clays, the viscosity of the soil-skeleton should not be disregarded. Therefore I think that consideration should be given to the following differential equations which I have derived for small deformations and under the assumption that the soil-skeleton may be regarded as a porous *Maxwell-body* (*Tan Tjong-Kie*)

$$-\Psi \Delta u + \left(\Theta + \frac{\Psi}{3} \right) \frac{\partial \varepsilon}{\partial x} + \frac{\partial \sigma_w}{\partial x} = 0$$

$$-\Psi \Delta v + \left(\Theta + \frac{\Psi}{3} \right) \frac{\partial \varepsilon}{\partial y} + \frac{\partial \sigma_w}{\partial y} = 0$$

$$-\Psi \Delta w + \left(\Theta + \frac{\Psi}{3} \right) \frac{\partial \varepsilon}{\partial z} + \frac{\partial \sigma_w}{\partial z} + \rho g = 0$$

$$\Delta \varepsilon = \frac{\eta_w}{3k\Theta} \frac{ap + 3E}{bp + E} p\varepsilon, \text{ with}$$

$$a = 6\eta(1 + \nu_e)$$

$$b = 6\eta(1 - \nu_e)$$

¹⁾ Laboratorium voor Grondmechanica, Delft, Netherlands.

Ψ	$= \frac{G\eta p}{(\eta p + G)}$
Θ	$= \frac{2G(1 + \nu_e)}{3(1 - 2\nu_e)}$
p	$= \frac{\partial}{\partial t} = \text{Heaviside's operator}$
u, v, w	= displacements in x, y, z directions
ε	= volume deformation
σ_w	= water pressure
k	= permeability
η_w	= viscosity pore-water
ν_e	= Poisson's ratio for elastic part of soil-skeleton
η	= viscosity of soil-skeleton
G	= shear modulus
Δ	$= \frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} + \frac{\partial^2}{\partial z^2}$

In the above equations pressure is taken as positive; it can be verified that for $\eta = \infty$ the equations are reduced to *Biot's* equations.

Following these lines settlements due to consolidation and secondary time-effects can be computed. In one-dimensional cases the ultimate settlement is $3(1 - \nu_e)/(1 + \nu_e)$ times as great as predicted by *Terzaghi* and *Biot* (for fat clays $\nu_e = 0.4 - 0.45$); in multi-dimensional cases the settlements ultimately attain a constant rate which is in agreement with measurements in practice carried out by *Terzaghi* and others.

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Tan Tjong-Kie (1954): Investigations on the Rheological Properties of Clays. Forthcoming Doctor's Thesis: Techn. Univ. Delft.

L'auteur relève que la théorie présentée par M. *Mandel*, selon laquelle le squelette du sol est élastiquement réversible, poreux et rempli d'eau, avait déjà été développée par *Biot* (1941). Il propose en outre de compléter cette théorie en introduisant la viscosité du sol dans le calcul ce qui permettrait de calculer l'effet secondaire du temps.

Mr. W. H. Ward²⁾

Mr. *Jennings* (Proceedings 1953, vol. I, p. 390) has in progress some field observations on the movements of buildings on unsaturated clays that promise to elucidate the mechanics of the process in some detail.

The writer has made a brief analysis of some of the data presented. It shows clearly that the lifting of the buildings is due primarily to the direct accumulation of rain beneath the buildings, but that there are a number of irregular secondary factors which require further investigation. No complex theories are required to understand the process. A plot of the cumulative rainfall against the lift from Figs. 1, 8 and 10 reveals the primary cause and confirms experiences gained by the writer in England and Canada. The "mean heaving curve" appears to have no physical significance.

It would be useful to know whether a deeper bed of clay under some of the buildings in Fig. 1, relative to those in Figs. 8 and 10 accounts for the greater lift per unit rainfall, and whether the variations from house to house are due to

²⁾ Building Research Station, Garston, Watford, Herts, England.

leaking drains, or to reference points that are not founded sufficiently deeply.

The data in the upper part of Fig. 1 and the lower part of Fig. 3 suggest, in the absence of extraneous sources of water, the possibility of an area of rain shelter on one aspect of the buildings. Records of rainfall distributions around a house at the Building Research Station showed a strong directional effect due to the prevailing wind. The results suggested an important secondary factor in the distribution of swelling under a building on unsaturated ground such as that with which Mr.

Jennings is dealing, but that it was not important under normal saturated English conditions.

It is gratifying to note that attention is being paid to the effect of trees. Their effects must be far more troublesome in the South African climate than in England.

L'auteur a examiné les données du travail de M. *Jennings* et est d'avis que les soulèvements par gonflement du sol sont causés par l'accumulation directe des eaux de pluie sous les bâtiments. Il indique divers facteurs secondaires pouvant provoquer une différence dans le soulèvement des bâtiments.