Barrages en terre, talus et tranchées ouvertes

Earth Dams, Slopes and Open Excavations

Sujets de discussion : Stabilité des talus. Evolution en fonction du temps. Intervention de l'ingénieur.

Subjects for Discussion: Stability of slopes, considered as a function of time ; effect of engineering works.

Président / Chairman : M. Vercon, *Yougoslavie,*

Vice-Président / Vice-Chairman : D. Olivier-Martin, *France,*

Rapporteur Général / General Reporter : D. H. Trollope, *Australie,*

Membres du Groupe de discussion / Members of the Panel : F. Arredi, *Italie*; J. Florentin, *France*; W. G. Holtz, *U.S.A.*; Y. Trofimenkov, *U.R.S.S.*; A. J. L. Bolognesi, *Argentine.*

Discussion Orale / Oral Discussion :

P. Anagnosti, *Yougoslavie,*
F. Arredi, *Italie,*
Z. J. Bazant, *Tchécoslovaquie,*
A. W. Bishop, *Grande-Bretagne,*
L. Bjerrum, *Norvège,*
A. J. L. Bolognesi, *Argentine*
V. Escario, *Espagne,*
J. Florentin, *France*
W. G. Holtz, *U.S.A.*
J. G. Lewis, *Australie*
A. L. Little, *Grande-Bretagne,*
A. Marsland, *Grande-Bretagne*
A. H. Naylor, *Grande-Bretagne,*
J. Osterman, *Suède,*
A. Penman, *Grande-Bretagne,*
K. L. Rao, *Inde,*
A. W. Skempton, *Grande-Bretagne,*
L. Šuklje, *Yougoslavie,*
G. Ter-Stepanjan, *U.R.S.S.,*
Y. Trofimenkov, *U.R.S.S.,*

Contributions écrites / Written Contributions :

J. Brinch Hansen, *Danemark,*
R. Coulomb, *France,*
N. Y. Denissov et G. A. Paooshkin, *U.R.S.S.,*
M. Doundoukov, *U.R.S.S.,*
O. K. Fröhlich, *Autriche,*
O. K. Fröhlich, *Autriche,* E. de Beer et E. Lousberg, *Belgique,*
J. A. Leadabrand, *U.S.A.,*
J. G. Lewis, *Australie,*
A. Mayer, *France,*
N. V. Ornatsky, *U.R.S.S.*

D. H. Trollope
Rapporteur Général, Division 6 / General Reporter, Division 6

Le Vice-Président :

Au nom du Comité d'Organisation, j'ai la charge d'installer le bureau de la séance consacrée à la discussion de la Section 6.

Le président de cette séance sera M. Milan Vercon (Yougoslavie), le Rapporteur Général M. Trollope (Australie). Le groupe de discussion comprend, de droite à gauche : M. Bolognesi (Argentine), M. Holtz (U.S.A.), M. Florentin (France), M. Trofimenkov (U.R.S.S.) et M. Arredi (Italie).

Je passe la parole à M. Vercon.

Le Président :

Mesdames, Messieurs,

Le Comité d'Organisation a voulu m'honorer en me chargeant de tâches assez lourdes, et notamment de celle de mettre en discussion un sujet qui est parmi les plus passionnants de la Mécanique des Sols, à savoir : Stabilité des talus — Evolution en fonction du temps — Intervention de l'ingénieur. Des membres d'une compétence indiscutable ont été désignés par le Comité d'Organisation, ce qui rendra ma tâche moins difficile. D'abord, le Rapporteur Général, M. Trollope (Australie), qui, avec son remarquable travail de synthèse, a su nous présenter une image très claire des rapports soumis au Congrès sur la question de la Section 6. Vous avez entendu M. Olivier Martin : M. Trollope sera secondé dans la discussion par les membres désignés par le Comité qui pourront, par leur savoir et leur expérience, contribuer largement à celle-ci. Le groupe ainsi formé, assisté de M. Olivier Martin, est incontestablement compétent pour aborder le problème. D'autre part, je me permettrai de faire appel à d'autres participants dans la salle.

Avant de donner la parole au Rapporteur général, il me faut donner quelques explications d'ordre général.

Le Président donne quelques indications précises sur la manière d'intervenir a la tribune, sur la durée de l'intervention et sur la façon de présenter les interventions écrites.

...Je m'excuse de cette explication qui était nécessaire et je passe la parole au Rapporteur général.

Le Rapporteur Général :

Before starting on a brief resumé of my general report, I would like to take this opportunity of expressing my sincere thanks to the French Organizing Committee and, in particular to Messrs Mayer, Habib and General Caminade, for the very great and kind assistance they have given me throughout the preparation for this Conference. It does mean a great deal, I am sure you realize, to have a sympathetic organizing committee.

There are one or two typographical and, perhaps some slightly more serious errors for which I apologize. I shelter under the fact that 13,000 miles is a long way away and I was not able to see the galley proof.

I will not bore you with all the detailed corrections but, before I start, there are two matters, that I would like to bring to your attention.

It was not my intention of defining a completely new " factor of insecurity "; on page 862, the ratio of activating moment — resisting moment, should obviously be reversed. And similarly on page 864. I am very grateful to my colleague Mr Trofimenkov for bringing this to my notice.

Another correction that might be necessary for later discussion : at the foot of the right-hand column of page 864, would you please read for the line A.B., the line A.C.

That is all I wish to comment on at this moment.

The topic for Section 6 is : *Earth Dams, Slopes and Open Excavations.* In many respects the building of earth and rockfill dams is viewed as the pinnacle of our achievement in this branch of soil mechanics, and in this context I think that at the present time we find ourselves at the beginning of quite an exciting phase of development.

As a profession, it took us quite a long time to reach a height of 400 ft. in earth dam construction. Now, at this Conference we have a note on the construction of Trinity Dam to over 500 ft; I understand from discussion this week that the Oroville Dam in California is going ahead and will, I believe, exceed 730 ft. in height; and, two weeks ago at the Large Dams Conference in Rome, our Russian colleagues informed us of a projected dam which will be very close to 900 ft. in height.

I think the stage is set therefore for an exciting period in this field of development and, I would like to try and direct my remarks this morning toward some element of caution. In this, I am doing no more than echoing the opinions expressed by our President, Prof. Skempton, earlier this week.

In going through my report it is not my intention to give a brief précis of all papers. I have selected those which I think will lead into the first discussion this morning and, I hope that the authors who find that their papers are not mentioned by me at this stage will understand that this is done in order to save time. I am sure we all recognize the value of all the papers which have been submitted and I, personally, am very grateful for having had the opportunity to read them in some detail.

Concerning excavations, I think there is a topic which merits brief consideration, although outside the general trend of discussion. I refer to the use of drilling mud (bentonite suspensions) in the forming of diaphragm cut off walls. I have recently had the opportunity of seeing some of this work in Italy and was duly impressed, as I was by the accounts given in the paper and, I do feel this technique as described in paper 69 by Mr Chadeisson and in paper 64 by Messrs Barbedette and Berra is quite an important step forward in our construction techniques.

Also in connection with excavations you will perhaps have noted that in my comments on the paper by Dr Leo Casagrande and his colleagues, I have made some comments on the role of moisture tension in ensuring stability of the slopes during construction. I do this because I would like to feel that considerable attention would be paid to the role of moisture tension in the stability question. It appears in our analysis as a negative 'u' factor in the strength equation and it appears to me that it can be quite important both in construction and in long term service.

It is perhaps regrettable, but nevertheless realistic, that one of the most fruitful sources of stimulus to progress is that of failure. I think nearly all of us hope to have the chance of investigating failures at one time or another provided they are not our own! We have one or two examples in this Conference of such investigations, and at this stage I would like to pay a compliment to Mr Marsland (paper 6/26) for the really excellent documentation of the description of this collapse in the Thames Valley. The point I would like to make about this — I think it represents a condition in relation to the time question — is, that in engineering, situations exist where there is a unique or critical set of circumstances which can occur at any time during the life of the structure. In this case we have a sudden very high flood level causing failure, as distinct from a progressive build up or break down of adverse conditions. It may therefore be pertinent to pay more attention, than we do at the moment, to assessing the statistical probability of these factors occurring during the life of the structure.

Also in this context, the paper by Mr Fukuoka (6/15) is, I think, a pointer to us. For some years now it has been evident in fields even outside soil mechanics and foundation engineering, that a great deal of work is being done on the observation of movements in slopes, excavations, etc. The techniques entail putting down bore holes and surveying them so that with intelligent interpretation one can forecast to some degree of accuracy what is happening within the slope and so perhaps avoid a catastrophe. This, I feel, is a most important tool. We should not be content with carrying out an analysis on a piece of paper, getting the construction done and then going away and forgetting about it. I think if we follow techniques like this we can perhaps get a lot of information, even without catastrophic failure.

The account by Messrs. Suklje and Vidmar in paper (6/37) is of particular interest in relation to the topic selected for discussion. The situation in the Gradot landslide is so complex that I found it extremely difficult to get a personal overall picture. However, it had obviously been studied in great detail by these authors and, I think the least we can say is that their observations are encouraging.

Dr Henkel has also brought further evidence towards support for the now well-known $c' = 0$ hypothesis. This is an intriguing hypothesis. However, I would like, at this stage, to express a word of caution in this. The physical implications of c' going to zero must, I feel, be bound up with the nature and, definitely, the stress history of the soil concerned.

I feel that active soils which have not been desiccated severely will represent the worst case, whereas an inactive soil with perhaps a very stable structure having been developed by high over-consolidation might tend towards the other end of the scale where this decay of c' to zero might not occur.

One of the most stimulating topics is obviously the theory of stability of slopes and, for a number of years, this has exercised the minds of a great number of people. Before getting down to some detail on this, I think this is a very opportune moment, as I have said in my report to recognize the great pioneer work of the French Engineer of last century, M. Collin, and I think that as a society we owe a great debt of gratitude to Mr Legget and his co-workers, and to Prof. Skempton, for his real contribution to the history of our subject.

The question of the factor of safety is obviously right in front of our minds in all aspects of Soil Mechanics. We have heard a great deal about it this week. Most of you will be aware of the discussion that has been going on particularly here, in Europe, about a number of methods. I found it encouraging therefore that — if I may so call them — the protagonists of the friction circle method have tended to bridge the gap somewhat in relating their factor of safety to a moment vector. Two of the papers, the first by Messrs de Beer and Lousberg, (6/6), and the other by Mr Fröhlich, (6/14), deal with this topic. As I said in my report, I find that by Messrs de Beer and Lousberg somewhat preferable to the — as I feel it — rather indefinite concept of the impulse put forward by Mr Fröhlich.

The paper (6/12) by Mr Escario caused me a great deal of bother over my Christmas dinner! I was severely upset by the appearance of a negative factor of safety. However Mr Escario obviously anticipated this in his paper and, after checking with him and following his analysis, I was able to find that at least the calculations leading to this negative factor of safety were correct. But I still feel that the rather arbitrary and I think unreal assumptions used by Mr Escario in arriving at this result do invalidate many of the conclusions.

I perhaps owe some explanation when I say that the differences are in reverse order to that previously claimed in relation to Dr Bishop's method posed in 1955. I felt that the divergence at values of the Factor of Safety < 1 conflicted with the way I had become used to thinking of the divergence between the two methods when the Factor of Safety was > 1. However, I may be wrong in this and perhaps we could have some discussion on it later ?

I would like to say just a brief word on the very important aspect of soil plasticity. This is a most important tool, but I would like to emphasise that at the moment, as far as I can see, all work in this field is entirely confined to applied stress variables.

On another occasion this week I had the chance of mentioning the seriousness of eliminating the pore pressure variable from Kötter's equation. This was brought out by Carillo in 1942, and while these investigations are of great value, I would like to see some more attention given to the development of an effective stress philosophy in slopes.

There are other aspects of soil engineering, for example instantaneous loading of foundations, where applied stress variables are applicable. But in slopes I think we have gone so far in effective stress philosophy that work on soil plasticity should be directed in the same way.

Since writing this report, I have seen and read an important paper by Messrs Bishop and Morgenstern, published in *Geotechnique* in 1960, and I felt that my report to this Conference would not be complete without taking this into account. Also during the last few months in relation to this work we have had the opportunity at Melbourne of doing some experiments on the question of the factor of safety of slopes; and if you will bear with me for just a few moments, I would like to show you a very short film of these experiments.

The models are approximately 6 ft. 6 in. high, symmetrical wedges of single sized (3/8 in) crushed rock, loosely placed.

At the centre a cut off is placed which is not extended to the full height of the model. Water is then allowed to discharge through the downstream slope.

In this way we have been able to reproduce conditions where the failure occurs on a surface of sliding that approximates closely to a circular arc. The failure is recorded by cine-camera on colour film so that the conditions at failure are known. The film shows that there are two mechanisms of failure — one a surface erosion that has been called an unravelling failure, the other a relatively deep seated circular slip. For present purposes we may disregard the unravelling failure as this can be treated as a surface support problem.

With this technique it is possible to analyse conditions on the actual failure surface. As the pore pressure distribution is known and the shear strength properties can be measured the unknown variable is the distribution of normal stress on the failure surface.

The results of a typical example are:

Method	*Calculated Factor of Safety*
Swedish Slices (May 1936) [1]........	1·13
Bishop (1955)	1·12
Bishop and Morgenstern (modified) .	0·95
Trollope (No-Arching Solution 1957).	0·94
Bishop and Morgenstern (1960)	0·59

(1) In the original presentation this value was quoted as 0.81 owing to an arithmetic error.

The surprising result of 0·59 from Bishop and Morgenstern's method was obtained by extrapolating their curves to the slope of 1·19: 1 used in these tests and using the method suggested by them for estimating the average value for the pore pressure coefficient r_u. When the actual pore pressure values as measured in our tests were taken then the method gave a factor of safety of 0·95.

This example does serve to focus attention on the need to predict pore pressures accurately. In order to arrive at the value of 0·95 in the modified method it was necessary to know the position of the actual slip surface and then calculate the mean value of r_u on this surface. It is not suggested at present that the results of these tests are correct to the order of accuracy shown in the table. It will be necessary to carry out a considerable number of tests in order to assess the range of error involved.

This leads me to the topics suggested for discussion.

I would start off by combining items 1 and 3 suggested as the questions. The decrease of soil strength and the associated problem of the tension field — I will have more to say about that in just a moment. The starting point for the decrease in soil strength is obviously the $c' = 0$ hypothesis and, I think it would be most useful if we could view this in the light of the influence of the tension effects.

The second point concerns the internal stress condition and, here I am going to eliminate the foundation completely if we just consider the slope. This is something which does give me a little concern. At the moment, it is merely a theoretical prediction that if a dam rests on a deformable foundation then the internal shear stress distribution becomes far more severe than for the case of a dam on a rigid foundation.

It would appear therefore that there is some reinforcement to this feeling that as we go to these higher and higher dams, we may be stepping a little beyond the bounds of our experience. I would be most interested to know of any contributors who could give us some information on the behaviour of dams on deformable foundations.

The other question is that, in slopes, if one excavates in a soil that heaves at the toe, the relative deformation, the rise of the toe relative to the inside, is physically analogous to settlement within the slope and also gives rise to these more serious stress distributions.

It is on that note that I would like to close, after trying to draw this together in a little diagram I prepared merely to give to you as a focussing point for some of these factors.

In Fig. 1*, I have drawn what I have noted of the Sokolovsky Arch. We were all most interested in the last Conference to read this work by Prof. Sokolovsky. However, I feel that in relation to time effects, Sokolovsky's work implies not only a constant tensile strength of the material, but that the envelope is extrapolated past the origin in a straight line. In a geological sense therefore, although these arch formations can be recognized, they should perhaps be regarded as temporary. This depends on the rate at which cementation, and erosion factors, can take place. In clays, I feel, the breakdown will be far more rapid.

I believe a number of other people have been for a long time unhappy with calculating the depth of a tension crack from simple active pressure theory. I would submit to you that the tension field exists above the line A.C., that below this the soil can adapt itself as groups of particles — (this is clay as well) — into a compressive system and, by compressive I mean there are compressive forces between all particles, and so it will adjust itself to this.

The composite surface of failure for relatively high slopes with a progressive deterioration towards what I have called the equilibrium slope following Prof. Skempton's work reported in the 1957 Conference is perhaps a factor that we should look at in relation to the decay of slopes with time.

Le Président :

Je remercie M. Trollope pour la façon très intéressante dont il nous a exposé ses idées. Je suis sûre que cette explication très claire pourra nous aider dans la discussion que nous allons maintenant déclencher.

M. Trollope nous a proposé une spécification plus étendue des sujets à l'ordre du jour en ce qui concerne les points suivants — comme vous pouvez le lire dans le rapport général — :

1. Recherche des éléments qui contribuent à la diminution de la résistance des sols en fonction du temps, dans les talus;

2. Modifications des contraintes en terre dans les talus des remblais, en fonction du temps;

3. Existe-t-il des sols soumis à des efforts de traction dans les talus constitués par de l'argile et, dans ce cas, ces sols argileux peuvent-ils résister à ces efforts ?

M. Trollope a bien voulu nous expliquer qu'il s'agit évidemment de questions qui encadrent des problèmes très vastes qui peuvent entraîner des sous-questions plus détaillées.

Pour avoir une discussion plus vivante, nous avons décidé de porter le débat sur les points 1 et 2 réunis, qu'il s'agisse de talus naturels ou de talus artificiels, en remblais ou en déblais.

La question est posée de l'existence dans les talus de zones soumises à des efforts de traction. Les membres du groupe sont libres de nous présenter leur point de vue qui devra être relié étroitement à la question posée par M. Trollope.

(*) Volume II, page 865.

Avant d'ouvrir la discussion, je prie les participants qui voudraient intervenir après la discussion de présenter leurs résumés avant onze heures, au moment où une interruption sera faite.

Pour commencer, je donne la parole à M. Florentin qui va nous parler du phénomène concernant les talus naturels considérés comme « éprouvettes » et répondra — ou tâchera de répondre —, à la question de savoir si on peut construire sur des glissements et dans quelles conditions.

M. Florentin (France)

C'est une lourde charge qui m'échoit d'ouvrir le feu aussitôt après le si remarquable rapport de M. Trollope.

Dans la Mécanique du Sol pathologique, ce sont les glissements des talus argileux qui constituent le cas le plus intéressant et sans doute le plus grave. Comme dans tout phénomène où l'homme n'est pas maître de tous les éléments, l'étude de la stabilité présente souvent aux chercheurs des aspects différents et parfois contradictoires. Mais je crois qu'il faut se ranger à l'avis de notre rapporteur général et admettre que, malgré ses lacunes, la méthode la plus simple et, dans une certaine mesure, la plus exacte dont nous disposions actuellement, après cinq congrès de Mécanique du Sol, est celle qui fait intervenir des contraintes réelles intergranulaires dans un glissement supposé circulaire. Cette méthode est souvent mise en discussion.

L'agnosticisme en tant que méthode de recherche est valable pour le savant. Il appelle l'ingénieur à ne pas faire trop de fautes. Mais il ne faut pas qu'il le conduise à l'inaction. De sorte qu'une méthode simple, à une étape donnée du développement scientifique, est valable. La méthode des glissements circulaires est simple et le progrès de tous les jours a besoin d'une méthode simple. Les spécialistes que vous êtes ne sont en fait au courant que d'un petit nombre de glissements par rapport à la masse des glissements qui se produisent sur les chantiers au cours des opérations qui intéressent les sols. Le progrès quotidien est à atteindre par la diffusion d'idées simples permettant, sur les chantiers, d'éviter des fausses manœuvres qui peuvent conduire à des désordres graves.

Dans le cadre de la méthode dont nous disposons, comment s'inscrit la question de stabilité à long terme ?

Deux seules communications y sont effectivement relatives : celle de M. Suklje et celle de M. D. J. Henkel. Toutes les deux supposent $c' = 0$.

Si on tient compte en Mécanique du Sol que la cohésion résulte de la mémoire que le sol a gardée des contraintes réelles antérieures, l'hypothèse revient à dire qu'à la longue, sous l'influence des forces extérieures, désagrégatrices, le sol a perdu sa mémoire.

Je pense que c'est sur les talus naturels argileux que les vérifications de cette théorie sont probablement les plus efficaces et les moins coûteuses. Pour les talus en remblais, l'ingénieur est, dans une certaine mesure — puisqu'il les crée — maître des conditions aux limites. Il peut empêcher le sol de perdre sa mémoire en munissant son ouvrage d'éléments qui empêchent cette perte de mémoire. Citons, pour les digues en terre, les recharges perméables sur le talus amont, les filtres, les zones de transition, etc.

Pour les talus en déblais, seule l'analyse des ruptures est fructueuse. Il faut évidemment en profiter. Il y en a souvent. Mais on n'est pas toujours au courant.

Par contre, pour les talus naturels argileux, la nature s'est chargée partiellement de faire le travail de désagrégation et l'observation est à notre disposition. Ce que je suggère, en somme, c'est un travail analogue à celui qu'a fait notre collègue M. Lane (U.S.A.), sur des talus de structure rocheuse, mais de roches altérables. Mais je suggère qu'on ajoute aux observations une étude très poussée des conditions hydrogéologiques qui règnent sur le talus et des études de laboratoire. Il faut choisir des talus qui ne soient pas modelés par l'érosion et qui soient en équilibre limite, parce que leur stabilité a été progressivement diminuée par la dégradation des caractéristiques mécaniques. En fait, ces talus ne sont pas en permanence en équilibre limite. Sinon, il y aurait longtemps qu'ils n'existeraient plus. Mais les conditions les plus sévères se réalisent temporairement et par exemple, dans le Midi de la France, on attend avec impatience les premières fortes pluies après la saison sèche car c'est réellement la période où se déclenchent les glissements. L'angle de ces talus naturels s'est adapté aux caractéristiques du sol et aux contraintes réelles qui peuvent exister dans le cas le plus grave. Pour l'observation de ces talus, la géométrie des parties extrêmes est perturbée par la nécessité du raccordement avec le relief général. Mais c'est surtout l'hypothèse relative à l'écoulement de l'eau qui constitue le point le plus délicat. Si le talus a une grande hauteur, on suppose en général l'écoulement parallèle au talus. Si β est l'inclinaison du talus, le gradient hydraulique est $i = \sin \beta$. Si φ' est l'angle de cisaillement intergranulaire, nous avons la relation

$$\mathrm{tg}\beta = \frac{\gamma_1}{\gamma_1 + \gamma_w} \cdot \mathrm{tg}\,\varphi'$$

Dans la mesure où on suppose que la densité de l'eau (γ_w) et la densité compte tenu de la poussée d'Archimède (γ_1) sont égales, on a la relation classique $\mathrm{tg}\,\beta = \frac{1}{2}\,\mathrm{tg}\ \varphi'$

On peut objecter que, dans la nature, on a des conditions de circulation d'eau qui peuvent être plus graves que la circulation parallèle au talus.

Vers la base du talus, l'inclinaison de l'écoulement sur l'horizontale diminue, et peut même passer sous l'horizontale. Si α est l'inclinaison de l'équipotentielle sur la normale au talus, comptée positivement si l'équipotentielle est dirigée vers le pied, et négativement si elle s'en écarte, la formule générale devient :

$$\mathrm{tg}\,\beta = \frac{\gamma\,1}{\gamma_1 + \gamma_w\,(1 + \mathrm{tg}\,\alpha\,\mathrm{tg}\,\varphi')} \cdot \mathrm{tg}\,\varphi'$$

L'angle β diminue lorsque α augmente.

Pour un écoulement horizontal ($\alpha = \beta$) et avec $\gamma_1 = \gamma_w = 1$, on a $\beta = \frac{\varphi'}{2}$. Avec $\varphi' = 25°$ par exemple, l'écart sur β entre l'écoulement parallèle au talus et l'écoulement horizontal est de 1°. Il est de 3° 6/10 pour $\varphi' = 35°$.

Ceci valide pour des argiles l'hypothèse d'un écoulement parallèle au talus. L'observation des talus naturels et la comparaison de β avec φ' présentent donc un très grand intérêt.

Rappelons toutefois que, dans des milieux anisotropes, il peut exister, loin derrière le talus argileux, des conditions hydrogéologiques plus sévères. C'est le cas par exemple d'un placage argileux, même épais, contre un massif calcaire fissuré, siège de circulations d'eau.

Peut-on construire sur d'anciens glissements?

On construit souvent sur d'anciens glissements sans le savoir. Ce sont ceux qui se sont passés auparavant. Par ailleurs, il existe ou même on projette de nombreuses constructions sur des talus qui sont en équilibre limite. La charge extérieure ne représente pas toujours l'élément le plus perturbateur. C'est surtout la modification des conditions aux limites que la construction introduit, qui peut avoir les conséquences les plus graves. Deux cas sont possibles. Ou bien on a modifié les conditions aux limites dans un sens défavorable et le talus peut glisser même avant la fin de la construction. Ou bien, alerté par l'observation, on cherche à modifier les conditions aux limites dans un sens favorable et on peut réussir (drainage profond, protection contre la fissuration).

Dans l'exemple que je vais vous donner, les deux cas se sont présentés successivement. Il s'agissait d'un talus dont la pente était de l'ordre de 20 pour cent (11°). Apparemment donc, pour un angle intergranulaire de l'ordre de 30°, le talus présentait un coefficient de stabilité appréciable, même pour une circulation permanente, parallèlement au talus. Les hameaux voisins : « Les Roulettes » — « Au Cruchon » — « Les Fontaines » — ont des noms évocateurs de conditions hydrogéologiques sévères.

Dans une première modification désavantageuse, le glissement se produit car on a mis, en crête de ce talus, un dépôt de 15 000 m^3 d'argile introduisant une charge de 350 tonnes/mètre linéaire, ce qui n'est pas négligeable. De plus, la saison pluvieuse fait de cette charge argileuse une réserve d'eau, ce qui permet la réalisation de conditions hydrauliques défavorables. Le talus glisse dès fin 1944 pendant près de deux ans. La vitesse est de 1 à 2 cm par jour à partir du moment où on a installé des moyens d'observation, soit huit mois après le début. Perplexité : doit-on abandonner le chantier ? Ce talus n'était pas à utiliser en première phase ; il ne servait que de décharge. Mais il était prévu pour une utilisation future. Le seul moyen dont nous disposions était de faire varier les contraintes intergranulaires dans un sens favorable. Le remblai est enlevé au printemps 1946 en même temps que l'on sème du gazon et que l'on plante des acacias. On installe en 1946 des drains verticaux avec pompes et, par la suite, des drains horizontaux. Le calcul montre que, compte tenu du drainage et des essais de laboratoire, le talus a un coefficient de sécurité supérieur à 1,5 si le drainage permet de maintenir une piézométrie basse. En 1952, les extensions sont construites sur le glissement après que l'on ait ajouté une nappe de drains horizontaux.

Prenons un autre exemple. Dans une ville comme Paris, Auteuil est construit sur de l'argile plastique. Avant qu'Auteuil ne soit inclus dans Paris, c'était un petit village très agréable sur lequel il y avait des pentes, très douces, d'argile plastique. Les premières constructions dans le quartier d'Auteuil ont donné lieu à de nombreux glissements. Actuellement, Auteuil est dans Paris, revêtu de bitume et l'eau passe dans les gouttières d'où elle gagne les égouts. L'argile plastique a une pression de consolidation élevée et on a beaucoup moins d'incidents maintenant — je ne dis pas qu'on n'en a pas du tout — même dans des fouilles plus profondes qu'autrefois. Ceci provient du fait que les conditions de circulation de l'eau ne sont plus les mêmes qu'autrefois. Il n'y a probablement plus la possibilité d'une dessiccation superficielle qui, au moment de la reprise des précipitations, provoquait des glissements dans les chantiers.

Le Président :

Merci, M. Florentin. La parole est à M. Holtz qui va nous présenter son exposé sur les gonflements et sur la résistance au cisaillement en fonction du temps.

M. W. G. Holtz (Etats Unis)

When moisture changes occur in expansive soils, the shear strength can be affected greatly. The shear strengths of all clay soils are influenced a considerable amount by moisture changes, but expansive clays often are subject to extreme changes in shear strength. In addition to the strength factors related to the minerals involved, the interrelation of moisture, density, and load plays an important part in the strength of expansive clays. The Bureau of Reclamation has encountered expansive clay soils at numerous construction sites located throughout the western part of the United States (Holtz, 1959).

Triaxial shear tests can be used to determine the shear strength. However, the sequence of loading and wetting (or drying) affects the volume changes and shear strengths determined. Therefore, it is important to duplicate prototype conditions closely. Fig. 1 and 2 are typical shear test data plots. These tests were made on undisturbed and remolded clays, respectively, from the Gulf Basins Project, Texas. The clays were a Ca-Beidellite. A most important fact to be noted from these tests is the loss of cohesion when the soils were wetted and dried and rewetted from the initial conditions. These changes are caused by expansion and related increased moisture content. The undisturbed soil (Fig. 1) had a cohesion of 4·5 psi at natural conditions. Upon wetting, the density decreased and the cohesion was reduced to 2·2 psi. When the soil was air-dried and rewetted, further density decrease occurred, and cohesion was reduced to 0·9 psi. There molded soil (Fig. 2) (remolded to 95 per cent of Proctor maximum density at optimum moisture content, less 2.5 per cent) showed greater strength loss. Under the same sequence of testing, the cohesion varied from 14 psi to 0·7 psi to 0·4 psi, respectively.

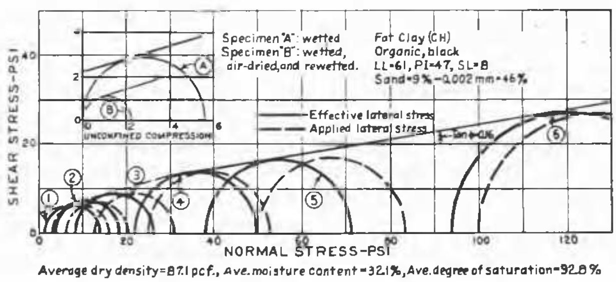

Fig. 1 Shear test data - undisturbed soils. Gulf Basins Project, Texas.

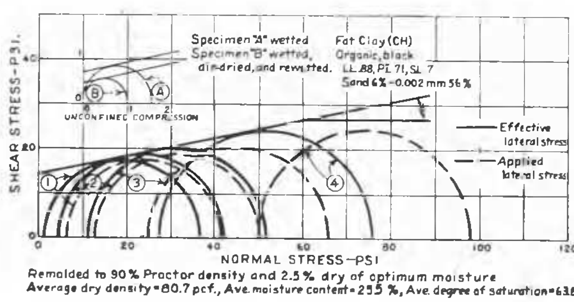

Fig. 2 Shear test data - remolded soils. Gulf Basins Project, Texas.

Fig. 3

Fig. 3 is a photograph of a series of slides in an earth section on the Friant-Kern Canal in California. The soil is Porterville clay of the Ca-Beidellite type. This section is 23 feet deep and on a 1-1/2 horizontal to 1 vertical slope. Slopes rebuilt on 2:1 were also unstable. Fig. 4 is a photograph of a slide of a 1 - 1/4:1 slope of a concrete-lined section of this canal. Deep, longitudinal shrinkage cracks occurred extensively along the banks and the clays at the base of the slopes became soft. The extent of shrinkage cracking can be seen readily at the upper part of the slide in the figure. Slides of these types have been occurring from 2 to 10 years after the canal was put into operation. This example further points up the length of time often involved for changes in moisture which cause expansion and shrinkage of these impervious clays. The original design was used several years ago as a calculated risk because replacement soils would have required extremely long hauls and very high costs.

Fig. 4

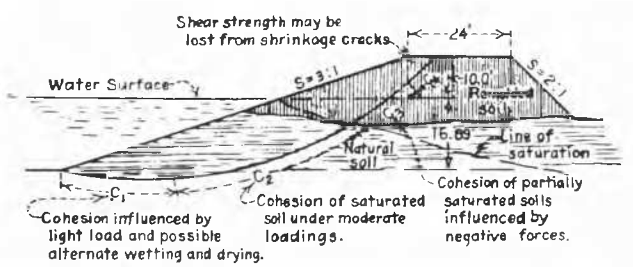

Fig. 5 Design of Slopes.

Fig. 5 is a simplified typical slope condition for a canal section in expansive clay. As shown on the figure, the cohesion at the base of the sliding arc (C_1) will be influenced by saturation, low loadings, and possible drying and resaturation. High volume changes and resulting low cohesion can, therefore, be anticipated. The soil above this segment and below the saturation line will be saturated, will be moderately loaded, and will be less likely to be subject to severe cycles of wetting and drying. Therefore, the cohesion of this segment (C_2) may be greater than C_1. Above the line of saturation, free water conditions do not exist, and the cohesion of the soil (C_3) is influenced by capillary and other tension forces, and may be quite high. As shrinkage cracks often open to depths of several feet when expansive-type clays become very dry, the cohesion (C_4) and frictional strength at the uppermost part of the arc cannot be relied upon. The above factors must be considered in any stability study. Only the cohesion strength factor was stressed because this factor has the great strength change.

Protective measures which can be taken to guard against shear failures in expansive-type clays are as follows :

The soil can be removed to adequate depth and breadth, and be replaced with nonexpansive soils.

Means for keeping moisture from entering or leaving the clay can be adopted. Shear failures have not occurred on the slopes of the Friant-Kern and Gateway Canals, where asphaltic membranes were placed between the concrete lining and the subgrade clays.

Designs can be adopted which will fit the soil conditions. For the moderate to highly expansive clays studied, slopes varying from 2 - 1/2:1 to 3 - 1/2:1 will usually provide adequate stability for canal banks about 25 feet high, even under relatively rapid drawdown conditions.

The clay soil can be stabilized or modified by the addition of cement, lime, or other additives. The most practical way to secure such stabilization today is to remove the expansive clay and replace with compacted clay treated with the additive. Sufficient depth and breadth of treated soil, as necessary, to achieve the desired stability would be required, just as in the method involving refilling with nonexpansive soils. I understand Mr. Leadabrand plans to discuss the soil-cement facing of one of our dams. While this dam was faced for wave protection, the procedure would be applicable to the above remedial measure.

Shrinkage in Earth Dams

Shrinkage during the construction of an earth dam can also be an important consideration. An experience which occurred at Lovewell Dam is cited as an example. Lovewell Dam, which was constructed during 1955 to 1957, is an earth structure located on White Rock Creek, a tributary of the Republican River in the northern part of the State of Kansas. The dam has a maximum height of about 65 feet and a crest length of 7,500 feet. The main section of the dam, Zone 1, was constructed by compacting fine-grained soils in 6-inch compacted layers using heavy sheepsfoot rollers.

At a time when construction was nearing completion, a program of drilling test holes through the dam was initiated to study the settlement of foundation soils beneath the axis of the dam. During the drilling, appreciable amounts of drilling water were lost. In one test hole this amounted to 750 gallons in 5 minutes in the bottom 5 feet of the fill. A test pit was put down through the dam at this location. Examination of the test pit showed that, while no cracks were found during excavation of the top 40 feet, many shrinkage cracks, up to 1/4 inch in width, developed from drying within 2 to 7 days after the pit was excavated. From 40 to 65 feet, the walls of the pit showed a well-compacted unstratified fill. No large voids or cracks could be found in the bottom 5 feet of the fill in which water losses were reported to have occurred. Undisturbed samples of 1-cubic-foot size were cut from the pit and shipped to the Denver Laboratory for examination and evaluation.

The average characteristics of the soil from this section of the dam were as follows :

1. Material finer than 0·074 mm = 97 per cent
2. Liquid Limit = 43 per cent
3. Plasticity Index = 25 per cent
4. Shrinkage Limit = 13·5 per sent
5. In-place Density = 105 pcf
6. Proctor Maximum Density = 101 pcf
7. In-place Density in per cent of Proctor Maximum = 104 per cent
8. In-place Water Content = 19·5 per cent
9. Optimum Water Content = 20·3 per cent
10. Permeability = 0·03 feet per year
11. Soil Classification (Unified) — Medium Plastic Lean Clay (CL)
12. Clay Fraction Consists of Illite and Montmorillonite.

The undisturbed block samples showed no readily noticeable cracks. Special shrinkage tests were performed in the laboratory to study the effects of drying and wetting. Specimens cut from the block samples showed cracking a few minutes after starting air-drying and gained their maximum surface width of 1/4 inch within about 24 hours. Drying was continued for 1 week after which the specimens were wetted. Upon wetting the cracks closed, but redeveloped when the specimens were again allowed to dry. Consolidation and shear tests were made to study these soil characteristics on the soils dried and rewetted. Upon drying the consolidation specimens, volume shrinkage in the order of 10 to 13 per cent occurred. Upon wetting this volume was regained, and swelling, under 5 - psi loadings, in the order of 1·5 per cent occurred. The consolidation characteristics of the compacted soil was increased somewhat after drying and rewetting.

A review of the embankment construction showed that the first 5 - foot thickness of Zone 1, which was constructed at this location, was permitted to remain exposed for about 3 weeks during which time the weather was "unusually dry and windy". Some of the materials which were exposed to drying were particularly susceptible to shrinkage. It was, therefore, concluded that conditions and materials were favorable to the formation of large shrinkage cracks which, as the result of light rains, were not visible when embankment construction was resumed. Since no cracks were evident in the bottom of the test pit at an elevation where the water loss occurred, it was believed that the cracks were sealed to a major extent. This was probably caused by the addition of water from the drilling of 35 test holes in the area prior to excavating the test pit. It was further concluded that saturation of the fill by the reservoir should complete the closing of any remaining cracks. No significant seepage has occurred through this dam during several years of operation.

Although the results of this study showed that there was no danger to this dam, they do point up the need to exercise caution during construction with respect to the drying of soils subject to shrinkage. This would be particularly true when constructing a closure section against embankment soils exposed for long periods. In this case, it would be good practice to excavate previously placed embankment to sufficient depth to remove soils in which cracks had developed.

Références

[1] Holtz, W. G. (1959). « Expansive Clays — Properties and Problems », *First Annual Soil Mechanics Conference of the Colorado School of Mines*, Golden, Colorado, Quarterly of the School of Mines, p. 89.

Le Président .

Merci, M. Holtz.

M. Trofimenkov a justement voulu nous poser la question de la résistance aux cisaillements pour savoir si elle dépend du temps. M. Holtz a apporté sur ce sujet la contribution de ses expériences remarquables. M. Trofimenkov a, je pense, autre chose à nous dire et je lui passe la parole.

M. Trofimenkov (U.R.S.S.)

In spite of fact that the topic of today's discussion : stability of slopes considered as a function of time, was indicated long before the Conference, only a few papers out of 39 contribute directly to this subject.

It does not mean, I believe, that the problem is unimportant for practice. It shows only that the problem is a very difficult one and that scientific workers and practical engineers have not got very much reliable data on this subject.

In October of the last year a Conference was held in the U.S.S.R. on the flow and long term strength of clay soils. More than 20 reports were made and different opinions were expressed. There were assertions that during a flow under a constant load the shear strength of the soil is decreasing with time. The rearrangement of the clay particles may be the cause of it. There also were views expressed (Prof. Denissov, Kogan) that time itself, if there is no swelling or decreasing of the cementing bonds, has no influence upon a shear strength and that only the change in external conditions may influence the shear strength.

The similar contradiction we have seen here in Section 1 of our Conference. This shows in what a difficult situation practical engineers are. This may be one of the questions of today's discussion. Really it is very difficult to evaluate the stability of slopes as a function of time because the stability depends both on the change in stresses of the soil mass and on the change of the soil strength. The lack of a reliable method of evaluating theoretically the long-term stability of slopes forces engineers in their practical work to search for indirect ways to solve this problem. For the specialists of our country it is of particular importance, as in many cases now the working of natural resources is organized in open cuts even if their depth reaches over 200 metres.

While speaking about indirect methods I should like to mention the paper 1/54 by Saito and Uezawa in which the authors suggest that by measuring the surface strain of slopes it is possible to forecast slope failure. Their method is applicable to the last stage of the creep just before the failure, which is somewhat too late for engineers, I believe.

We use a method which was developed on the finding that very small inclinations of the ground surface near the top edge of the cut slope can indicate the latent stage of slide movement and the change in the slope stability long before the appearance of cracks on the surface (some weeks or months). These inclined movements (differential deformations) are the result of local plastic deformations in a body of slide mass.

From the paper 6/15 presented by Fukuoka and Taniguchi it is seen that rather like procedure was used in Japan to determine the area of the landslide. The importance of such a method has been underlined in today's speech of the General Reporter, Mr Trollope.

For the last few years we have been designing and making supervision on the construction of a big mining open cut of about one square km of area and up to 100 m deep. The bottom of the cut is 60 m below the original water table. The special instrumental control of the slope stability carried out in the course of the construction should give us some data on slope deformations in terms of time, which may be of some interest to our Section.

The soil conditions were as follows : 12 m of Quaternary sandy clay, 30 m of sand, 30 m of marl and 25 m of Jurrassic sandy clay.

All of you understand too well that while designing the slopes of such a cut, we used almost all the methods available including Bishop's and Morgenstern's published recently.

For homogeneous soil medium we usually apply the exact solution of limiting equilibrium theory developed by Prof. Sokolovski. This solution is tabulated and can be used by every engineer.

For unhomogeneous medium we applied the approximate solution of limiting equilibrium theory developed with the help of a digital computer. The average angle of inclination of the cut sides was accepted to be 25°. The average factor of safety was of about 1·40 calculated by the slip circle method using effective stress parameters. By the approximate solution of limiting equilibrium theory, the factor of safety was determined at about 1·50.

In addition to these theoretical investigations, the instrumental observations of the ground surface inclinations at the top of slopes and on berms have been performed during construction by special clinometers or precise levelling of bench marks. For the surveying crack development crackmeters have been used with the error of reading 0·01 mm.

The dip of the surface is characterised by the angle of its inclination per day.

The supervisions in clay slopes with the factor of safety of about unity have shown that the speed of the surface inclination (angle dip) up to 20 seconds a day characterises the latent stage of slide development when cracks had not appeared. In some cases (on different job sites) cracks had been formed at the speed of the surface inclination, from 40 seconds to about 3 minutes a day. It should be noted that there were surface inclinations in the direction of an open cut and in the opposite direction.

Very interesting data were received by S. N. Nikitin while measuring the rate of cracks widening. It was several mm a day and reached 30 mm a day on the eve of the failure.

While measuring the rate of the widening of cracks with instruments of great accuracy, it was found out that the widening was not uniform during a day but proceeded in steps. In the observations performed every one to three hours a leap of 0·3-0·4 mm took place. These leaps may be considered as a result of a local failure of the soil in a zone of the slip surface.

Since the described investigations allow the prediction of the transition of a slope in an unstable condition they are very useful in practical work.

The *in-situ* instrumental observations on the long-term stability of slopes may be also a subject for today's discussion.

Le Président :

Je remercie M. Trofimenkov.

M. le Prof. Arredi se propose de vous présenter une communication sur les surfaces de rupture et sur la stabilité des barrages en fonction du temps.

La parole est à M. le Prof. Arredi.

M. ARREDI (Italie)

Ma contribution à la discussion sera limitée au sujet de l'évolution, en fonction du temps, de la stabilité des barrages en terre.

J'essayerai de souligner le facteur qui, à mon avis, pourrait influencer à long terme les caractéristiques mécaniques ou hydrauliques des matériaux qui composent ces ouvrages.

Je m'excuse de regarder la question surtout d'un point de vue technique; c'est-à-dire de chercher des conclusions pratiques même si les phénomènes observés ne sont pas encore éclaircis, du point de vue scientifique; je pense que ceci est exigé par l'importance de tels ouvrages. J'espère que mes camarades du groupe de discussion et tous les Congressistes voudront bien m'apporter leur aide. Il se peut que la question dans le cas des barrages soit plus facile à discuter parce que nous connaissons mieux les caractéristiques des matériaux d'un massif artificiel que des matériaux naturels en place.

Dans un barrage en terre nous avons en général trois espèces de matériaux : la terre plus grossière constituant les deux demi corps à l'amont et à l'aval, la terre fine plus ou moins argileuse constituant le noyau et enfin le matériau de fondation : chacune de ces espèces pose une question particulière au sujet des rapports entre le temps et les conditions de stabilité de l'ouvrage.

La caractéristique mécanique fondamentale et unique du matériau granulaire grossier, avec lequel, en général, sont formés les demi corps de l'ouvrage, est l'angle de frottement. Et aujourd'hui on ne doute pas qu'il ne demeurera constant dans le temps que si les caractéristiques des surfaces des grains demeurent inaltérées.

Mais il est possible de penser que, à long terme, l'eau du réservoir pourra développer des réactions chimiques de contact, et, il faut, à ce propos distinguer des réactions de corrosion et des réactions de dépôts. Les réactions de corrosion sont plus rares; d'autre part elles ne donneront pas une diminution du coefficient de frottement et si le matériau est régulièrement choisi elles n'auront pas une influence pratique remarquable sur le tassement.

Les réactions chimiques de dépôt sont moins rares, surtout celles concernant des sels calcaires. Ces dépôts pourront donner une soudure des éléments du matériau laquelle, même légère, augmentera la résistance au cisaillement. Mais d'autre part cette soudure provoquera une réduction de la perméabilité et, pour cette raison, la condition statique du matériau sera rendue plus mauvaise durant la vidange du réservoir.

Donc si les caractéristiques chimiques des eaux font soupçonner les réactions chimiques prévues ci-dessus, il apparaît convenable d'employer un matériau de perméabilité élevée.

La question de la dégradation de la résistance posée par M. le Rapporteur Général se réfère aux sols argileux, donc en particulier aux noyaux des barrages en terre.

M. le Rapporteur a souligné que l'hypothèse de la dégradation de la résistance au cisaillement, et en particulier la réduction jusqu'à la valeur zéro de la cohésion, sont fondées sur des éléments limités; au point de vue théorique il serait à imputer ces faits à des changements de la distribution des tensions capillaires dans le sol. Je suis de la même opinion, et en considérant la situation du noyau d'un barrage en terre, je pense que, dans une première période d'exploitation on arrivera à une distribution des tensions capillaires qui restera presque constante dans les temps successifs si l'exploitation du réservoir demeure régulière. Presque constante veut dire qu'il y aura peu d'influence à chaque remplissage ou vidange du réservoir en raison de la lenteur des variations de la distribution capillaire et de la protection contre les variations de la température et de l'humidité atmosphérique exercée par les corps du barrage et les filtres qui renferment le noyau.

Mais si le rythme des remplissages et des vidanges change notablement et surtout si l'on a de longues périodes extraordinaires, de vide ou de plein, il est possible d'arriver à des redistributions des tensions capillaires dont il faudra tirer les conséquences, si on le peut.

Les mêmes questions que j'ai évoquées pour le corps et le noyau du barrage se posent également pour la fondation en sol granuleux ou en sol argileux.

En ce qui concerne la fondation il faut considérer aussi le gonflement de l'argile.

L'on est quelquefois obligé de fonder des barrages en terre sur des argiles gonflantes et remaniées et le problème se pose de connaître la profondeur sur laquelle le phénomène du gonflement se développera.

Le phénomène intéresse la stabilité générale du barrage du point de vue de la variation des caractéristiques mécaniques des argiles gonflées et du point de vue du soulèvement de la surface de fondation. Il faut en particulier considérer que ce soulèvement n'est pas uniforme; il sera nul à l'aval du noyau où il n'y a pas d'eau, grandira à l'amont à partir du noyau et deviendra maximum au pied du talus à l'amont, et ceci en raison de la distribution des pressions exercées par le massif. Ce soulèvement ressemblera à un tassement renversé; il se développera à long terme en fonction de la lenteur de la pénétration de l'eau dans le sol d'une telle fondation.

Dans de telles conditions je crois utile de prendre des précautions. Je pense qu'il faudrait adopter la solution du noyau incliné qui diminue la largeur de la zone de fondation à l'amont, c'est-à-dire la largeur intéressée dans ce tassement renversé. Plus efficace encore pourra être l'emploi d'un tapis sur la surface de fondation du massif à l'amont du noyau, éventuellement prolongé quelque peu à l'amont du pied du barrage, tapis imperméable et non gonflant, constitué avec le matériau du noyau et qui aura pour but d'empêcher le contact de l'eau avec le sol gonflant.

Le Président :

Merci, M. le Professeur Arredi.

M. Bolognesi voudra bien maintenant nous exposer ses expériences.

M. Bolognesi (Argentine)

Since I come from a country where we are just constructing our major earth dams I can't contribute very much to a discussion dealing with the decrease of soil strength with time in slopes.

Nevertheless, as a designer engineer I would like to take a very short time to make some comments on the $c' = 0$ hypothesis because of its practical importance, with the hope of obtaining more information for its use. Being an empirical hypothesis supported by field evidence of first class researches, we must accept its partial validity, and watch out for those soils where c' may become equal to 0.

In my experience I have had at least two types of practical problems where I made use of a c' value larger than 0 and I came out with designs similar to the cross sections of dams that have been standing for many years.

I have not heard of any high dams made exclusively of clays (except of course the filters, rip-rap, etc), but there are many published cross sections of clay dams up to about 30 to 40 meters high where the $c' = 0$ hypothesis can be tested.

For example, if we assume that in each slice pore pressure may reach a value of roughly 40 to 60 per cent of the weight of the material on top of the sliding circle, we will find that a certain amount of cohesion must be introduced in stability analysis to justify the slopes of these dams.

For that reason I believe that as long as we keep the method of slices for stability analysis, we are perfectly safe in using, at least for certain soils, a c' value larger than 0. This is particularly true for clays of the CL type, (which are preferred because they are so much easier to handle during construction). These clays do not swell and they keep their shear strength provided there is enough overburden load to avoid leaking.

In high dams where clays are generally limited to the impervious core, the assumption of $c' = 0$ has no practical consequences, since most of the strength is furnished by friction along other materials. But there is a very important practical problem where the $c' = 0$ hypothesis requires a most careful analysis.

In many high dams the critical section is not the highest, that is usually placed on top of the gravel river bed, but one on the abutments. This section is very frequently placed on top of clays.

In analyzing the possibility that the whole dam may slide downstream on the stability of the upstream slope, we might or might not use the c' value obtained from tests. Since these clays have had the opportunity to swell, for at least several decades by the time the samples are taken, I think this can be presented as another case in which, even for CH clays, we may safely introduce a c' value larger than 0 into our design.

I shall now mention a fact which I have not seen previously in books or papers on earth dams. It is the intense lamination that certain soils, particularly CL and ML soils, suffer under the effect of some types of compaction equipment. This effect can be intensified by weather conditions, such as the one prevailing during hot and dry summers.

This I think is a case where the stability analysis (of these compacted laminated soils) must be made with the $c' = 0$ hypothesis assuming that no cohesion exists along horizontal planes.

Le Président :

Merci, Monsieur Bolognesi.

Je crois que dans les sujets que les membres de la tribune ont exposés, on peut trouver les éléments d'une discussion vivante. Je suis sûr qu'on a encore beaucoup à dire. Des sujets aussi passionnants que ceux qu'on a abordés exigent des interventions volontaires — dirai-je — de la part des personnalités de grande notoriété en matière de mécanique des sols. Je voudrais bien appeler à la tribune M. le Dr Bjerrum, M. le Dr Bishop et M. le Dr Skempton.

La parole est à M. le Dr Bishop.

M. Bishop (Grande-Bretagne)

There are two points to which I wish to refer. The first is the factor of safety as determined by the various methods.

The Swedish method which is shown in the General Reporter's example as giving $F = 0{\cdot}81$ is the simplified Swedish method used by Prof. Terzaghi and detailed, for example, by May and Brahtz (1936). The original Swedish method described by Fellenius (1927) included the forces between the slices, and should give the same result as the method attributed to me, since my alteration was primarily a procedural one to make numerical analysis easier. The inclusion of the forces between the slices in what we may call the Fellenius method in fact takes account of "arching", if one wishes to use this term, in that the shear forces between the slices allow for redistribution of the vertical stresses within the sliding mass.

The range of this redistribution and its effect on the factor of safety were discussed in my paper to the Conference on the Stability of Earth Slopes at Stockholm in 1954, to which Dr Trollope has referred. It appeared that in a circular arc analysis redistribution has a very minor effect on the factor of safety, a difference of only a few per cent being obtained in the case examined.

This effect was also examined by Jacobson in a discussion submitted to the same Conference. Two limiting cases of the circular arc analysis were considered, the forces acting on the soil mass being replaced either by a single vector near the middle of the arc or by two vectors acting at its ends. Even with these two extreme assumptions the factors of safety lay within a relatively narrow band.

These earlier examples make me think, therefore, that my own method and Dr Trollope's method which takes arching into account should give similar results, providing the same assumptions about pore pressure are included. It is possible that the difference which is in fact not very large may be accounted for by the methods used to deal with the forces between the slices.

It is basic that any method in which the sum of the forces acting on the sliding surface balances the weight of soil above it must lead to an almost identical result, provided that the same soil properties and the same pore pressures are used and that the factor of safety is defined in the same way. In the method I was using I followed Fellenius (1936) and Taylor (1948) and defined the factor of safety as the factor by which the two shear strength parameters c' and $\tan \varphi'$ had to be divided to bring the slope into limiting equilibrium. This definition has the advantage that it can be extended to any method of plastic analysis and to non-circular slip surfaces without modification.

I have emphasised that the redistribution of forces within the sliding mass is not of great significance in the circular arc analysis. However, in the non-circular analysis it becomes a matter of great importance. We are at present investigating the general case. Some preliminary results were published in a master's thesis by Kenney (1956) and attempts are now being made to persuade the digital electronic computer to produce similar results very much more rapidly. It is clear that, with dams on soft foundations, with stratified embankments or with narrow cores subject to high pore pressures, it is absolutely necessary to use a non-circular slip surface to obtain the most critical case.

My second point relates more directly to the question before the panel. Uncertainties in stability analysis have often appeared to lie more in the difficulty of determining the relevant soil properties than in the applied mechanics of the analysis. Where the calculated factor of safety approximates to unity in a case where a slip has occurred (as in the Lodalen slip described by Sevaldson, 1956), we tend to claim that this has proved that both the method of analysis and the soil properties are correct. This may well be true, but it is open to the philosophical objection that there are in fact two uncertainties and that they should be considered separately.

However, the General Reporter has suggested, in his Summary and Conclusions (Vol. II, p. 864) that there is at present no satisfactory theoretical basis for circular arc methods of stability analysis. This seems to me a little unjust both to himself and to those others who have been working on this problem since the time of Collin, and also to contributors to this Conference such as Kopacsy (Paper 23, Division 6) who has successfully applied the calculus of variations to the shape of the failure surface. Some work on similar lines by my colleagues Dr Gibson and Mr Morgenstern has recently been submitted for publication. This shows that for a homogeneous soil analysed in terms of effective stress, the shape of the failure surface can be predicted and that a circular arc and a logarithmic spiral result in special cases.

The General Reporter also said that the fact that water tension had not so far been taken into account invalidated most, if not all, of the comparisons published to date. I disagree with him on this point. In the case of the slip at Lodalen and in a more recent slip at Selset (Skempton and Brown, 1961) the water table was so high that the only part of the slip surface over which the pore pressure could be negative lay in the zone where a tension crack was allowed for in the analysis. This zone is generally so fissured that no account can be taken of cohesion or of the negative pore pressure.

I also disagree with the General Reporter's remarks about field pore pressure measurements. The effective stress in partly saturated soil depends both on the pore air pressure and the pore water pressure, and the pore air pressure is higher than the water pressure due to surface tension. The effective stress will therefore have a lower value if we put the correct air pressure in the equation than if we assume the air pressure is equal to the water pressure. Hence, if we apply the correct soil properties in the analysis and use the correctly measured pore water pressure (for example using a saturated fine grain ceramic disc) but ignore the air pressure, we obtain an over-estimate of the factor of safety.

Références

[1] Bishop, A. W. (1955). « The use of the slip circle in the stability analysis of slopes ». *Geotechnique*, Vol. 5, Part 1, p. 7.

[2] Fellenius, W. (1927). « Erdstatische Berechnungen mit Reibung und Kohäsion (Adhäsion) und unter Annahme kreiszylindrischer Gleitflächen ». Ernst, Berlin.
— (1936). « Calculation of the stability of earth dams ». *Trans. 2nd Congr. Large Dams*, Vol. 4, p. 445.

[3] Jakobson, B. (1954). *Discussion Proc. European Conf. on Stability of Slopes*, Vol. 3, pp. 33-35.

[4] Kenney, T. C. (1956). An examination of the method of calculating the stability of slopes. *M. Sc. Thesis, London Univ.*

[5] May, D. R. and Brahtz, J. H. A. (1936). « Proposed methods of calculating the stability of earth dams ». *Trans. 2nd Congr. Large Dams*, Vol. 4, p. 539.

[6] Sevaldson, R. A. (1956). « The slip in Lodalen, October 6, 1954 », *Geotechnique*, Vol. 6, p. 167.

[7] Skempton, A. W. and Brown, J. D. (1961). « A landslip in boulder clay at Selset, Yorkshire », *Geotechnique*, Vol. 11, pp. 280-293.

[8] Taylor, D. W. (1948). « Fundamentals of soil mechanics », John Wiley, New York.

Le Président :

Merci, M. Bishop. M. Trollope voudrait répondre à M. Bishop sur les controverses soulevées par celui-ci, peut-être ?

Le Rapporteur Général

On the first point of Dr Bishop's discussion concerning factors of safety, I don't think I have a great deal to add. I think in general I am in agreement with his statement that when we take everything into account all methods should give the right answer.

However, what I am concerned about, but as yet have no definite evidence, is that all these methods predict a unique failure condition within the slope, irrespective of the deformation of the foundation ; and it is perhaps in this respect that I would say we have not yet fully answered this problem. It may well be that there are two stages of failure when a foundation deforms and an internal redistribution of stresses occurs which does not lead to a complete collapse and, that following this redistribution the final collapse is independent of the stress redistribution introduced by the foundation. However, this is being experimented on at the present time and will be reported as soon as possible.

I would hasten to agree with the last topic he mentioned. This is the effect of pore air. It has only very recently been emphasised in a paper to this Conference by Dr Bishop and Dr Donald and in some other publications, that the importance of taking into account the pore air pressure is not only of great research significance but, as has been pointed out, can also be of practical significance.

However, I think we must leave the question of justification of existing design methods from records of actual slip failures in a stage of disagreement. I am not happy with the accuracy of measurements actually taken in the field. Even though the water-table is relatively high, there is a fringe effect which can be quite significant. In many instances the actual depth of water, the actual pore pressure, in a positive sense, is not very much greater than the component that could be developed due to a negative stress. In other words, the negative stress, even though the water-table is high, could be a significant proportion of the positive pore pressures that are taken into account.

So I have no doubt that this whole question needs very close and thorough examination. If I may, Mr Chairman, at this point I would beg leave to continue the debate.

Le Président :

Merci, M. Trollope.

Je demande à notre honorable Président, M. Skempton, d'intervenir dans la discussion, surtout sur le désaccord qui continue à exister.

M. Skempton (Grande Bretagne)

I have listened to the Panel discussion this morning with the greatest interest, and the more so as the discussion by the Panel has tended to centre on a problem with which I have been very much concerned for a considerable number of years. I refer to the question as to whether the cohesion intercept c' tends to zero with time in clay soils of various kinds ; and I got the impression from the Panel discussion that the suggestion that c' does tend to zero with time in clay soils is, first of all, considered now to be rather generally applicable and, secondly, that this idea is particularly associated with my own name.

I think therefore, Mr Chairman, that this may be an opportune moment to say a few words about this question because I am quite sure that c' does not tend to zero in all clay soils under all conditions.

This subject is perhaps more than any other dependent on field evidence, and I will therefore confine my remarks entirely to evidence of this kind.

In England we have investigated two landslips which have occurred under perfectly natural conditions and after a very long time ; that is to say we are considering natural slopes which have failed due to some very slow sequence of geological events such as slow erosion at the toe of a slip by a river or some such case. Both of these, one being in the carboniferous clays in Shropshire and the other in the Lias clays in Worcestershire, have shown quite clearly that c' was zero in the field, although there is equally not the slightest doubt that in the undisturbed material, not taken from the actual slip surface but from immediately adjoining it, c' was very different from zero, having a value of the order of 200 pounds per sq.ft.

I would like to emphasize however that in the former case we were exceptionally fortunate in being able to obtain two samples both of which included the actual slip surface itself. In this zone or surface — it was only a matter of an inch thick — the clay was greatly softened as compared with the rest of the body of the clay and, in this narrow zone, laboratory tests showed that c' was zero.

When we say that c' tends to zero what we mean is that if we were to go to a site when it was still a level field and we took samples, and we measured a certain value of c', then if a cutting or an excavation is made would the value of the cohesion intercept in the clay decrease progressively with time, or would it not ? If it does decrease progressively with time, then this is reflected necessarily in increased water contents in a potential shear zone and, finally, if a failure does occur the water content in the shear zone would be such as to be entirely consistent with the cohesion intercept being zero.

We have a number of cases in the London clay where cuttings have been made and where slips have occurred at various times after excavation. Our time scale ranges, in about twelve cases, from thirteen years to eighty years after excavation. The analysis of these failures shows fairly clearly that c' is decreasing with time. The laboratory value on undisturbed clay is of the order of 280 pounds per sq.ft. This decreases to about 200 after thirteen years and reaches values of about 50 pounds per sq.ft. after 80 years.

Once again I wish to point out that this reduction in c' is unquestionably associated with very local softening in the shear zone ; and, in three cases in the London clay, we have been fortunate in actually discovering this very narrow, highly softened zone of clay. I am certain that this is not a pre-existing zone but is a direct consequence of deformations and other effects following the opening up of the excavation.

We have also found in several natural slopes, where slides had occurred in London clay, after very many years, perhaps after many centuries, that c' is virtually zero. But these slips are shallow and are at least approaching the zone of weathering ; which is another matter altogether.

I would like to emphasize, however, that in Shropshire the shear surface in the carboniferous clays was well beneath the weathering zone.

Now, all the cases I have so far mentioned have this in common : the clays were fissured. They belong in fact to the category first defined by Terzaghi as " stiff-fissured clays ".

But not all clays are fissured or fractured. There are a number of clays which are intact and one of these is the clay at Lodalen in Norway. A slide occurred at this site in 1954 and was analysed with the utmost care. A number of careful triaxial tests were carried out and a number of pore pressure measurements were made at the site.

The analysis, published by Mr Sevaldson, showed that the factor of safety was $1 \cdot 0$ if the full value of c' was taken into account (that value being about 210 pounds per sq.ft.) but if c' was equal to zero the calculated factor of safety fell to $0 \cdot 7$; a 30 per cent error which, under the conditions of that particular investigation, is most improbable. In other words, in this case we have an instance where c' was not zero but, indeed, was either equal to or very close to its full value.

We have long been wanting a further example in the same category and, not long ago, we had the opportunity of investigating a slip which occurred in an intact clay in the Pennines, in north Yorkshire in England. This was a natural slide in an intact, non fissured clay till, or boulder clay, in a natural hillside. We know from old maps that the river had been running at this particular point, just at the toe of the slope, within a very few feet of its present position, for at least one century. Thus we were dealing with a very long term slip. The laboratory value of c' in this case was 180 pounds per sq.ft. and here again, as at Lodalen, this full value of c' is required in order to obtain a factor of safety of unity. If c' were equal to zero, the factor of safety falls to $0 \cdot 65$; that is a 35 per cent error which, again, I think is unacceptable.

The conclusion is that the full value — or very nearly the full value — of the cohesion intercept is operative. This clay is not fissured or cracked ; and the slip was fairly deep. At the same time however, the surface of the slope was also unstable. This of course is in the weathering zone, and in the weathering zone with repeated drying and wetting it is inconceivable that the cohesion intercept could remain for any length of time at its full value and, sure enough, when we analyse these surface slips, perhaps only 3 or 4 ft. deep — we find the value of c' to be about 40 pounds per sq.ft.

Therefore it seems that we must be very careful indeed in this subject, and it is not a universal rule that c' tends to zero in clays, on a long term basis. In some cases, in some clays, it does go to zero while in others it does not. And it seems to me that an explanation of these facts should start from our field observations which point very strongly in the direction that the initiation of whatever process it is which starts this decrease in c' is very likely to be associated with cracks or fissures in the clay.

I think that the fissures can lead to local over-stressing and stress concentrations which would lead to progressive failure and, in particular, to take the stress-strain curve over the peak, down to the residual or ultimate value. And it appears that the cohesion intercepts associated with those residual strengths are either zero or, at any rate, very much smaller than the values associated with the peak strengths.

Now, in an intact clay, I cannot see what mechanism in general is going to be present to take the deformations past the peak, whereas in a fissured clay I think that the local overstressing due to the cracks or fissures may be the reason why the strains can pass the peak and reach the residual value. Of course, by going over the peak, in overconsolidated clays, there is an increase in water content ; which also fits in with the field observation, since there is a very considerable increase in water content actually in the slip zone itself.

My last point refers to a matter raised by Dr Bolognesi when he suggested that c' may not fall to zero in compacted clay fills, in an earthdam core. And I also think he suggested that this might not be the case in foundations. I agree with this view, for a well compacted clay core certainly ought not to be intersected with cracks and fissures. I would say therefore that with a well compacted fill which is statistically isotropic or homogeneous, one might not expect this phenomenon to occur. But this is a point on which we are desperately needing field evidence and on which I am not aware of any satisfactory published papers.

The available evidence suggests that the softening action leading to a decrease in c' does not occur under foundations, and perhaps this is due to the fact that the average effective stresses are increasing under foundations, not decreasing as in the case of an excavation.

Le Président :

Je vous remercie, M. Skempton, pour votre intervention extrêmement intéressante.

Avant de terminer notre discussion, je voudrais donner la parole à M. Holtz qui aimerait intervenir sur le commentaire de M. Trollope en ce qui concerne la comparaison des contraintes de cisaillement du matériau des noyaux de barrage avec les résultats de laboratoire.

M. Holtz

At the bottom of page 859 of Mr Trollope's report a statement is made that " the writer cannot see that the size alone should account for the difference unless there is a distinct change in mineralogical character and/or shape of the larger pieces ". This compares laboratory tests on sandy or finer grained cohesionless soils with tests on the strength of total gravel-zone or rockfill materials. I would not disagree entirely with this, because I think we generally find that the shape of the material for a rockfill dam, and some of the pervious zones of earth dams, are different From sand and, also, the gradation is considerably different.

However, I would bring up this point : The strength is also affected by the void-ratio. On some of the large-scale shear tests we have made, we have observed extremely high values for very well-graded materials with large angular particles (Holtz and Gibbs, 1956). These materials had low void-ratios. On the other hand, strengths of rockfill materials for the Furnas Dam, Brazil, in which the particles were very flat and had sharp points, were not of the highest order, because there was very little fine material and the void-ratios were quite high. Perhaps, Dr. Casagrande might favor us during the discussion with a brief comment on this.

Références

[1] Holtz, W. C. and Gibbs, H. J. (1956). « Triaxial Shear Tests on Pervious Gravel Soils », *Journal of the Soil Mechanics and Foundations Division*, A.S.C.E., January 1956, Paper No. 867.

Le Président :

Merci, M. Holtz.

Je m'excuse de ne pas avoir respecté l'horaire prévu. Mais la discussion était très intéressante et je n'ai pas pu l'interrompre. Je vous propose de suspendre la séance pendant quinze minutes ; nous reprendrons la suite de nos travaux à midi moins le quart.

(La séance est suspendue)

Le Président :

Messieurs, je vous informe que le résumé de la discussion sera donné par M. Trollope dans les conclusions des Rapporteurs Généraux.

Je passe la parole à M. Bjerrum, le premier orateur inscrit.

M. L. Bjerrum (Norvège)

In connection with the discussion of the long-term strength of soils in natural slopes, I should like to mention some observations made in connection with the investigation of the landslide which occurred on April 14th, 1959, at Furre in the Namsen Valley in Norway. This landslide is certainly unique, as a single flake of ground with the dimensions 800 m by 300 m slid out rapidly on a long inclined surface formed by a 10 cm thick layer of normally consolidated quick clay.

The main slide failed under drained conditions, and, from an analysis of its stability in terms of effective stresses, a φ' - angle of 7° is obtained for the quick clay on the sliding surface. This value is certainly low and it amounts to only one-third of that found by triaxial tests.

Due to the large horizontal movements of the sliding masses the slip surface was exposed at the rear of the slide. This gave us an opportunity to carry out *in situ* a large-scale drained shear box test on the quick clay. These tests confirmed the low φ' - value of the quick clay given by the stability analysis.

From the study of the slide two important conclusions could be drawn. First, the virtual agreement of the φ' - values observed in the shear box tests and given by the stability analysis indicate that at failure the shear strength was mobilized simultaneously over the whole sliding surface at the main slide. This finding is of particular interest as quick clay with its small strain at failure and complete loss of strength on remoulding, is a material in which progressive failure might be expected.

The second conclusion was drawn from the observations made during the large-scale shear box tests. The shear deformations in the shear box proved to be accompanied by a considerable vertical contraction of the clay. The internal energy thus released contributes therefore to the overcoming of the shearing resistance and accounts fully for the low value of the angle of shearing resistance that was found. This effect of the " contractancy " of a soil sample subjected to shear deformations has not been observed before as it does not show up in the triaxial test.

Returning to the question of the long-term stability of slopes, we have now learned that soil compressibility may be an important factor. In the case of the slide at Furre the compressibility of the clay has increased greatly since its deposition as a result of leaching out of salt which originally was confined in the pores of the marine clay. This increase in compressibility has taken place without causing any appreciable reduction in water content and it first appears when the clay is subjected to increased shear stresses.

A shear deformation will, however, lead to a release of the " unstable " potential energy stored in the mineral structure, and this energy will contribute to the overcoming of the shearing resistance. In the case of Furre, the reduction of the angle of shearing resistance due to the contractancy effect was of the order of 15°.

A detailed description of the investigation of the landslide at Furre has been published in the June issue of *Geotechnique* by Mr J.N. Hutchinson who carried out the investigations for the Norwegian Geotechnical Institute.

Le Président :

Je remercie M. Bjerrum.

M. Fröhlich qui devait prendre la parole étant absent, je demande à M. Anagnosti de parler le premier. M. Bishop lui succèdera.

M. P. V. Anagnosti (Yougoslavie)

The main question that should be treated at the present Conference is that of shear strength and stresses in slopes and their change in the function of time. This is a proposed subject to be discussed in the 6th section.

This short note will refer to the stability of the slopes of an earth embankment during the draw down condition of a reservoir. The papers 6/7 of Mr Browsin and 6/30 of Mr Patel with Mr Maheshwani have also dealt with the subject.

In the report 6/7 a simple solution of the nonsteady filtration in an earth embankment is presented. The solution is based on the Boussinesq equation of nonsteady flow. For the analysis of the stability of an earth slope the most interesting subject is the dissipation of pore pressures during draw down if the lowering of the water level is not sudden. In this case the line of saturation in the embankment will follow the level in the reservoir at a certain distance and the shape of the flow net depends on the level in the reservoir.

The estimation of the magnitude of the initial gradient as the necessary condition for causing the filtration due to the water's own weight in the voids of the fill, is very important because of very high tensile stresses in the voids' water which can reduce or prevent the percolation of the water out of the fill.

In the report 6/30 model test measurements are presented for the same problem, i.e. for the draw down case. The special shape of a downstream filter blanket is recommended and its influence on the shape of the flow net after draw down pointed out. Some useful charts and graphs have been presented, too.

It is necessary to emphasize that the results presented in the report and the calculations may be used only in the case of a rigid incompressible embankment. Pore pressures in an earth fill embankment due to the consolidation of the fill after draw down should be investigated, applying the theory of consolidation or by introducing the triaxial test constants A and B in the well known Skempton equation. Therefore, the pore pressure due to consolidation of the fill is to be added to the pressure of the water head of the nonsteady filtration. It is not clear at the moment what the effect of the excessive pore pressure is to the filtration flow net, so that to keep on the safe side it is better to take them separately and to obtain the final pore pressure by superposition of the two pressures mentioned. It is obvious that the position of the line of saturation in the fill is very important not only for the filtration flow net determination but also for the consolidation pressure estimate.

Therefore the stability factor should be evaluated on the basis of a study of fill consolidation as well as of the flow net. The most dangerous conditions need not arise at the moment when the reservoir is empty, but all stages of the draw down case should be investigated, especially if there is question of semipervious fill, the permeability of which is of the same order as the velocity of draw down. The disregarding of all these factors in the calculations of the stability of an earth dam may cause new failures, as it is pointed out in Mr Skempton's discussion. The example of an extremely steep slope (upstream) is presented in the design of the Mattmark Dam in report 6/16, where for a moraine material the angle of internal friction was found to be of the order of 40° and the coefficient B of 5 per cent in a large scale triaxial test. The result of such favourable characteristics obtained in testing is a design of the upstream slope of a dam 100 m height ranged from 1:1,7 to 1:2,0. The cross section of the dam consists of the upstream core with rip-rap slope protection and wide downstream rockfill shell. The upstream slope of the moraine core on the average 35-40 m in thickness was checked by slip circle analysis, as stated in the report, and the obtained stability factor for rapid draw down was 1,3. It seems that a very low pore pressure has been used in the stability calculations. In the case of a very high dam with a rather steep slope for an impervious core material, very careful investigations should be undertaken to be absolutely sure of low pore pressure in the core in the case of a rapid draw down.

Le Président :

Merci, M. Anagnosti. La parole est à M. Bishop.

M. Bishop

I wish to reply to one point raised by the General Reporter, and by other members of the Conference, about the value of the factor of safety given by the stability coefficients for the particular example shown in his film.

The stability coefficients are simply a tabulated form of the Fellenius solution with the assumption that the pore pressure throughout the slope bears a constant ratio to the weight of soil above it. Where this assumption holds they correspond to the solution given by Professor Taylor's tables and have a similar degree of accuracy. However, in presenting the coefficients we realized that this assumption did not apply in a great many practical problems and we therefore developed a simple averaging technique to be used in cases where the pore pressure ratio is not constant. This represents a separate stage in the calculation and its accuracy is quite a separate problem from that of the stability coefficients themselves. With the averaging method described in our paper in *Geotechnique* an error of 2 to 3 per cent might arise in a typical steady seepage case.

In that paper two particular examples of steady seepage were given. The larger error occurred in the case of the Lodalen slip and was 7 per cent. In the case of the Selset slip the error was about 1 per cent. The steepest slope quoted in our paper was 2 to 1, because we were considering the range of natural clay slopes and earth dams rather than rock fills.

Dr Trollope, in his illustration, has a steeper slope and a very unusual flow pattern. It is probable that this requires a different averaging technique than the one suggested in our paper, particularly as the use of the pore pressure on the observed failure surface led to a factor of safety of 0·95 with extrapolated stability coefficients.

I am surprised that the factor of safety based on average pore pressures was lower than this and I would like to look at this case in more detail. Extrapolation of the coefficients may be a contributory cause and this can easily be checked against the values already available in London.

However, I think that the large error in this case arises from applying the methods in our paper outside the range for which we originally intended them. Within that range the errors appear to be less than ± 10 per cent.

Le Président :

Merci, M. Bishop.

La parole est à M. le Docteur Rao qui sera suivi par M. Bazant.

M. Rao (Inde)

In India we are now constructing a very large number of dams under various conditions and, just as I was called up here, I was reading a letter informing me that one of our earthdams, just nearing completion, breached, causing an immense loss in the valley.

On the question of earthdams, I wish to confine my remarks to two aspects which affect the stability of earth dams with time; the pore pressure and the effective shearing strength component $c' = 0$. They are the two factors which actually give a very good idea of the effect of time on the stability of earth dams.

There is unfortunately a sort of fear created on account of some misinterpretation or misunderstanding of these two factors. I am very glad Mr Skempton has clarified one of these points this morning; this is of very great value and will close the controversies. For example, there is absolutely no necessity, as regards modern dams— I am not talking about pure clay expensive structures — to be worried about the pore pressure, just as the temperature-rise in concrete dams is no longer a problem. The pore pressure is practically a similar phenomenon; just as in a mass concrete dam it is possible to control the temperature-rise by different techniques, similarly in earthdams it is possible by modern procedures to see that there is no problem of excessive pore pressure.

If the reservoir gets filled up there is a complete change in the pore pressures pattern. The pressures become coincident with a hydraulic gradient curve tending to be comean inclined straight line in course of time. The one factor, however, concerning the design of earthdams on which the International Conference of Soil Mechanics can give valuable advice, is about the pore pressures and the dissipation of the pressures due to sudden drawdown. The persistence of pressures near the upstream plane of the impervious zone in the case of a sudden drawdown is noticeable. Therefore field observations and experiments on this aspect of the dissipation of the pressures in consequence of a sudden drawdown will be of great assistance and value.

With regard to the $c' = 0$, Mr Skempton has made it quite clear that this is applicable only to the excavated slopes of overconsolidated clays with inherent fissures of slickensided conditions. But in the case of controlled dams where the compaction is normal, there is absolutely no necessity to assume that c' reaches zero or any appreciable diminishing value.

In India we have a large number of dams which have been existing for seven centuries and, in particular, we have analysed the property of these soils which have been standing for seven centuries. The dams were constructed before 1300 AD and we have found that the cohesion in effective shear strength was quite appreciable — something like 600 pounds per sq. ft. Therefore I do not think that time has any effect on the stability of earthdams. On the contrary, due to dissipation of construction pore pressure, due to the fact that fines containing the water get injected in the embankment into the soil, due to increase in structural bond in course of time and, due to the fact that the deformability of the structure gets reduced, it is only a logical conclusion to say that time will, if anything, improve the stability and make the structure more lasting even than anticipated.

As Mr Skempton pointed out, it is very useful to conduct field experiments to confirm the old saying that earthdams grow stronger with age.

Le Président :

Merci, M. Rao.

La parole est à M. Bazant.

M. Z. Bazant Jr. (Tchécoslovaquie)

The effect of time is to be found in saturated sand also. In Paper 6/5, Volume II, p. 539 I have tried to clarify the complexities of the dynamical stability of saturated sand, which were followed by some examples from Russian research. Testing dynamical stability in the Foundation Engineering Laboratory of the Technical University of Prague we have found it useful to measure, besides the thirteen unknowns mentioned in eq. (2) of Paper 6/5, another one, the settlement of surface. The relation between settlement s and time t being easy to measure, we have treated with the use of dimensionless analysis especially the case of vertical vibration of saturated sand with a horizontal surface, which is relatively a simple one (nine unknowns only). We have established the function of five unknowns, settlement s, amplitude A_y frequency $\bar{n}$, permeability k, and time t

$$f\left(\frac{s}{A_y},\ \frac{A_y\,\bar{n}}{k},\ \frac{k\,t}{A_y}\right) = 0 \qquad \ldots.(1)$$

the other unknowns : angle of friction φ, surcharge p, saturated unit weight γ_t and specific weight γ_w of water combined in dimensionless form φ, γ_t/γ_w, $p/A_y\gamma_w$ assuming constant. This function allows to measure the settlement s as function of time and it is possible to predict the limit settlement for $t \to \infty$.

The tests were performed in a glass cylinder, filled with sand and water. The cylinder was bolted to the table of the vibrating machine of UTAM Academy of Sciences, Prague, which assured vertical vibrations, the disturbing effect of horizontal vibrations being very small. Care was taken to experiment with sinusoidal vibration only, nevertheless this was not achieved at all frequencies and amplitudes.

The eigenfrequency of sand being in the range of 40 Hz, we measured from 30 Hz under. This condition limits the scale of reduction. E.g. for the scale 1:30 we model with 30 Hz frequency 1 Hz, which is the frequency of some Kaplan hydraulic turbines. If we needed to test higher frequency, we would have been obliged to use a smaller reduction of scale, and models would have been very large.

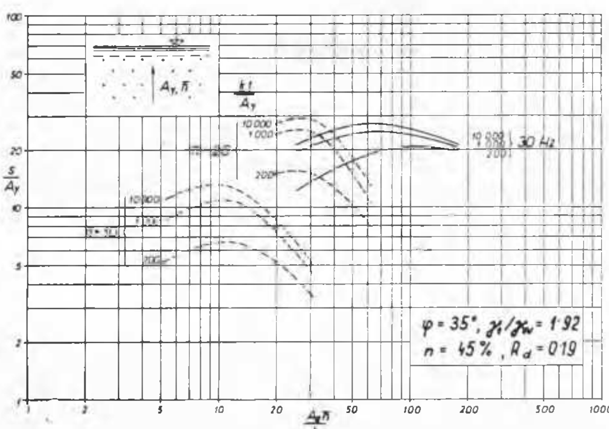

Fig. 6 Vertical vibration of saturated sand with horizontal surface.

Results of tests without surcharge are given in full lines in Fig. 6, plotted in logarithmic scale. The sand from the river Vltava at Štěchovice had $\varphi = 35°$, $(\gamma_t/\gamma_w = 1{\cdot}92$, $n =$ 45 per cent, $R_d = 0{\cdot}19$. Grains were of medium size, from 0·2 to 0·5 mm. From Fig. 6 we can for given amplitude A_y, frequency $\bar{n}$ and permeability k compute the settlement s, which is the function of time t, the limit of settlement assumed to be slightly higher than gives the curve $kt/A_y = 10{,}000$. The computed settlement is attained for very loose sand having as relative density $R_d = 0{\cdot}19$ only. Chart Fig. 6 is restricted to the settlement at sinusoidal frequencies. Pure sinusoidal vibration was produced for frequency 30 Hz and it was tested in the range of amplitudes from 50 to 260μ.

Fig. 6 gives the maximum settlement. It was obtained as the result of fifteen tests, treated in such a way that we have taken the four highest settlements, then we computed the mean m, standard deviation σ and as highest settlement was assumed the value $s = m + 2\sigma$. Rigorous mathematical treatment was not possible, as the number of tests was not sufficient and therefore we have not obtained the Gauss probability curve.

Vibration with frequency 20 Hz had on the main sinusoid an appreciable amount of vibration with frequencies three times higher and amplitudes three to five times smaller than vibrations and therefore we obtained higher settlement than with pure sinusoidal vibration only. This result is represented on Fig. 6 by dotted lines. At first glance dotted lines which differ from full ones can be misunderstood as the proof that Eq (1) is not valid. Vibration with frequency 10 Hz had smaller disturbances than with 20 Hz. At higher amplitudes disturbances were for 10 and 20 Hz also smaller.

Because many machines are producing nonsinusoidal vibrations, we can expect in some cases settlements greater than are given by Fig. 6. Highest settlements are obtained in the case resonance, when it is necessary to be prepared to cope with very quickly produced settlements about 10 times higher than Fig. 6 predicts.

Le Président :

Merci, M. Bazant.
La parole est à M. Penman.

M. Penman (Grande-Bretagne)

Mr. Chairman, Ladies and Gentlemen. I would just like to emphasize the importance of pore pressures in the stability of clay slopes which have been standing for some time and I will do this by means of an example.

An analysis of a clay slope may show that it is quite stable and an engineer may be very happy with this result. But a change in the piezometric level behind the slope may cause a certain embarrassment. In a particular case which I have been looking at, there is a clay slope that had at one time been the face of the clay pit for an old brick works. It had not been worked for about 70 years and it originally had been about 60 ft high. It is now slumped down to an irregular slope, formed by a series of slips, but these have not moved at all during the last ten years. The old brick works had been changed into a modern factory; and the factory wished to expand : it was necessary to put up a new building. The engineers who were responsible for the building made the assumption that the old clay slope which had been stable for 10 years and was 70 years old would remain stable.

Of course we know that they had a good chance of being rather wrong. The building was put up and then we had the misfortune to have an exceptionally long rainy period, which in England extended from the middle of last summer until the beginning of this one. The effect was to raise the piezometric level behind this clay slope which moved and caused some concern by piling a large amount of clay against the walls of the new building.

Just in conclusion, I would like to emphasize that we must be very very careful of our assumptions about the piezometric level and the pore water pressure conditions in any of these slopes that look to be stable.

Le Président :

Merci, M. Penman.
La parole est au Professeur Ter-Stepanian.

M. G. Ter-Stepanian (U.R.S.S.)

Further development of principles of landslide mechanism proposed by Prof. Terzaghi (1950) permitted a type of slow gravitational deformations of slopes to be distinguished. These deformations, called the depth creep of slopes occur in the landslide hearth, i.e. in that part of the slope body, where the local concentration of highly mobilized shear resistance takes place (Goldstein and Ter-Stepanian 1957).

The rate of depth creep varies with the intensity of processes, leading to landslides, such as the development of pore water pressure etc. (Ter-Stepanian 1960 *b*). In most cases these slow deformations can be considered as the preparatory phase of sliding, which turns into the failure phase when the total shear resistance along the potential surface of sliding is mobilized.

According to the local geological conditions different types of depth creep of slopes are observed. In homogeneous soils rotational depth creep of slopes develops, which leads to the rotational sliding. If the slope body is composed of inclined stratified soils with dip parallel to the slope, or if the inclined surface of unweathered rocks is covered by clayey products of weathering, another type of depth creep of slopes may occur.

The slope deformations of this type, which we call planar depth creep of slopes are also very common. When developed they lead to the planar sliding. Since the thicknesses of clay layers are usually small as compared with the heights of slopes, by computation of planar depth creep the simplifying concept of infinite slope may be assumed (Taylor 1948).

The rate of depth creep is controlled by the values of the coefficient of mobilized shear resistance, $\operatorname{tg}\theta$. This coefficient is the ratio of the shear stress, τ acting on the potential plane of sliding to the reduced effective stress $H + \sigma'$ acting on the same plane, where $H = c' \operatorname{ctg}\varphi'$ is the tension intercept or the traction resistance in terms of effective stresses :

$$\operatorname{tg}\theta = \frac{\tau}{H + \sigma'}$$

In the case of planar depth creep of an infinite slope the planes of potential sliding are parallel to the slope. The value of the coefficient of mobilized shear resistance acting on such planes at the height z from the bottom of clay layer equals

$$\operatorname{tg}\theta = \frac{q_t - \gamma \cdot z}{p_t - \gamma_w \cdot h_p - \gamma' \cdot z} \operatorname{tg}\beta \quad \dots. \quad (1)$$

where

$q_t = q_0 + \gamma h$

$p_t = \dfrac{H}{\cos^2\beta} + q_0 + \gamma h$

q_0 = charge corresponding to weight of overburden,
γ = unit weight of soil,
γ_w = unit weight of water,
γ' = submerged unit weight of soil,
h = height of clay layer,
h_p = height of piezometric level from the bottom of clay layer, and
β = angle of the slope.

Since the values of the coefficient of mobilized shear resistance increase towards the bottom of the clay layer, the lowest part of this layer reveals the higher rate of creep. On the contrary, the upper part of the clay layer may show values of mobilized shear resistance ever less, than the bond resistance of the soil, τ_0 and correspondingly, the clay may be in the state of rigidity.

The position of the boundary between zones of creep and rigidity varies according to the intensity of processes leading to landslides, such as the fluctuation of pore water pressure. This depends on the position of the piezometric level. When the height of piezometric level reaches its critical value, the coefficient of mobilized shear resistance of clay in the surface of contact with the rock bed equals to the total shear resistance of the soil, and the failure of the slope occurs.

The rate of the planar depth creep of slopes may be found using the Bingham rheological equation. It is more convenient to express it in the form

$$\operatorname{tg}\theta = \operatorname{tg}\theta_0 + \frac{\dot{\varepsilon}}{\lambda}$$

where $\operatorname{tg}\theta_0 = \dfrac{\tau_0}{H + \sigma'}$ is the coefficient of bond shear resistance of soil.

$\lambda = \dfrac{H + \sigma'}{\eta}$ is the coefficient of liquidity,

η = coefficient of viscosity of soil, and
$\dot{\varepsilon}$ = rate of shear deformations.

The rate of planar depth creep of slopes is

$$v = \lambda \cos\beta \left\{ \left(\frac{\gamma}{\gamma'} \operatorname{tg}\beta - \operatorname{tg}\theta_0 \right) h_r + \frac{\operatorname{tg}\beta}{\gamma'} \left[\frac{\gamma}{\gamma'} \left(p_t - \gamma_w h_p \right) - q_t \right] \ln \left(1 - \frac{\gamma' h_r}{p_t - \gamma_w h_p} \right) \right\} \quad \dots. \quad (2)$$

where h_r is height of boundary of rigidity zone.

Analysis of equations (1) and (2) enables to solve different practical problems, such as :

1. Determination of the critical height of piezometric level; this may be necessary for prediction of failure of the slope.
2. Determination of the factor of safety of an existing slope.
3. Determination of the height of allowable piezometric level, which corresponds to the given factor of safety of slopes; this may be used for calculation of drainage system on slopes.
4. Estimation of the relation between the height of piezometric level and the rate of planar depth creep of slopes, etc.

The validity of such conclusions may be proved by means of field observations on landslides. These observations are very much recommended. Some results of such observations were published recently (Ter-Stepanian 1960 *a*).

Références :

[1] Goldstein, M. and Ter-Stepanian, G. (1957). « The Long-term Strength of Clays and Depth Creep of Slopes ». *Proc. 4th International Conference on Soil Mechanics and Foundation Engineering*, London, Vol. 2, pp. 311-314.

[2] Taylor, D. W. (1948). *Fundamentals of Soil Mechanics.* J. Wiley & Sons, New York.

[3] Ter-Stepanian, G. (1960 *a*). « Measurement of Depth Creep of Slopes ». *International Society of Soil Mechanics and Foundation Engineering. Regional Conference* (Asia), New Delhi, India, Subj. I(*c*), pap. (*iii*).

[4] — (1960 *b*). « Relation between Pore Pressure and Rate of Depth Creep of Slopes ». *Ibid.*, Subj. I (*c*), pap. (*iv*).

[5] Terzaghi, K. (1950). « Mechanism of Landslides ». *Geological Society of America, Engineering Geology.* Berkey Vol., November, pp. 83-123.

Le Président :

Merci, Monsieur le Professeur.
La parole est à M. Šuklje.

M. Šuklje (Yougoslavie)

Monsieur le Rapporteur Général a proposé que la discussion porte sur l'existence des contraintes de traction dans le sol ainsi que sur la résistance à la traction des sols. A ce sujet, je prendrai la liberté de présenter quelques données résultant de l'analyse statique d'une rupture de l'excavation du canal Tisza-Danube, en Yougoslavie, portant sur une longueur de 800 mètres à peu près, la profondeur de l'excavation étant de 20 mètres environ. Cette rupture était prévue, mais les puits déchargeant la pression de l'eau artésienne, proposés pour prévenir la rupture, n'étaient pas encore réalisés.

La base argileuse (CH – CI) de l'excavation, susjacente au récipient sableux (SF_s – SP – SU) de l'eau artésienne, était analysée comme un système statique où les forces de gravité étaient équilibrées par des forces de pression artésienne. Ce système statique était supposé être chargé aux surfaces limites par des moments d'encastrement ainsi que par la pression du terrain environnant, les deux charges étant supposées être en équilibre indépendant et correspondant à l'hypothèse que les contraintes de traction aux limites supérieure et inférieure de la base cohérente étaient égales.

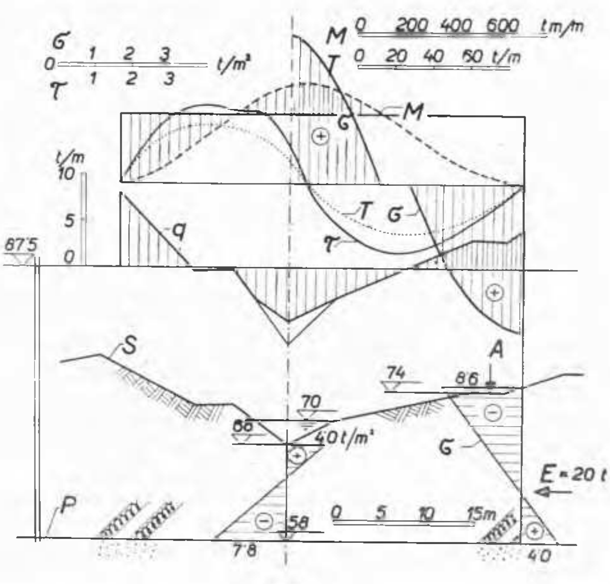

Fig. 7

La Fig. 7 montre la coupe de l'excavation à l'instant de la rupture, à l'endroit où la première des sources artésiennes précédant la rupture est apparue. La position de la source (A) tombe dans le domaine des contraintes de traction maximum. Celles-ci s'élèvent jusqu'à $\sigma_m = 4$ t/m², et les contraintes de cisaillement moyennes dans la direction verticale jusqu'à $\tau_{m\ med} = 1{,}9$ t/m². L'analyse de la coupe où le premier grand renard est apparu, a donné un résultat semblable : $\sigma_m = 3$ t/m², $\tau_{m\ med} = 2{,}4$ t/m², mais avec l'apparition du renard dans le domaine des contraintes de cisaillement maximum (L. Šuklje, 1961).

La Fig. 8 montre la résistance à la compression simple des échantillons cylindriques pris dans les différents sondages le long du canal, confrontée aux valeurs correspondant à la

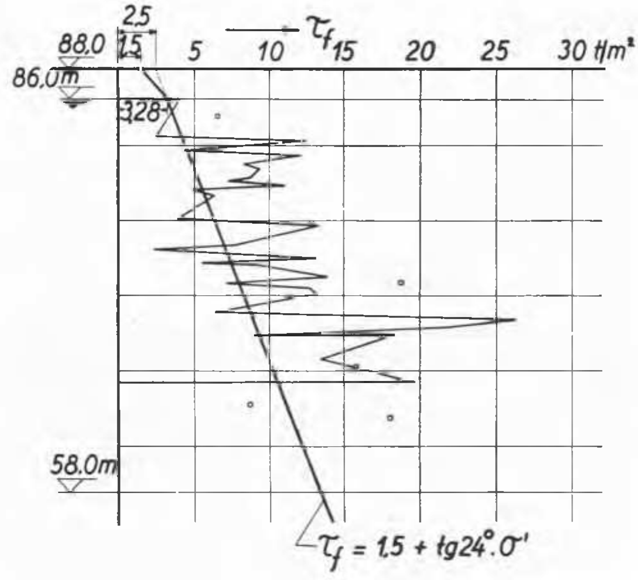

Fig. 8

loi $\tau_f = 1{,}5 + \mathrm{tg}\ 24° \sigma'$ (t/m²) qui se dégage des essais de cisaillement direct faits dans les appareils à torsion.

Sur la Fig. 9 nous avons présenté les cercles de Mohr des états de contraintes initiaux (avant l'excavation) et finaux (à la rupture) pour les élévations 68 et 58 aux endroits de $\sigma_m = 4$ t/m² (voir Fig. 7) ainsi que les cercles de Mohr pour le point avec $\tau_m = 1{,}5$ $\tau_{m\ med} = 2{,}9$ t/m². Les tangentes aux cercles de Mohr des états de contraintes finaux passant par les points de la résistance initiale correspondante forment avec l'axe σ des angles compris entre 18° 45′ et 20° 15′ tandis que les essais de cisaillement direct faits sur des échantillons préconsolidés et déchargés — peu nombreux d'ailleurs — ont donné un angle de 17°.

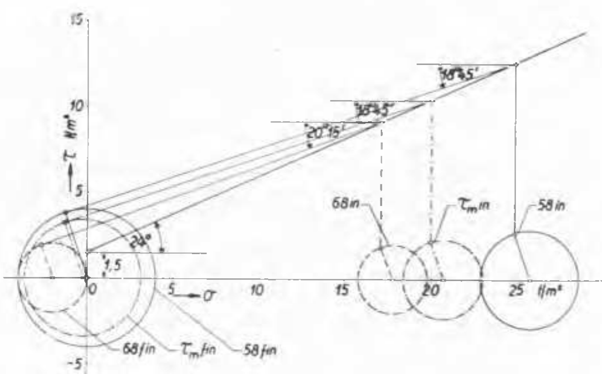

Fig. 9

Les résultats présentés ci-dessus ainsi que les autres analyses de la fouille après rupture montrent que les contraintes de traction interviennent considérablement dans la stabilité des excavations soumises aux surpressions hydrauliques. Les conditions de rupture dépendent aussi bien de la résistance au cisaillement due aux contraintes effectives que de la résistance à la traction, et l'anisotropie du sol ainsi que les effets du fluage et de la rupture progressive y jouent un rôle important.

Référence :

[1] Šuklje, L. (1961). « Facteurs gouvernant les gradients critiques dans la base d'excavation ». *AIRH, Neuvième Assemblée Générale*, Belgrade 1961, Rapport II/23.

Le Président :

Merci, M. Šuklje.
La parole est à M. Osterman.

M. J. Osterman (Suède)

As mentioned by Dr Bishop, the Swedish slice method in international usage refers to a simplification. During 1916-1929 the reactions between the individual slices were often taken into account in the calculations. Using a graphical solution, W. Fellenius (1929) discussed in a paper the error involved in the simplification (horizontal instead of inclined forces in sand), finding some 12 per cent error in some rather extreme cases.

The importance of measuring creep in slopes was pointed out in the panel discussion. I would like to mention that an interpretation of the results demands a thorough knowledge of the climatic influences on slope travel.

With reference to the contribution by Prof. Skempton, I wish to mention that a dry crust, from a prehistoric slide, was found in a Swedish lake many years ago, the pieces of the crust still being stiff. Thus, the cohesion intercept c cannot always be neglected in an overconsolidated clay. If, however, water and dissolved chemicals by a pressure gradient are forced through an overconsolidated clay, softening occurs, which has also been indicated in some cases in Swedish clays.

In the discussion, the " effective stress philosophy " was mentioned, but the method of effective stresses is often difficult to use.

When investigating a landslide in normally consolidated clay, for instance, the cohesion method gives a safety factor of about one. Assuming normal stress σ_n, and employing a reasonable pore water pressure u, deduced from inquiries and measurements in the vicinity of the slide area and applying the shear strength equation

$$\tau_f = c + (\sigma_n - u) \tan \varphi$$

one can calculate an angle of apparent friction utilized, say 16-17°, which is then found to fit an undrained test, thus φ_{cu}.

If using an angle, of say, $\varphi' = 22°$, which may correspond to a drained test or to an effective stress interpretation of an undrained test, one must introduce into the equation an excess pore pressure u_f arising at failure.

This excess pressure is not known. This dilemma seems to me to be one of the main reasons for being cautious when venturing the effective stress method in Sweden and other countries with saturated soft clays.

Le Président :

Merci, M. Osterman.
La parole est à M. Little.

M. Little (Grande Bretagne)

I have been interested to hear of Mr Osterman's difficulties, because I have come across difficulties of my own in doing stability analysis.

In the course of doing a large number of stability analyses on earth dams of various kinds, we have used different methods of stability analysis, in the past total stress methods but nowadays effective stress methods.

Unfortunately, in order to use an effective stress method it is necessary to decide on the pore pressure to be used ; and this is where my difficulty comes in. I refer particularly to the case of the saturated upstream slope of an earth dam. Time does not permit me to enlarge upon the methods that we use, but it is sufficient to say that we have to make certain assumptions and insert them in the calculations in order to arrive at a factor of safety. We would, therefore, very much like to have observations on pore pressures so as to be able to check these assumptions.

The amount of information that has been published so far is rather meagre ; some of it is not really applicable, either because the slopes are not saturated, or the material is of sufficiently high permeability to permit rapid dissipation of the pore pressures.

The material in which I am particularly interested is that where the rate of drainage is slow and presumably rather large pore pressures are left. In fact, some measurements have been made in Great Britain and in Canada, which indicate that quite high residual pore pressures are left after drawdown. I would therefore plead for many more results to be published.

I appreciate that not many engineers can persuade employers to empty their reservoirs for the benefit of other engineers and of other employers, but in the case of hydro-electric schemes, where drawdown is fairly frequent, I feel that there must be some information, probably on files of various organizations, which could be made available.

On the matter of c' — in our methods of analysis when dealing with over-consolidated clays, we put $c' = 0$ and are usually satisfied so long as the factor of safety with this condition is greater than unity.

Le Président :

Merci, M. Little.
La parole est à M. Marsland.

M. A. Marsland (Grande Bretagne)

Low embankments (10-30 ft. high) used for flood protection receive much less attention than high dams, but they can present the soil engineer with equally difficult problems. The stability of such banks varies appreciably with both time and external conditions. I will illustrate this by the case of a flood bank constructed with soft sensitive clay dug from the adjacent marsh. In England the in-situ strength of these clays is from 250-350 lb/sq ft. while the fully remoulded strength is only about one quarter of these values.

Excavation and placing disturbs the clay and slips during construction are common. These slips can be adequately investigated on a $\varphi = 0$ basis. After construction the strength of the clay within the bank increases due to the consolidation produced by its own weight and suction forces resulting from its elevated position above the average water table and surface drying. The shear strength increase follows the effective stress envelope for the normally consolidated, partially remoulded soil for which the effective stress parameters are $\varphi' = 25°$ and $c' = 0$. Shear strengths of the order of 800 lb/sq ft. are reached in a few months and we then have a stable bank capable of withstanding a reasonable amount of overtopping during flood conditions, since the clay is intact and not quickly softened. In a humid climate such as that in England, this favourable state of affairs may last several years.

Progressive surface drying resulting mainly from the presence of vegetation causes further shrinkage, which in time leads to the development of cracks and to a highly-fissured zone several feet deep. The clay in this zone is in the form of rectangular shaped nodules and behaves in a similar manner to an interlocking gravel. When a flood level reaches this highly-fissured zone, appreciable seepage occurs and the combination of softening and seepage forces can lead to a slip on the downward slope of the bank and to the rapid breaching of the embankment, prior to, or during the initial stages of overtopping (Reference : Cooling and Marsland, 1953).

In order to study the shear properties of the fissured clay under flood conditions, in situ field tests were made in which a narrow trench was dug round pedestals of soil 3 ft. long by 2 ft. wide by 2 ft. deep. The top and sides of the pedestals were supported by an open-bordered box and a waterproof surround was placed round the outer sides of the trench to facilitate flooding of the pedestal of soil. In all the tests the pedestal of clay was saturated for at least half an hour before being sheared across its base. Some tests were made in which the pedestal was submerged during the shearing process, while in others it was allowed to drain just prior to shearing. In one of the tests an additional dead load of 600 lb. was placed on the top of the pedestal. Piezometer points were inserted to measure the pore-water pressure in the fissures along the shear plane. These tests gave effective stress parameters between $c' = 20$ lb/sq ft., $\varphi' = 25°$ and $c' = 30$ lb/sq ft., $\Phi' = 30°$. More prolonged wetting, such as occurs in natural slopes, may reduce the effective stress parameters to even lower values. It should be noted that the effective stress envelope for the highly-fissured soil is a little higher than the normally consolidated envelope as a result of the other consolidation which has taken place, but is well below that for an intact soil subjected to the same stress history. Had the clay not fissured it would have retained a c' of at least 200 lb/sq ft. under fully softened conditions, and thus the low c' obtained is primarily due to the fissuring of the clay. In addition to having a low c' the fissured soil responds very rapidly to changes in seepage forces.

This example emphasizes the need to consider the long-term changes in the shear properties of the soil which can occur in earth banks, as well as the changes in pore pressures which occur during floods.

Référence :

[1] COOLING, L. F., MARSLAND, A. (1953). « Soil Mechanics Studies of failures in the sea defence banks of Essex and Kent ». *Proceedings of a conference on the north sea floods*, 31st January, 1st February, 1953; *Institution of Civil Engineers*.

Le Président :

Merci, M. Marsland.
La parole est au Prof. Naylor.

M. NAYLOR (Grande Bretagne)

The normal conception of the factor of safety is based upon the stability of the whole of the material above the slip surface. In the case of a bank of purely frictional material the circular slip surface degenerates into a straight slip surface parallel to the downstream slope with a factor of safety equal to $\frac{\tan \varphi}{\tan \theta}$, where φ is the angle of internal friction and θ is the slope of the bank face. This gives neither the true lowest factor of safety nor is it the failure surface in the case of a frictional bank retaining a weak or low frictional fill. This surface would in fact pass through the soft fill, Fig. 10. Thus the factor of safety as normally conceived and derived is no longer valid. This is not surprising with a slip surface which falls into two contrasting parts : one convex in the bank and the other concave in the fill.

In cases like this where a soft fill exerts an active pressure on a passive resisting bank it is logical to regard the bank as the structure and to base the factor of safety on it alone. It is suggested that it should be the ratio of the density of fill necessary to cause failure to its actual density, the " c " and " φ " properties remaining unchanged.

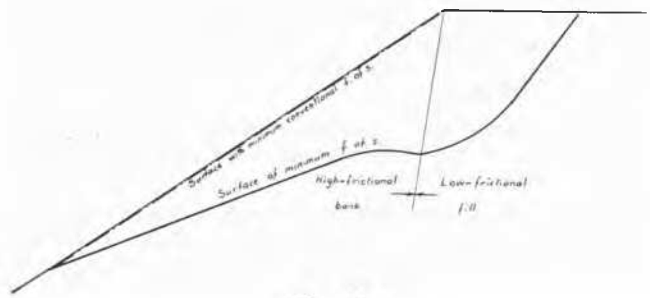

Fig. 10

It would appear that the normal definition of factor of safety should be confined to cases where the failure surface is concave throughout. With a bank retaining fill, the above definition must be used. To avoid confusion it could be called the " load factor ".

Nonveiller (1957) first mentioned this matter [1] and it is dealt with by myself and colleagues in greater detail in the current *Geotechnique* (June 1961).

Référence :

[1] NONVEILLER : " Stability of non-homogeneous dams ". *Hydrotechnical Institute of Yugoslavia.*

Le Président :

Merci, M. Naylor. La parole est à M. Lewis.

M. LEWIS (Australie)

I wish to comment also on this value of c' especially in relation to sudden drawdown on some earthdams that have been built in Australia. I would like to say that I was delighted to hear the remarks of professor Skempton, because I feel that in a way he has this morning prevented a mental land-slide towards the $c' = 0$. I had gathered the impression that every one was going that way.

We have built in the western part of Australia in the last 15 years a number of dams between 150 ft. and 200 ft. high, on compressible foundations and built also of residual granitic soils with a plasticity index from 25 to 40. They have an angle of friction of about 15 to 20° only and when we calculate the stability for the Slices Method, we find that if c' is 0, the factor of safety of these dams is about .7 to ·8. If c' is 3 lb p.s.i. the factor is about 1·1; and if c' is 6 lb p.s.i., the factor goes up to about 1·4.

I certainly believe that c' will never reach 0 in this class of material. So, I would disagree with any tendency towards a general statement that c' will tend to 0 for all materials. I do think, however, that we should look further into this problem. Mr Holtz gave us examples yesterday and today of comparisons of strength of existing dams by taking undisturbed samples.

I feel that much more should be done in this regard. It would be of great value for the next Conference if we could produce data from dams of some considerable age to compare what the computed factor of safety is and compare it against the performance record. In think this is all I have time to mention.

Le Président :

Merci, M. Lewis.
La parole est à M. Escario.

M. Ventura Escario (Espagne)

Prof. Trollope shows himself somewhat surprised in his General Report with respect to the results presented in the above-mentioned Report.

The three points which induce him to have some doubts are the following ones :

1. By applying the conventional method of the slices for estimating the slope stability to the case in question, negative safety factors are obtained.

2. The differences among the safety factors obtained with the three methods applied are very large.

3. In his opinion, the values are in reverse order to those previously established by other investigators.

In connection with the first point, we wish to make it clear that it is not necessary to choose pore pressures corresponding to a very extreme set of assumptions, in order to obtain negative safety factors, when applying the conventional method. In the case of Fig. 11, for instance, of a semi-infinite body with water level on the surface of the ground, to which a uniform pressure is applied, when estimating the resisting moment corresponding to short term stability we have

$$M_r = R^2 \int_{-\frac{\pi}{2}}^{+\frac{\pi}{2}} [(p + \gamma' z)\cos^2\alpha - u]\mathrm{tg}\varphi' d\alpha$$

where γ' is the submerged specific gravity of the ground; taking into account that

$$z = R\cos\alpha$$
$$u = p$$

we obtain

$$M_r = R^2 \,\mathrm{tg}\,\varphi' \left[\frac{4}{3}\gamma' R - \frac{\pi}{2} p\right]$$

Consequently, for values

$$p > \frac{8}{3\pi}\gamma' R$$

a safety factor of $-\infty$ is obtained, because the overturning moment is zero.

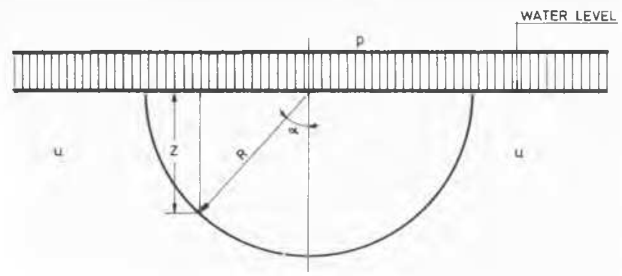

Fig. 11

Obviously, this result is entirely absurd, for under these conditions the ground is stable.

As to the second point, we have checked a few circles using the three methods; the corresponding calculations have been carried out by an Assistant Engineer different to the one who worked them out the first time and the values obtained are given in the following Table.

TABLE I

Values of the Safety Factor

	Location of the Circle									
	$X_0 = 5$, $Y_0 = 0$	$X_B = 10$	$X_0 = 0$, $Y_0 = 15$	$X_B = 14{,}50$	$X_0 = 0$, $Y_0 = 5$	$X_B = 9$	$X_0 = 5$, $Y_0 = 0$	$X_B = 15$	$X_0 = 0$, $Y_0 = 5$	$X_B = 10$
	Orig. calc.	Checkg.	Orig. calc.	Checkg.	Orig. calc.	Checkg.	Orig. calc.	Checkg.	Orig. calc.	Checkg.
Conventional Method	— 0,49	— 0,49					0,18	0,16	0,18	0,05
Considering effect of pore press. on sides of slices..........	1,54	1,64					1,47	1,45	0,87	0,76
Bishop's Method....			1,30	1,29	1,60	1,52				

X_0, Y_0 are the abcissa and ordinate of the centre of the circle and X_B is the abcissa of the point of intersection of the circle with the ground surface under the dyke.

Some of the circles chosen do not correspond to minimum safety factor circles and therefore the corresponding factors of safety do not coincide with those given for the same center in the Report.

As it may be observed, the results of the checking do agree rather satisfactorily with those previously calculated; the percentage difference is only important in one case where the safety factor is very small, arising from the difference between two figures of the same order of magnitude.

One thing we want to point out is that the calculations given in the Report and in the checking were carried out for a height of counterdyke of 4,00 meters in the conventional method as well as in the second method; Bishop's method was applied with a height of counterdyke of 3,75 m. This fact still increases the difference between this method and the two other ones.

The expressions used in the calculations for each of the methods are given at the end of this discussion, following the wishes of the General Reporter.

So far as the third point is concerned, it surprises us that Prof. Trollope should assert that the results are in reverse order to those noted in previous studies. On the contrary Bishop's method gives higher safety factor values than those given by the other two methods, as it was expected.

Computation of the safety factors.

The calculation of the safety factors has been performed in the following way :

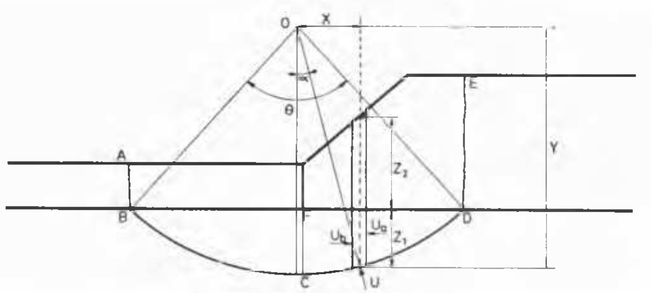

Fig. 12

Let us suppose a slip circle as the one shown in Fig. 12. The pore pressures due to the fact that the soil is submerged have been taken into account by taking for the same a specific gravity of $\gamma' = 0{,}6$, and therefore from now on we will work as if they did not exist. If for a moment we assume that the pore pressures due to the application of the load have dissipated, and we call

M_0 = Overturning moment of the solid ABCDE in relation to the center
M_r = Resisting moment of the shear stresses at BCD with respect to the center
s_x = Unit shear strength of the element of circle corresponding to the abscissa point X
c' = Effective unit cohesion
φ' = Effective angle of internal friction
F_c = Factor of safety with dissipated excess pore pressure

and with the remainder notations shown in the figure, we have

$$s_x = c' + (\gamma_1 z_1 + \gamma_2 z_2) \cos^2\alpha \operatorname{tg}\varphi'$$

$$F_0 = \frac{M_r}{M_0} = \frac{\theta R^2 c'}{M_0} + \frac{\gamma_1 I_1 + \gamma_2 I_2}{M_0} \operatorname{tg}\varphi' = (\lambda N_c + N_f) \operatorname{tg}\varphi'$$

where :

$$I_1 = \int_B^D I Z_1 \, Y dx \qquad I_2 = \int_B^D Z_2 \, Y dx$$

$$\lambda = \frac{c'}{\operatorname{tg}\varphi'} \quad N_c = \frac{\theta R^2}{M_0} = \frac{M_c}{M_0} \quad N_f = \frac{\gamma_1 I_1 + \gamma_2 I_2}{M_0} = \frac{M_f}{M_0}$$

For obtaining the aforesaid expressions it has been assumed that there are no stresses whatsoever on the sides of the slices. Neglecting the effective pressures acting on both sides of the slices, and calling U_a, U_b and U the total pore pressures (the hydrostatic one not included) acting on the sides and base of the slice, respectively, as shown in Fig. 12, it may be shown that we must add the expression $\Sigma[(U_b - U_a) X - UR]$ tg φ' to the resisting moment M_r.

If we only take into account the pore pressure on the base of the slice, the expression to be added up is :

$$- \Sigma \, UR \operatorname{tg}\varphi'$$

Therefore we have the following expressions giving the factors of safety F_1 and F_2, the former considering only the pore pressure on the base of the slice and the latter also considering the ones corresponding to the sides :

$$F_1 = (\lambda N_c + N_f - N_u) \operatorname{tg}\varphi'$$
$$F_2 = (\lambda N_c + N_f - N_u + N'_u) \operatorname{tg}\varphi'$$

where

$$N_u = \frac{M_u}{M_0} = \frac{\Sigma UR}{M_0}$$

$$N'_u = \frac{M'_u}{M_0} = \frac{\Sigma(U_b - U_a)X}{M_0}$$

Le Président :

Merci, M. Escario.

Nous avons encore deux rapports, mais ils n'ont pas trait aux sujets inscrits à l'ordre du jour. Aussi, selon les recommandations reçues, ils seront publiés dans le volume n° III.

Mesdames, Messieurs, il appartient à M. Trollope de faire le résumé de la discussion que nous avons eue ce matin. Je veux simplement constater, après cette discussion extrêmement intéressante, qu'il existe encore des hypothèses qu'on n'a pas définies. Nous devrons encore réfléchir sur le sujet. Mais nous sommes tout de même très optimistes, car nous voyons la mécanique des sols se développer à une telle allure que M. Bishop est obligé de demander aux machines électroniques de travailler plus vite !

Je vous remercie pour votre attention et je lève la séance.

(La séance est levée à une heure dix.)

Interventions écrites / Written Contributions

M. J. Brinch Hansen (Danemark)

Prof. Rodriguez develops (6/34) by means of Kötter's equation, formulae for the internal forces in a circle of rupture. I did the same in 1953 [1], but my formulae are simpler and have been simplified further in 1957 [2], where extensive tables are also given for their practical use.

Whereas this is a matter of convenience only, a serious error is introduced by assuming $\sigma = 0$ in the rupture-line at the ground surface. This is correct only for cohesionless earth (and an unloaded ground surface).

Prof. Rodriguez rightly states that this boundary condition is deficient, and that a new boundary condition is desired, preferably so that the calculation based on Kötter's equation gives the same result as the Swedish method.

This is precisely what I did in 1953 ([1], p. 61-66).

The pertinent boundary condition is, using Prof. Rodriguez's symbols, and referring specifically to the case in his Fig. 1 :

$$\sigma = c \frac{\cos\varphi \cos(\beta - \varphi)}{\sin\beta}$$

Références :

[1] Brinch Hansen J. (1953). *Earth pressure calculation*, Copenhagen.

[2] — (1957). « The internal forces in a circle of rupture », *The Danish Geotechnical Institute, Bulletin* No. 2, Copenhagen.

M. R. Coulomb (France)

Le Département de la Seine construit actuellement à l'est de Troyes, dans le département de l'Aube, le réservoir Seine.

La cuvette, de 205 millions de m³, sera ceinturée par trois petites digues en terre (dont deux sont en construction) et par une grande digue de 2 800 m de longueur, de 24 m de hauteur maximum, représentant 1 800 000 m³ de remblais.

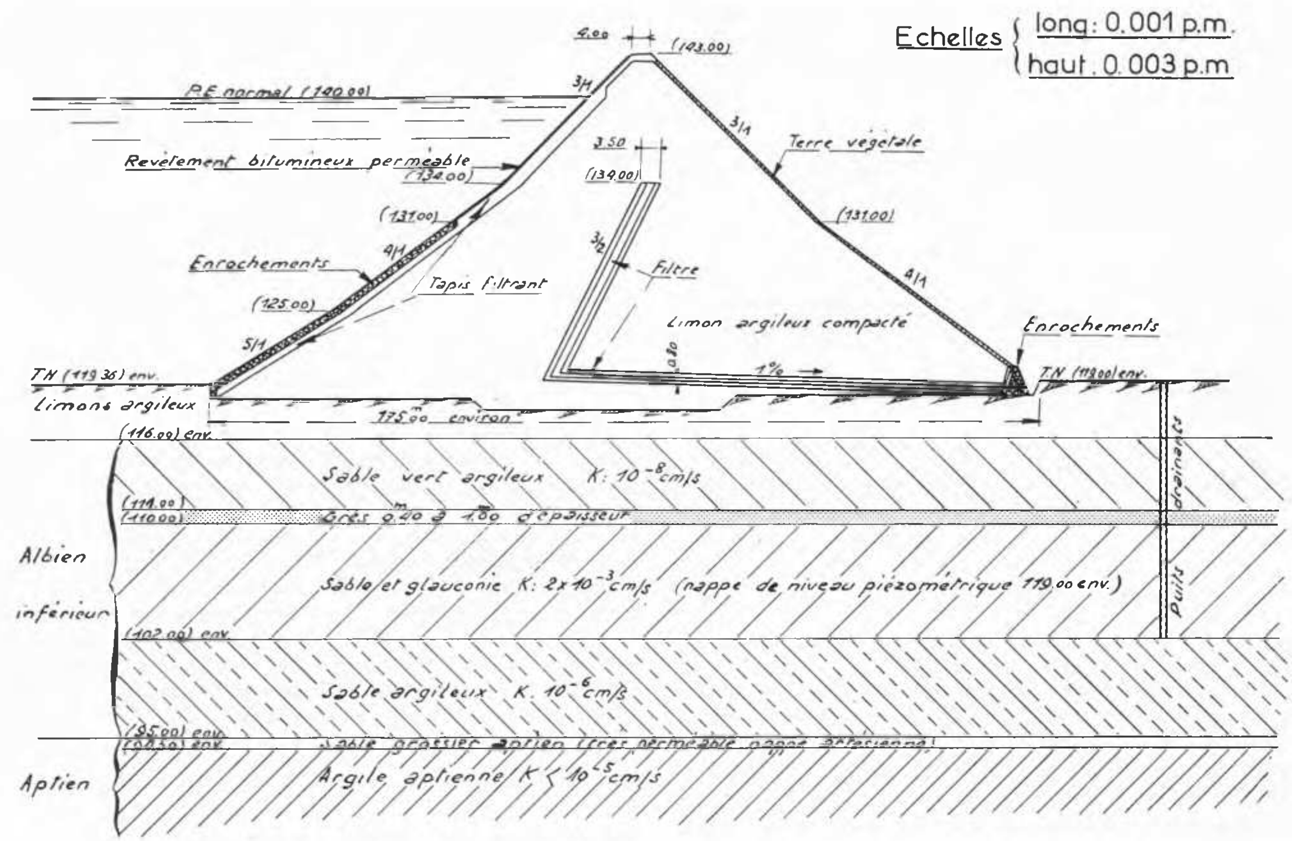

Fig. 13 Profil en travers schématique de la digue et du sous-sol.

Cette digue, en limon argileux, appelée « digue de la Morge » comportera un filtre central incliné vers l'amont et draîné par un tapis filtrant débouchant dans le fossé de pied aval (voir profil en travers). Le rapport 6/30 présenté par MM. Patel et Maheshwari montre l'intérêt de ce type de filtre, dont l'application à la digue de la Morge avait été envisagée il y a plusieurs années.

La digue repose sur une couche de limons argileux de 3 m d'épaisseur. Ceux-ci surmontent une couche de 5 m de sables verts de l'albien, qui sont ici très argileux et imperméables. Au-dessous, sur 8 mètres d'épaisseur, on trouve du sable glauconieux, beaucoup plus perméable, renfermant une nappe dont le niveau piézométrique est voisin du terrain naturel. Il est possible que cette couche affleure à divers endroits de la retenue et on ne peut écarter l'hypothèse qu'elle se mette en charge. En-dessous, on peut considérer le substratum comme pratiquement imperméable sur une épaisseur suffisante pour ne pas tenir compte de la couche de sable grossier aptien, située à 25 m de profondeur.

La digue de la Morge ne comporte aucun écran étanche rejoignant ce substratum imperméable : les pertes à travers les limons et sables argileux de couverture seront négligeables. Par contre, la nappe perméable des sables glauconieux qui doit normalement se draîner vers une rivière voisine pourrait (si les résistances en aval étaient trop fortes) entraîner des désordres lors du remplissage, la charge piézométrique sous le barrage pouvant alors atteindre 140,00 environ. Aussi avons-nous prévu l'exécution de puits de décharge en aval pour draîner ces sables glauconieux.

Afin d'étudier l'efficacité de ces puits et du filtre, nous avons demandé au Service des Etudes Hydrauliques d'Electricité de France (M. Cavaillé-Coll) d'effectuer une étude par analogie électrique.

Sans entrer dans le détail de cette étude, effectuée avec un réseau de résistances amovibles, nous résumerons les hypothèses faites et les résultats obtenus.

1. *Hypothèses* :

(a) Le niveau du réservoir en écoulement permanent ou avant la vidange a été pris égal à 141 (niveau exceptionnel).

(b) La position de la surface libre est déterminée par approximation. Elle doit satisfaire à deux conditions :

1. Pression égale à la pression atmosphérique.
2. En régime permanent elle n'est traversée par aucun débit.

On évite ainsi l'erreur faite par MM. Patel et Maheshwari qui utilisent la formule de Kozeny.

(c) Le corps de la digue mis en place par couches horizontales sera sûrement anisotrope : on a supposé des coefficients de perméabilité horizontale $K_h = 10^{-7}$ cm/s et verticale $K_v = 10^{-8}$ cm/s. La perméabilité du filtre a été prise égale à 10^{-4} cm/s (ce qui suppose un colmatage partiel). La perméabilité des limons et des sables argileux a été prise égale à 10^{-8} cm/s. Pour le sable glauconieux, faute de pouvoir réaliser des perméabilités plus grandes, on a dû prendre $K_v = 10^{-5}$ cm/s, $K_h = 10^{-4}$ cm/s.

Avec ces valeurs, cette couche se comporte comme un tube de courant et des valeurs plus fortes ne changeraient pas la forme de l'écoulement.

(d) On a admis qu'à 14 m à l'aval du fossé de pied aval, le système de puits draînants assurait un potentiel correspondant à la cote 118,83 m.

2. *Etude de l'écoulement permanent* : Pour tenir compte d'affleurements possibles dans la retenue, on a supposé que le modèle était alimenté à 58 m du pied amont de la digue à un potentiel égal à 140, hypothèse pessimiste, qui augmentait la pression des pores sous la digue. Grâce au filtre, la ligne phréatique est abaissée et le talus aval est pratiquement sec; le filtre incliné n'est pas saturé et sa face amont est une surface de suintement, tandis que le tapis horizontal est légèrement en charge (avec la valeur pessimiste adoptée pour sa perméabilité). Cependant la surface libre *ne s'arrête pas dans le filtre incliné*, mais passe au-dessus, une partie du débit s'écoulant dans la partie aval de la digue (malgré un filtre élevé jusqu'à la cote 134, alors que dans les premières études, il s'arrêtait à 133). Etant donné la faiblesse du débit non absorbé par le filtre incliné, on ne peut pas supposer le terrain saturé à l'aval; cette partie aval du domaine d'écoulement est restée indéterminée : il est probable que le débit correspondant diffusera dans le terrain sans le saturer.

3. *Etude de la vidange* : le caractère argileux du matériau constituant la digue rend difficile l'estimation de sa porosité utile, et ne permet pas de considérer les limites et la *structure* du milieu comme des constantes en régime lentement variable. On retrouve les difficultés signalées par le Rapporteur Général, M. Trollope (Vol. II, p. 863).

Le but étant de vérifier la stabilité de la digue dans l'hypothèse la plus défavorable, on a étudié seulement la vidange *instantanée*.

Dans le cas d'une vidange instantanée, l'état final de l'écoulement ne dépend que de la position initiale de la surface libre et des conditions aux limites du domaine en fin de vidange.

Dans l'étude du régime permanent, on avait admis l'éventualité d'une alimentation de la couche des sables glauconieux près de la digue. Mais il était prudent d'envisager ici une alimentation éloignée correspondant au maintien, malgré la vidange, d'une cote piézométrique élevée dans les sables glauconieux au pied amont de la digue. Deux essais ont été faits pour les potentiels $h = 122$ et $h = 126$ (hypothèses pessimistes). La forme de l'écoulement obtenu a été contrôlée sur un modèle sommaire en papier conducteur qui a l'avantage sur le réseau maillé d'avoir une structure continue.

***

Ces études ont montré l'intérêt des dispositions adoptées la nécessité d'avoir un filtre incliné suffisamment haut, et ont servi (pression des pores) aux calculs de stabilité effectués notamment par Mécasol (MM. Florentin et L'Hériteau).

MM. N.Y. Denissov and G.A. Paooshkin (U.R.S.S.)

Application of Γ-location for examination of dynamics of flow-landslide

Among various stationary observations carried out at flow-landslide sites the examination of amount, direction and rate of stratum displacement, and the changing of these characteristics depending on the depth and time, is of great interest.

For settling this problem the Γ-location method can be used. The Γ-photons radiated by some source are known to be spreading in the vacuum rectilinearly in all directions from the source. If a detector (an impulse counter) is set at some distance from the source, the detector will count a certain number of Γ-photons that reach the device by the shortest distance.

If the isotropic sensibility on some part of counter's surface is disturbed, we shall see unequal intensity when rotating the counter. The change of the intensity occurs provided the part of the counter's surface with disturbed sensibility is turned towards the source.

Knowing the orientation of this part of the detector's surface we can determine the way of the Γ-rays to the source, i.e. we can find the direction in which the Γ-photon source is placed. Placing the source into some media we can examine motion of this media.

The isotropic sensibility of the counter can be disturbed if we surround the counter with a screen of a Y-ray absorbing material, and make an opening in the screen (see Fig. 14). We shall find the greatest number of impulses if the axis of the opening and the direction line to the source coincide.

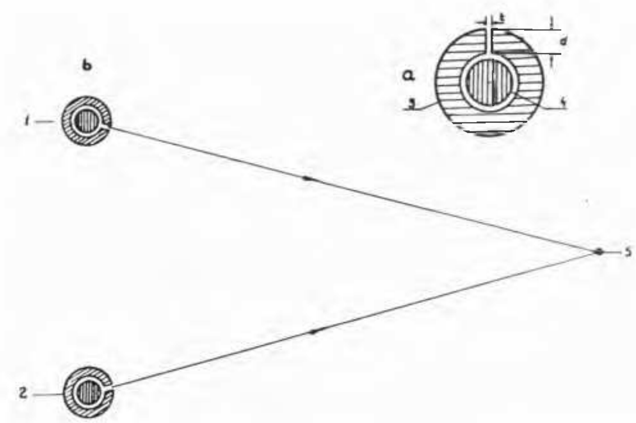

Fig. 14 The principal scheme of the Γ-location: 1. γ-radar (1st position); 2. γ-radar (2nd position); 3. screen; 4. detector; 5. source (radioactive tracer).

Relation between the intensity of the Γ-radiation $\mathscr{I}$ and the angle of view α is represented on Fig. 15 *a* (the angle of view is an angle between the geometrical axis of the opening and the direction line to the source).

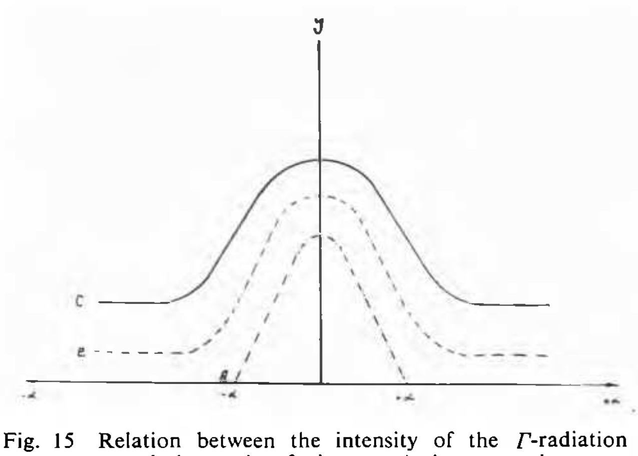

Fig. 15 Relation between the intensity of the Γ-radiation I and the angle of view α: *a*) the screen is not permeable for Γ-rays; *b*) the screen is permeable for Γ-rays; *c*) the source and the counter in a medium.

In general the nature of a curve depends upon the geometrical parameters of the screen and the opening. The ratio of the screen depth to the opening width should be as large as possible. With the increase of the ratio the steepness of the curve is increased hence this method becomes more exact, more accurate. The screen being somewhat permeable for the Γ-rays, the accuracy of the method is reduced to a certain extent. The curve $\mathscr{I} = f(\alpha)$ grows less steep and wider (Fig. 15 *b*).

It is also important to bear in mind that when the source is placed into the soil, some dissipation of the Γ-photons occurs as a result of their collisions with the electrons of soil particles. As a result of this the Γ-photon loses part of its energy and deviates from its original direction.

As a consequence a certain number of dissipated Y-photons will be noticed even in the case when the detector " does not see " the source. The curve $\mathscr{I} = f(\alpha)$ grows flatter and wider and the accuracy of the method is reduced.

In the measuring scheme an amplitude discriminator (a discriminating gear) is involved for the sake of the discriminating from all impulses penetrating into the detector the impulses of maximum amplitude that are of interest to us. In this case dissipated Γ-photons having smaller amplitudes are hardly registered.

Thus the essence of the proposed method is the following :

The sources of Γ-radiation Co^{60} are put into an uncased hole at regular intervals of 1.0 m. As each source is set in position the test hole is plugged. The remote control of the source location is exercised by a special device, the Γ-radar, at any period of time.

The Γ-radar is a hollow lead cylinder inside of which the Γ-photon counter (Geiger counter or photoelectronic multiplier) is placed. The lead screen has two narrow slots (one is horizontal and the other is vertical) placed within the receiving portion of the impulse register. Thus the Γ-radar will take the maximum number of impulses when the axis of the opening coincides with the direction line to this source.

To observe the location of the sources in a benchmark, two observation holes are made some meters away from it. The Γ-radars are put down into these holes by means of two rigid boring rods. Measuring the intensity along the hole depth, the source level can be obtained. The maximum Γ-radiation obtained corresponds to the level where the source is placed. By ranging the corresponding levels of the two holes one can determine the planned location of each source. See Fig. 14-b.

It is possible to carry on these observations for years without hard excavating work for digging out the benchmarks.

The application of the Γ-location is the only way to solve the problem of estimating the intensity of time stratum displacements that are taking place at different levels under the earth's surface.

The proposed method has been tested under laboratory and field conditions and now is being used by the landslide station for observation at landslide sites.

M. M. Doundoukov (U.R.S.S.)

The hydraulic fill method of dam construction is widely used in the U.S.S.R.

In many cases the fill was made on sites covered by water up to 30 meters deep. It was found that the density of soil in such dams is lower than when the fill is made above the water-level. The improvement may be achieved by the proper location of outlets of pipe lines and so on. In many cases values of dry density up to 1·55 tons per cubic meter were obtained. For higher values of density the vibration method or explosion was used.

Another feature of filling under water-level is that the slopes are usually flat; at depths up to 5 meters they are equal to 1·5, and at depths up to 15 meters 1·15.

For drainage purposes porous precast concrete blocks are used; they form a monolayer filter. The cost of such drainage is approximately three times lower than that of the method of filling filter layers.

Special full-scale investigations were conducted to find the best method of decreasing filtration losses in canals made in loess soils, which subside when wetted. The width of the canal at the bottom was 4 meters, and at the top 32 meters, while the depth of it was 4·5 meters. For this purpose the bottom and the slopes of the canal were compacted by means of 5 ton sheep feet rollers. The observations showed that the effect of the compaction reached a depth of up to 1 to 2 meters and seepage losses were decreased by up to 25 per cent.

M. O.K. Fröhlich (Autriche)

Discussion of the Paper 6/14

On page 598 (volume II) the author states with respect to Equation No. (24) " This equation was published by the author in 1950 in a slightly different form. It leads to a minimum value for the factor of safety ".

In the author's paper (see Reference No. 1, 1950, Vol. III, p. 363, of the Proceedings of the 5th Conf. 1961) the following equation of the factor of safety is computed :

$$\mathscr{F} = \left\{ \frac{2r\bar{c}\alpha}{R} + \frac{\cos\delta \cdot \sin\bar{\varphi}}{\cos\varepsilon} \cdot \frac{\cos\nu_0(\bar{\varphi} - \varepsilon)}{\cos\nu_0\alpha} \right\} \cdot \frac{r}{a} \quad \ldots . \quad (25)$$

The notation is the same as in the present paper 6/14.

$$2\,r\bar{c} = \frac{R_c}{\sin\alpha},\ 2\,r\bar{c}\alpha = \frac{\alpha}{\sin\alpha}R_c\,,\ \frac{\alpha}{\sin\alpha} = \frac{\rho_c}{r}$$

Therefore,

$$2\,r\bar{c}\alpha.\ \frac{r}{a} = R_c\,\frac{\alpha}{\sin\alpha}.\frac{r}{\alpha}$$

With the abbreviation of

$$\frac{\alpha}{\sin\alpha}.\ r = \rho_c \text{ and } \frac{R_c}{R} = \bar{\zeta}$$

we obtain

$$\frac{2\,r^2\bar{c}\alpha}{Ra} = \bar{\zeta}.\ \frac{\rho_c}{a}$$

If we put :

$$\frac{a}{\rho_c} = \mu$$

we obtain

$$\frac{2\,r^2\bar{c}\alpha}{Ra} = \frac{\bar{\zeta}}{\mu} \quad (1)$$

The auxiliary angle ε is determined by the relation :

$$\tan\varepsilon = \tan\delta - \frac{R_c}{R\cos\delta} = \tan\delta - \frac{\bar{\zeta}}{\cos\delta}$$

$$\tan\varepsilon = \frac{\sin\delta - \bar{\zeta}}{\cos\delta} \quad \ldots . \quad (2)$$

$$\cos\varepsilon = \frac{1}{\sqrt{1 + \tan^2\varepsilon}}$$

Therefore :

$$\frac{\sin\bar{\varphi}\cdot\cos\delta}{\cos\varepsilon} = \sin\bar{\varphi}\sqrt{1 + \bar{\zeta}^2 - 2\,\bar{\zeta}\sin\delta} \quad (3)$$

Finally we have, with the abbreviation

$$\frac{a}{\rho_{\varphi}} = \nu :$$

$$\frac{r}{a} \frac{\cos \nu_0 (\bar{\varphi} - \varepsilon)}{\cos \nu_0 \alpha} = \frac{\rho_{\bar{\varphi}}}{a} = \frac{1}{\nu} \qquad \ldots . \quad (4)$$

By inserting Eq. 1), 3) and 4) into the old Equation (25) above, we obtain :

$$\mathscr{F} = \frac{\bar{\zeta}}{\mu} + \frac{\sin \bar{\varphi}}{\nu} \sqrt{1 + \bar{\zeta}^2 - 2 \zeta \sin \delta} \qquad (5)$$

This Equation (5) is exactly the dimensionless Equation (24) in Paper 6/14, Vol. II, p. 598. It was computed before 1950 by using a driving moment M_{add} as " impulse ".

The same equation can also be produced in a twofold way :

(*a*) by using a simple load P_{add} normal to $\overline{OS}$ as impulse for mobilizing the parameters $\bar{c}$ and $\bar{\varphi}$, if its point of application S is moved from $\rho_s = \rho_{\bar{\varphi}} \sin \bar{\varphi}$ to infinity $\rho_s = \infty$ along the line of action of Z ;

(*b*) by using a fixed point S of application, identical with the point of intersection of R with Z and varying the direction of P_{add} from the direction of Z until the direction normal to the middle line between Z and Z'.

In both cases (*a*) and (*b*) the point S of application of P_{add} always lies at the line of action of Z. In addition to that, the magnitude of the resulting factor of safety lies in both cases (*a*) and (*b*) between an infinitely great and a minimum value. The latter is identical with the result of applying a driving moment M_{add} for mobilizing Coulomb's parameters $\bar{c}$ and $\bar{\varphi}$.

From the above stated facts it may be very easily concluded which procedure of determining the factor of safety may be recommended for practical applications.

It is generally known that the only correct critical slip circle has to be determined according to the " *Minimum-Principle* ". The same applies to the choice of the only correct procedure out of the existing infinite number of procedures.

Notice : Equ (24) of Paper 6/14 remains valid for influences of hydrostatic uplift, earthquake action, hydrostatic excess pore pressures, seepage water pressure etc., all of which change only *the size and the position* of the resultant R and its inclination to the axis of symmetry, that is the *angle* δ.

MM. O.K. Fröhlich (Autriche), E.E. de Beer et E. Lousberg (Belgique).

Deux communications faites au Congrès, l'une du Prof. Fröhlich (6/14), l'autre du Prof. de Beer et de l'Ir. Lousberg (6/6), ont eu pour objet de définir la notion de coefficients de sécurité à la « rupture » pour mesurer la stabilité au glissement d'un talus. Cette méthode, présentée, pour la première fois en 1950 par le Prof. Fröhlich [1], a été développée de manière quelque peu différente dans les deux contributions. Au cours du Congrès, les auteurs se sont rencontrés et ont décidé, eu égard à l'importance qu'ils attachent à ce problème, de procéder à une confrontation des préoccupations qui les ont menés à la notion de ces coefficients. De cette confrontation ils espèrent dégager une méthode commune pour la définition des coefficients de sécurité à introduire dans le calcul de vérification de la stabilité des talus.

Référence :

[1] O. K. Fröhlich : « Sicherheit gegen Rutschung einer Erdmasse auf kreiszylindrischer Gleitfläche mit Berücksichtigung der Spannungsverteilung in deiser Fläche ». *Beiträge zur angewandten Mechanick* (Federhofer-Girkmann-Festschrift — Wien, F. Deuticke 1950).

M. J.A. Leadabrand (Etats-Unis)

Bonny Dam Experimental Project

The Bureau of Reclamation began in 1951 investigating the possibility of using soil-cement in place of riprap. Their studies culminated in construction of an experimental project located in eastern Colorado on the reservoir just above Bonny Dam. This site was selected to give exposure to severe wave action and much freezing and thawing.

Design

A 22·5-ft. high embankment of impervious material was constructed in 6-in. compacted layers using procedures similar to those for earth dam construction. The 2:1 slope was then protected by construction of a 345-ft. length facing of soil-cement. The facing was built 7 ft. wide and composed of 6-in. horizontal layers of the soil-cement. Each superimposed layer was set back from the previous layer to give a stair-step effect as shown in Fig. 16. The finished soil-cement facing provided a thickness of 2·7 ft. perpendicular to the 2:1 slope.

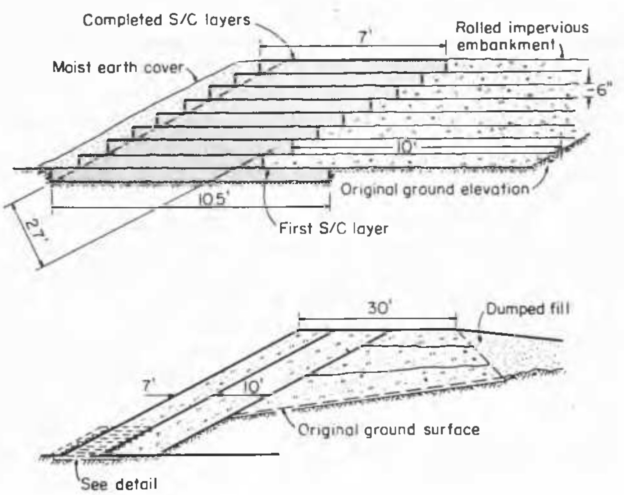

Fig. 16 Details of compacted soil-cement facing for Bonny Dam test section.

The two soils used were obtained from deposits near the embankment. One soil was classified fine sandy loam by the U.S. Department of Agriculture system and as A-4 [4] under the American Association of State Highway Officials' system. By the Unified System the soil is classified S.M. Similarly, Soil No. 2 is a coarse sand of A-3 [0] classification. Under the Unified System it is S.M.

Stability tests were conducted. Wet-dry and freeze-thaw tests were made and cement factors of 10 per cent and 12 per cent by volume respectively were specified.

Construction

The earth embankment and soil-cement facing were constructed together. First a layer of embankment soil was placed. Then on the outer 7-ft. width the proper amount of loose soil for a 6-in. layer of the soil-cement facing was placed. Bags of cement were spotted. A rotary mixing machine then mixed the soil and cement in place ; water was added and mixing completed. Compaction was obtained with tamping rollers and rubber-tired rollers. Before placement of the next layer the compacted soil-cement was lightly scarified to assure bonding.

Fig. 17. View of Bonny Dam soil-cement facing after 10 years of service.

Throughout the ten years of service the soil-cement facing has performed quite satisfactorily, see Fig. 17. Compressive tests of cores lifted from the soil-cement facing after 10 years of service averaged approximately 2,500 lb. per sq.in. This appears most significant considering that the facing is exposed to severe wave action and even significant ice-block action.

Merritt Dam

Because of the success of the experimental soil-cement facing at Bonny Dam, the Bureau of Reclamation in 1961 awarded the first contract for full-scale soil-cement facing on the approximately 3,500-ft. long Merritt Dam in north central Nebraska. The 2-ft. thick facing will be constructed in horizontal layers on a 4:1 slope, similar to the Bonny Dam construction. The soil-cement will be plant mixed.

Alternate bids were received for both soil-cement facing and riprap. The soil-cement facing cost about $5,50 per sq.yd. and the cost of the riprap approximately twice as much.

Conclusion

The Bonny Dam experimental section built by the Bureau of Reclamation has proved that a soil-cement facing provides permanent and satisfactory protection to the earth embankment. The Merritt Dam bid prices for soil-cement facing show that considerable savings in cost over rock facing may be realized for earth dams in locations where satisfactory rock is expensive or unavailable.

Références :

[1] Floyd A. Baker, « First Soil-cement Laid in Colorado ». *Western Construction*, August, 1951.

[2] « Riprap Substitute Tested in the Great Plains Area ». *Engineering News-Record*, March 29, 1951, Vol. 146, No. 13, p. 72.

[3] « USBR Seeks to Avoid Cost of Importing Riprap, Studies Other Slope Protection ». *Engineering News-Record*, April 26, 1951, Vol. 146, No. 17, p. 48.

[4] « USBR Tests Soil-Cement Facing for Dam Protection ». *Soil-Cement News*, No. 37, August, 1951, p. 1.

[5] « Laboratory Studies of Soil-Cement Materials for Test Installation of Riprap Substitute », Specifications No. 3227, Bonny Dam — St-Francis Unit — Upper Republican Division — Missouri River Basin Project — *Earth Materials Laboratory Report EM*-250.

M. J. G. Lewis (Australie)

I would like to elaborate slightly on the brief remarks I was able to make at the Conference as follows.

Two of the dams referred to are Samson's Brook and Stirling Dams. These were designed by empirical methods based on previous experience in the years 1937-1940. Construction of Samson's Brook Dam was completed in 1940 and Stirling Dam in 1946. Since that time each dam has been subjected to almost complete seasonal drawdown every year.

This would cause a reversal of loading which some authorities are suggesting would bring about a successive increase in moisture content and reduction in cohesion tending towards zero in the limit. Quite recently boreholes have been drilled

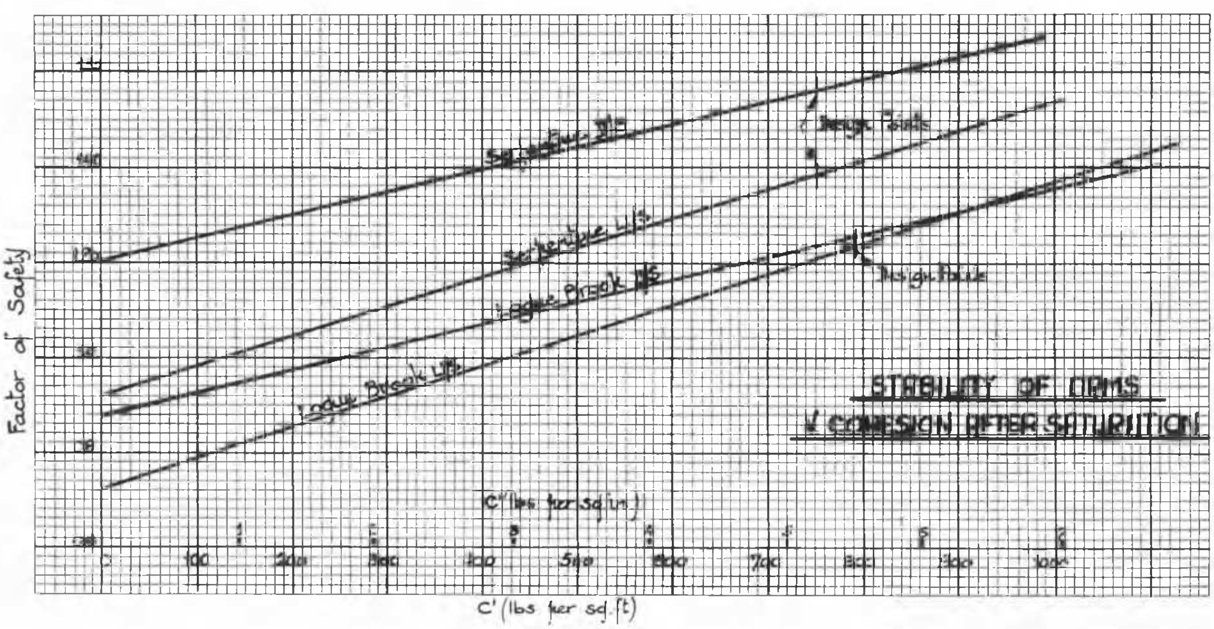

Fig. 18

in these dams and undisturbed samples taken. Although some soft patches were found, the general cohesive strength was very appreciable.

More recently another dam, Serpentine Dam, has been completed and Logue Brook Dam is under construction. Both of these dams have been examined exhaustively by laboratory tests and the most recent methods of stability analysis applied. It became evident that apart from better positioning of the drainage and filter systems, the profiles showed only minor economy over those determined earlier from experience alone.

Fig. 18 shows the effect of cohesion after saturation on the stability of both slopes of these two dams. If the method of slices without inclusion of the forces between the slices, is accepted as the criterion of safety, it is clear that these dams rely on cohesion for their safety. Similarly the older dams, Samson Brook and Stirling would show similar characteristics being of similar profile and built of similar materials.

To date, there has been no sign of excessive movement at any point in these dams, which are all equipped with a grid of surface measuring points. This is taken as part evidence that cohesion plays a considerable part in the stability of dams built of compacted clay soils originating from the decomposition crystalline rocks *in situ*.

A feature to be remembered is that the interpretation of cohesion in laboratory tests is at zero normal effective stress whereas the slip circle for a medium sized dam traverses a zone where the effective stresses due to overburden are important and would tend to limit the loss of cohesion due to swelling under cyclic reversal of loading.

It is admitted, of course, that these conclusions are clouded by other uncertainties such as the effect of thixotropy, possible inaccuracy of the triaxial test, conservative estimation of safety by the slices method of analysis and field compaction standards. However, the evidence of practical experience cannot be overlooked and indeed if cohesion is taken as zero for these materials, the profiles designed by current scientific methods will be much more conservative than those proved in practice.

M. A. MAYER (France)

Au sujet des conditions d'application de l'égalité $c' = 0$.

La très brillante intervention du Prof. Skempton au sujet des conditions d'annulation de la cohésion effective m'a fait le plus grand plaisir et je ne puis m'empêcher d'en souligner les points fondamentaux. Ce sont : d'abord que la cohésion effective ne s'annule pas avec le temps pour toutes les argiles et dans toutes les circonstances. Cette baisse de la cohésion effective implique un matériau fissuré, très sensible à l'action de l'eau sous l'effet des concentrations de contraintes qui se produisent dans les zones de cisaillement maximum. Enfin la transformation du matériau n'a lieu que sur une faible épaisseur le long de la surface de cisaillement, c'est-à-dire dans la zone des contraintes maxima. Le Prof. Skempton signale avoir vu également, après deux glissements au moins dans l'argile, la surface de glissement constituée sur une faible épaisseur par un matériau extrêmement humide, plastique, à cohésion manifestement nulle.

J'ai moi-même fait cette constatation tout au début de ma carrière de Mécanicien des Sols, lorsque je fus appelé sur un barrage en terre qui venait d'être terminé. L'ouvrage était un barrage homogène en argile corroyée, compactée comme on le faisait à l'époque, simplement par la circulation des engins qui apportaient les matériaux. C'est dire que les blocs d'argile extraits d'une couche relativement homogène, se retrouvaient en place avec les fissures provenant de l'action de la pelle au moment de l'extraction.

Lorsqu'on me demanda de venir sur les lieux, la face amont, que l'on venait de dresser, formait un ventre sans qu'aucun glissement se soit encore produit. Je fis aussitôt fixer la face amont en bloquant l'intervalle entre le barrage et le batardeau et creuser un puits dans le corroi de manière à atteindre la zone correspondant au cisaillement maximum dans une masse homogène et pesante. Un peu plus haut que le niveau auquel nous l'attendions, nous sommes tombés sur une bande de quelques centimètres d'épaisseur, remplie de matériaux remaniés et sans cohésion. Visiblement le glissement était en cours de formation, mais nous avions assisté au phénomène d'annulation de la cohésion dans un plan d'une digue homogène. Nous avons pu heureusement bloquer la face amont de la digue par son pied sur plusieurs mètres de hauteur; elle tient maintenant depuis plus de 25 ans et on a même pu la surélever au cours des dernières années. Bien entendu, il n'a pas été possible, à l'époque, de faire une étude de la stabilité aussi complète qu'on l'aurait fait maintenant. Mais il nous avait paru certain que la zone où s'était produit la rupture correspondait au maximum des contraintes de cisaillement dans la masse, que le matériau s'était localement cisaillé, formant des passages pour l'eau incluse qui, sous l'effet des charges, avait trouvé un chemin vers la zone fissurée et avait désorganisé l'argile dans cette zone. L'hypothèse d'une fissuration préalable, que suggère le Prof. Skempton, ne peut qu'avoir facilité la formation d'une pareille surface.

Faut-il en déduire, comme certains ingénieurs seraient tentés de la faire, qu'il faut dans tous les cas, pour être du côté de la sécurité, prendre $c' = 0$? Je suis tout à fait d'accord avec le Prof. Skempton pour ne pas le croire. Mieux vaut, surtout dans la construction de barrages en terre, choisir les matériaux de telle manière qu'un pareil accident ne soit pas à craindre. Ceci conduit à préférer, même pour les noyaux étanches des barrages, des matériaux de perméabilité relativement basse, 10^{-5} ou 10^{-6} cm/sec. qui assurent une dissipation rapide de la pression interstitielle, aux argiles compactes de perméabilité égale à 10^{-10} ou davantage. Dans le cas de barrages homogènes, ces matériaux sont moins sensibles aux effets d'une baisse rapide du plan d'eau dans la retenue. D'autre part les matériaux correspondants, plus sableux, moins sensibles aux variations de teneur en eau et aux souspressions, conservent une fraction importante de leur résistance, même après cisaillement.

Je donnerai deux exemples où le choix de l'ingénieur s'est exercé dans ce sens. D'abord le barrage de Serre-Ponçon où les projeteurs avaient le choix entre un important dépôt d'argile fine, très étanche, et des produits de décomposition des schistes, charriés par les torrents jusqu'à proximité de l'ouvrage. On préféra ces derniers, malgré la valeur relativement élevée de leur perméabilité (de l'ordre de 10^{-5} cm/sec.) parce qu'ils assuraient une dissipation très rapide de la pression intertitielle.

Je citerai également le cas de trois digues de 15 à 30 m. de hauteur prévues pour l'irrigation d'une plaine basse et où l'on disposait de deux types de matériaux, une terrasse quaternaire à granulométrie continue, mais très argileuse et de perméabilité de l'ordre de 10^{-10} cm/sec et un sable limoneux miocène, de perméabilité comprise entre 10^{-5} et 10^{-6} cm/sec. Le choix de l'ingénieur s'arrêta sur ce dernier, qui devait supporter plus facilement les vidanges annuelles correspondant aux périodes d'irrigation.

Dans l'un comme dans l'autre cas la résistance au cisaillement tient non seulement à la cohésion, mais au frottement intergranulaire. Même dans l'hypothèse du $c' = 0$, à laquelle je ne crois absolument pas dans le cas particulier, la résistance de l'ouvrage serait assurée.

M. N. V. Ornatsky (U.R.S.S.)

En ce qui concerne la question de la stabilité des talus en fonction du temps, je voudrais communiquer quelques résultats expérimentaux obtenus au Laboratoire de Mécanique des Sols de l'Université de Moscou.

Ces résultats sont relatifs à la charge de rupture des argiles denses, saturées, et ont été obtenus en reproduisant en laboratoire les conditions d'équilibre des talus en carrières profondes, dans lesquelles la pression est très forte.

L'étude d'échantillons d'argile, contenant environ 66 % d'éléments inférieurs à 0,001 millimètres avec un indice de plasticité voisin de 38, prélevée à 50 mètres de profondeur, était faite dans un oedomètre rectangulaire spécial, dont l'un des côtés pouvait être enlevé pour réaliser la pression convenable. On a pris des mesures pour que l'argile ne sèche pas pendant tout le temps d'essai.

Les échantillons étaient détruits quand on appliquait tout d'un coup une charge d'environ 7 kg/cm^2. Cette charge correspond à peu près à la pression normale engendrée par le recouvrement. La destruction correspondait à l'hypothèse du corps fragile; la surface plane de glissement formait un angle d'environ 45° avec la direction de la force de compression. Lorsque les échantillons étaient soumis à une charge un peu inférieure à la normale, (6,6 kg/cm^2), et qu'ultérieurement on augmentait la charge par paliers de longue durée (de 30 à 240 heures) jusqu'à cessation du tassement, une consolidation lente de l'argile se produisait dans la direction verticale sans déformation latérale importante. Pendant ce temps la teneur en eau diminuait un peu et simultanément la résistance de l'échantillon s'élevait.

Quand on augmentait la durée de l'essai de 184 à 485 heures avec conservation de l'échelonnement des charges (6,6; 8,0; 9,3; 10,8; 16,0; 18,6; 21,3 kg/cm^2) la charge de rupture augmentait de 10,8 à 21,3 kg/cm^2, c'est-à-dire presque de 100 %. Parallèlement le chargement prématuré du modèle à la charge 6,6 kg/cm^2 pendant 260 heures n'a pas donné d'augmentation considérable de la charge de rupture.

Ces chiffres indiquent la grande influence de la durée de chargement et du régime de son accroissement pour la stabilité des hauts talus.

On peut en tirer la conclusion qu'il est nécessaire d'éviter l'exécution des tranchées ouvertes d'un seul coup à une grande profondeur. Il est préférable d'organiser les travaux largement mais en gradins relativement petits (pas plus de 3-4 mètres). On laisse reposer chaque gradin le plus longtemps possible avant de recommencer des travaux dans cette tranchée.